Bay 135

Digitized by the Internet Archive
in 2021 with funding from
Wellcome Library

https://archive.org/details/s3748id1397101

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

VOL. XIII.

(THIRD SERIES, VOL. IV., JULY TO DECEMBER 1851.)

WITH THREE LITHOGRAPHS, AND NUMEROUS WOODCUTS.

EDINBURGH: SUTHERLAND AND KNOX.
LONDON: SIMPKIN, MARSHALL, AND CO.

MDCCCLI.

95400

WELLCOME LIBRARY INSTITUTE

WELLCOME INSTITUTE LIBRARY

Coll.	WellMOmec
Coll. No.	

MURRAY AND GIBB, PRINTERS, EDINBURGH.

ALPHABETICAL LIST

OF

CONTRIBUTORS OF ORIGINAL ARTICLES.

Ballingall, G. R., M.D., H.E.I. Co.'s Service.

Bell, Benjamin, Esq., F.R.C.S.

Bennett, John Hughes, M.D., F.R.S.E., F.R.C.P.E., Professor of the Institutes of Medicine and Clinical Medicine in the University of Edinburgh.

Bookless, John, Esq., Surgeon, Kelso.

Bramwell, James P., M.D., Perth.

Christison, Alexander, M.D., Annual President of the Medical Society of Edinburgh.

Christison, Robert, M.D., V.P.R.S.E., Professor of Materia Medica and Clinical Medicine in the University of Edinburgh.

Crawford, Robert, M.D., Ratho.

Easton, J. A., M.D., Medical Officer to the Glasgow Police, one of the Physicians to the Glasgow Royal Infirmary, &c.

Gairdner, W. T., M.D., F.R.C.P.E., Pathologist to the Royal Infirmary, Edinburgh.

Harvey, Alexander, M.D., Physician to the Aberdeen Royal Infirmary, and Lecturer on the Practice of Medicine in the University and King's College of Aberdeen.

Husband, William, M.D. and M.A., Edinburgh.

Imlach, Dr, Liverpool.

Kerr, William, Esq., Surgeon, Corresponding Member of the Medical and Physical Society of Calcutta, and of the Medico-Chirurgical Society of Glasgow.

Mackenzie, Richard J., M.D., F.R.C.S.E., Junior Ordinary Surgeon to the Royal Infirmary, Edinburgh.

MACLAGAN, DOUGLAS, M.D., F.R.S.E., Fellow of the Royal College of Surgeons, Edinburgh.

MACLAGAN, J. M'GREGOR, M.D., Extraordinary Member of the Royal Medical Society, Edinburgh.

MILLER, HUGH, M.D., Bombay.

REDFERN, P., M.D., F.R.C.S.L., Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

SANDERSON, JOHN SCOTT, M.D., Vice-President of the Physiological Society of Edinburgh.

SMITH, ALEXANDER, M.D., Edin., Forfar.

STEWART, ALEXANDER P., M.D., Assistant Physician of the Middlesex Hospital.

SYME, JAMES, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.

THOMSON, F., M.D., Perth, Visiting Surgeon to the City and County Infirmary.

THOMSON, THOMAS R. HEYWOOD, M.D., Surgeon, R.N.

TRAILL, JOHN, M.D., Surgeon to the Arbroath Infirmary.

TRAILL, THOMAS STEWART, M.D., Professor of Medical Jurisprudence in the University of Edinburgh.

THE

MONTHLY JOURNAL

OF

MEDICAL SCIENCE.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Poisoning by Scheele's Green.* By THOMAS STEWART TRAILL, M.D., Professor of Medical Jurisprudence in the University of Edinburgh.

IT is well known that various substances capable of acting as poisons are in common use as pigments, and especially that a very beautiful green is obtained from the arsenite of copper, which is commonly known as *Scheele's Green.* An instance of the deleterious effects of this substance occurred here on the 31st of May.

George Webster Graham, a child of three years of age, had obtained some slips of paper coloured with this material, and had chewed some of it. He returned home, complaining of severe pain over his abdomen, and vomited repeatedly. His mother observed some shreds of the paper in his hands, and saw some of the green matter on his lips and cheeks. The vomiting returned at intervals with much severity, the pain was violent, and I was sent for; but I did not return home for three hours after he was taken ill. In the meantime, his pulse became feeble, his extremities cold, and face livid. I went immediately on coming home, and found that the vomiting had somewhat abated, and that the skin was recovering its heat; but the pain in the bowels was still acute. On hearing the symptoms, I inquired if the child had swallowed anything unusual, heard the story, and on seeing the piece of paper, immediately concluded that it was coloured by arsenite of copper, and had caused the child's illness.

I instantly gave the only remedies at hand,—magnesia in milk, with the whites of two eggs diffused in water,—until I could procure the more powerful antidote, the *hydrated peroxide of iron.* I also directed small portions of milk porridge to be repeatedly given. All the symptoms were so relieved before the peroxide was procured, that I judged it unnecessary to administer that antidote; and next morning the boy was quite convalescent.

On the third day the child passed by stool one or two fragments of the paper, on which some of the green colour was still visible; and for a few days more he did not take his food with his usual relish; but no other bad consequence has followed.

I preserved the shreds of paper which the child had carried home, scraped off a portion of the green colour, mixed it with a little soda flux, and subjected it to heat in a tube; when I obtained a distinct metallic crust, with a crystalline inner surface, an alliaceous smell, and resolvable, by heat and air, into white crystals, with triangular facets, proving the colouring matter to contain arsenic, of which the quantity appears much more considerable than I expected. On comparing the pigment on the paper with Syme's Wernerian colours, it answers to pale *emerald green* (No. 52).

It seems improper to employ so dangerous a material as *arsenite of copper*, in such large quantity, on articles like paper-hangings, which may often come in the way of children, or other persons little aware of the deleterious properties of this colouring matter.

JUNE 14, 1851.

ARTICLE II.—*On the Pathological States of the Lung connected with Bronchitis and Bronchial Obstruction.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.—(*Continued from Vol. XII., p.* 453.)

PART II.—PERMANENT LESIONS OF THE AIR-VESICLES AND BRONCHI DEPENDING ON BRONCHITIS.

Relation of Bronchitic Collapse to Pulmonary Emphysema.—In the last paper I discussed the mode in which the collapsed lung, under favourable circumstances, reverts to its natural condition. The mechanism by which this is effected, and particularly that portion of it which I have theoretically suggested under the name of the deobstruent function of the bronchial tubes, must be considered as of vast physiological interest, if we reflect that there is scarcely a case of fever, or any other debilitating disease, in which the signs of mucous accumulation and of partial pulmonary collapse may not be discovered at one period or other at the lower and back part of the lungs; and that the same forces which under these circumstances restore the lung to its normal state, by throwing off the load in the bronchi, are probably perpetually in action to prevent a similar

accumulation in the state of health. A little careful reflection on the mode in which the free and unembarrassed play of the lungs is maintained in health, notwithstanding the constant presence of a viscid secretion from the bronchial mucous membrane, will probably satisfy every one that a special function for the removal of this secretion must form an element of the highest importance in normal as well as morbid respiration; and the ciliary apparatus, as I have already mentioned, is not calculated fully to perform this office, being less abundantly distributed to the smaller bronchial tubes, where its presence is apparently most required.

In many persons the removal of the bronchial mucus is habitually ill-performed. The quantity of mucus is not materially greater than natural, but it is not discharged as rapidly as it is secreted, owing to some defective condition of the deobstruent apparatus, or perhaps a defective innervation of the bronchial muscles. In this condition, whatever be its cause, the tranquil and insensible processes of the normal economy are exchanged, sometimes for fits of coughing, which bring up pellets of tenacious pearly mucus, the result of undue accumulation; and sometimes for paroxysms of dyspnœa, which end in more copious evacuations of bronchial mucus, and a return to comparative health. These persons are the subjects of the *dry* or *humid asthma* of English authors, and of the *catarrh sec* of Laennec. Their disease, though simple and free from danger in its outset, is, according to the judgment of all physicians, apt to lay the foundation of organic disorder, which usually assumes the form of pulmonary emphysema. This consequence is peculiarly apt to occur, if care be not taken to guard against the supervention of bronchitis, which in these individuals generally assumes characters of great intensity, and is uncertain and protracted in its cure.

There are other persons who, with a habitually normal state of the respiratory functions, are subject to repeated acute bronchitic attacks of great severity. They are careless in their mode of life, frequently exposed to cold, or endowed with a peculiar sensitiveness of the pulmonary mucous membrane, while they are at the same time free from the tuberculous constitution, with its organic sequelæ. In such persons, also, pulmonary emphysema is known to be a frequent disease; being left behind as the legacy of the bronchitic attacks, and aggravated after each successive invasion.

Not unfrequently a considerable, or even an extreme, amount of pulmonary emphysema is observed to follow a single attack of acute disease in the chest. Thus emphysema frequently arises in the earliest years of infancy and childhood, as the consequence of some form of severe infantile bronchitis; and all practitioners can bear witness to many cases in which shortness of breath and incapacity for exertion can be traced distinctly back to the date of an attack of hooping-cough or measles. Some of the most marked instances of emphysematous lungs in young subjects that have fallen under my notice in dissection, have had a similar history; and all authors

on the diseases of children, who have carefully investigated the morbid anatomy and history of these affections, concur on this point. Again, in adults otherwise healthy, the severer forms of epidemic influenza are peculiarly apt to be attended with, or followed by, the development of emphysematous lesions; a fact which has been well observed and carefully recorded by Dr Peacock, in his excellent history of the last London epidemic of that disease.[1]

Finally, a certain amount of emphysema of the lungs is of so frequent occurrence in the aged, as to be scarcely entitled to the name of a disease, distinct from the other evidences of corporeal decay. This fact was first pointed out by Magendie, and the form of emphysema here alluded to has since been described by many pathologists as a peculiar one, constituting a kind of senile atrophy of the pulmonary tissue. But there can be little doubt, that here also the pulmonary lesion is the concomitant of a bronchial affection,—the chronic bronchitis or bronchorrhœa,—which is the almost constant companion of the more advanced periods of human life. In cases where this has been absent, I have repeatedly found the lungs of very aged individuals quite free from all trace of emphysematous lesion.

Considerations like these have, ever since the accurate descriptions of emphysema by Laennec became generally known, given rise to a general belief among practitioners that emphysema is related to bronchitis as effect to cause; and that it is indeed the organic lesion of the lung of all others most closely and invariably connected with long-continued or severe bronchial affections. In taking it, therefore, as the starting-point of the following researches on the permanent lesions of the lungs connected with bronchitis, I shall have the advantage, not only of beginning with a disorganisation so palpable and well-known as to be rarely overlooked at the present day by any one acquainted with pulmonary pathology, but one, the relation of which in some way or other to bronchitis, is almost universally admitted, notwithstanding the numerous differences of opinion as to its mechanism and causation. The observation of it somewhat more rigorously will serve, therefore, as a criterion of the correctness of the observations in the first part of this memoir, and at the same time will lead naturally to the consideration of other subjects.

---

[1] The Influenza or Epidemic Catarrhal Fever of 1847-8. By Thomas Bevill Peacock, M.D., &c.: London, 1848. See pp. 31-32, 134-135, 143-144, for graphic descriptions of the morbid appearances after death from influenza. Dr Peacock has favoured me with a letter on this subject since the publication of the first part of this memoir, and I am happy to be able to state, that this experienced pathologist—my predecessor in my present office—is convinced of the correctness of my explanations of the bronchitic collapse of the lung, and satisfied of the identity of that affection with many of those indicated by him in the pages to which I have referred.

Analysis of Cases of Pulmonary Emphysema.—The last number of this series of papers contained a table of forty cases of emphysema of the lungs (May Number, pp. 448-453), the object of which is chiefly to show the connection of that lesion with other collateral affections of the pulmonary tissue. The state of the bronchi is not always noted, nor indeed can it be fairly assumed that, in a chronic lesion such as emphysema, the condition of the bronchial mucous membrane at the period of death has any direct relation to it in the majority of cases. The statements in the table accordingly show, not the evanescent and inappreciable conditions of the bronchial membrane, but the more permanent and evident affections of the air-vesicles and pulmonary tissue. The other columns are added for the satisfaction of those who may be studying the same subject under different aspects, and references are given in all the cases to the Registers of Dissections in the Royal Infirmary, where more detailed reports of them may be found. It is right also to state, that no cases of considerable emphysema have been excluded from the table, excepting a few, in which the report was, from one cause or other, considered to be inadequate or untrustworthy.

The most cursory inspection of this table will show that pulmonary emphysema is in by far the greater number of instances accompanied by other lesions of the air-vesicles and pulmonary tissue; and that, in fact, its occurrence as an isolated affection of the lung, is not only uncommon but doubtful. In every instance it was found connected with some mode or form of condensation of the pulmonary tissue, except in the two cases marked XII. 32, and XII. 323. In one of these the report was not drawn up by me; and in neither of them do I now feel certain of its accuracy in this respect, as some of the lesions which I shall have to describe in the sequel as concurring with emphysema are easily overlooked, and have, in fact, been constantly overlooked by Laennec and other writers on this subject.[1]

The appearances in the other cases may be arranged as follows:—

Hepatization in four cases, or 10 *per cent.*, viz., XII. 67, 345; XIII. 1, 74.

Tubercle (or tubercle with condensation) in eight cases, or 20 *per cent.*, viz., *tubercle without excavation*, XII. 25, 220, 349, 360; *with excavation*, XII. 36, 108, 169; XIII. 81.

Condensation (presumably *bronchitic collapse*, and often described as such) in 27 cases, or 67·5 *per cent.*, viz., XII. 10, 25?, 67, 222, 272, 275, 288, 317, 326, 327, 345, 350, 355, 360, 364, 390; XIII. 1, 9, 10, 18, 27, 30, 34, 49, 74, 81, 84.

---

[1] In Louis's essay on Emphysema, in the Mem. de la Societé d'Observation, he describes at considerable length numerous cases of this lesion affecting the *entire* lung, without any concurrent affection,—a condition which, I do not hesitate to say, is not found in nature.

Bronchial abscesses, or non-tubercular ulcerations, in seven cases, or 17·5 *per cent.*, viz., XII. 47, 272, 326, 327, 350, 355; XIII. 86.

Chronic induration or atrophy, in ten cases, or 25 *per cent.*, viz., XII. 47, 108, 345, 352, 400; XIII. 30, 34, 49, 86, 88. Besides some of the tubercular cases, and the following:—

Contraction of opposite lung (absorbed pleuritic effusion) in one case, XII. 349.

Concretions in eight cases, or 20 *per cent.*, viz., XII. 10, 274, 352; XIII. 1, 34, 49, 81, 86.

Even a superficial inspection of this catalogue of morbid appearances will serve to corroborate many of the views previously expressed, and will conduct us to conclusions simplifying very much the whole subject. We find, in the first place, that 67·5 per cent. of the whole cases of emphysema were connected, at the time of death, with those forms of pulmonary condensation which have been ascertained in the preceding pages to be most frequently the result of bronchitis, and which, indeed, concur with it in the great majority of cases. The per-centage of bronchitic lesions, however, as we shall see in the sequel, will be increased from those found under other heads. Contrast with this the proportion of cases referred to hepatization or tubercle, two of the commonest of pulmonary lesions, the former of which is, nevertheless, found only in 10, the latter only in 20, per cent. of the emphysematous cases; and it will be at once evident, to what a large extent the bronchitic lesions predominate over all others. Nor is this apparent predominance merely the result of their greater absolute frequency; for I find, by my manuscript returns of the results of 502 post-mortem examinations of all kinds of disease performed in the hospital, during the periods referred to in the table, that, among these mixed cases, hepatization occurred 48 times, or 9·8 per cent., and tubercle of the lung 100 times, or 20 per cent.; while the other forms of condensation alluded to occupy a medium position in frequency, viz., 59 times, or 11·8 per cent., being only a little more frequent than hepatization, and very considerably less so than tubercle.[1]

---

[1] As all the circumstances tending to affect the accuracy of these returns ought to be mentioned, so far as known to me, I may here indicate that I believe the frequency of these bronchitic lesions to be understated in both classes of cases, from the slighter forms being not always recognised or recorded, especially before I became quite familiar with their character and significance. These omissions would affect both classes of cases quite equally, and therefore be of little moment, were it not that the distinction between hepatization and collapse of the lung is not always sharply drawn in the earlier cases; and I think it possible that, especially in the larger series of returns, a few cases of the latter may have found their way under the head of the former. Even if we suppose, however, the per-centage of hepatization *slightly diminished*, and that of collapse of the lung *slightly increased*, in the series of mixed cases, the difference between the two affections in regard to the production of emphysema, will remain too broad and well-marked to be explained by any accident.

I am aware it will be said, that tubercle stands in a wholly peculiar relation to this subject, as several pathologists of great eminence have maintained the doctrine of the incompatibility of emphysema and tubercle; supposing, on the ground of their comparatively rare co-existence, that the former confers on those attacked an immunity from the latter affection. Rokitansky has, indeed, given the sanction of his high authority and immense experience to a doctrine which may be considered inclusive of this assertion of the French pathologists; viz., that all affections producing *venosity*, or imperfect oxygenation of the blood, such as cyanosis, curvature of the dorsal spine, emphysema, &c., confer an immunity from tubercle. Without entering here on the discussion of this doctrine, in its more general relations, it may be confidently stated, that the portion of it relating to emphysema gains no support from the numbers just quoted. According to this doctrine, it might reasonably be expected, that among persons dying with emphysematous lungs, tubercle would bear a decidedly lower proportion to the whole numbers than in a mixed hospital mortality; whereas, by a remarkable enough accident, it happens that the proportion is, in the above numbers, precisely the same, viz., 20 per cent. in both classes of cases.[1] And although it would be too much to argue from this coincidence, that emphysema and tubercle exert no influence upon each other, yet I think it may justly make us pause before accepting a doctrine which has not, *a priori*, much argument in its favour, and the evidence of which has never been presented to the public under a form approaching to exactness. I shall have occasion hereafter to state my own views on this subject.

The following table exhibits, in one view, the per-centage of most of the lesions referred to above, in emphysematous and in mixed cases of disease,—the numbers from which it is calculated being derived from the same hospital returns, so as to assimilate the conditions of observation as nearly as possible.

	In mixed Cases.	In Emphysematous Cases.
Hepatization,	9·8 per cent.	10·0 per cent.
Tubercle,	20·0 —	20·0 —
Condensation (collapse),	11·8 —	67·6 —
Bronchial abcesses,	5·5 —	17·5 —
Induration and atrophy,	7·5 —	25·0 —
Concretions,	4·1 —	20·0 —

It will be seen that while the first two lesions in the preceding table appear to have no special numerical relation whatever to

[1] Rokitansky admits the conjunction of obsolete or cretaceous tubercle with emphysema. But, in the cases above referred to, all the instances of obsolete tubercle have been excluded from both lists. In the cases conjoined with emphysema, it will be seen that there existed cavities in four cases; the others were miliary or yellow tubercle without excavation.

emphysema, their per-centage being nearly the same in this affection as in the general returns, the remaining four are found to be greatly more frequent in connection with emphysema than under other circumstances. But this is not all; for, as tubercle is almost invariably connected with some form of condensation, and was so connected in many of the cases here referred to, and as all the cases of hepatization are also to be found under the head of bronchitic condensation, it becomes nearly certain that, of the whole forty cases of emphysema, not one had any direct connection with either hepatization or tubercle, as such, but only through the medium of the other lesions mentioned. Tubercle and hepatization, therefore, are in all probability merely the accidents, and not either the causes or effects, of emphysema of the lungs.

If now we consider the all but invariable connection of emphysema with one or other of the remaining lesions of the lungs, and the frequency with which all of them occur in emphysematous as compared with mixed cases, we shall be driven almost inevitably to the conclusion, that some circumstance, common to them all, and not necessarily present in hepatization and tubercle, is closely connected with the production of emphysema, if not, indeed, its real pathological cause. What that circumstance is, we may now endeavour to discover.

Mechanism of Emphysema.—Emphysema of the lungs was said by Laennec, in one of the most original and accurate of his descriptions, to have two varieties: the one being a dilatation of the air-cells, and finally a rupture of them one into another by removal of their septa; the other, a rupture of the air-passages directly into the interlobular areolar tissue. It is needless to repeat these descriptions, the distinction of vesicular and interlobular emphysema being well known to every one, or at least accessible to all, in words which cannot be improved. It is only necessary to add, that the microscope and other modern means of investigation, which have done so much for morbid anatomy, have scarcely availed here to augment our knowledge; having only succeeded in demonstrating more clearly the fact, known to Laennec, of the gradual breaking up of the vesicular septa, and the obliteration of their capillary network.[1]

Emphysema, therefore, is an abnormal distension of the pulmonary tissue with air. In its earliest stages, whether interlobular or vesicular, or, as frequently happens, both combined, nothing can be more certain than that it is essentially a mechanical lesion: in fact, the distension of the air-cells, giving the peculiar cushion-like and

[1] After frequent personal observation on this subject, I am compelled to regard the late theory of Mr Rainey, in regard to the dependence of emphysema on fatty degeneration of the lung, as fallacious. The granules described by him certainly do not always occur in emphysematous parts; and when they do so, they are so few, and so little characteristic of this particular lesion, that it is plain Mr Rainey's views have been founded on an imperfect appreciation of the relations of the so-called "fatty granules" to morbid tissues.

pale appearance to the lung, can be exactly imitated by inflating it with undue force artificially. Moreover, the whole of the subsequent structural changes implied in the gradual removal of the septa and obliteration of the capillaries, are readily explained by the mechanical effects of distension. Upon this subject M. Poiseuille, to whom we owe so many interesting facts in mechanical physiology, has a very beautiful experiment.

An instrument being adapted to the pulmonary artery of an animal, by which a given quantity of liquid was propelled with a given force through the capillaries of the lung, he found that this was effected, in the normal condition, in 29 seconds. M. Poiseuille now inflated the lungs so as exactly to fill the cavity of the chest; the time was still 29 seconds. On distending the lungs, however, farther, so as to produce the appearance of a partial emphysema, the time required for the passage of the fluid became lengthened to 62 seconds; when the emphysematous appearance was increased, 95 seconds; when it pervaded the whole lung in consequence of excessive distension, 129 seconds were required, and the fluid returned from the pulmonary veins mixed with some bubbles of air.[1] From these results, it is evident that whenever the air-cells are distended beyond the amount required or possible in the healthy condition, the flow of blood through the ultimate capillaries of the lung must be retarded or obstructed;—a condition not only corresponding with the appearances observed in emphysema, but readily accounting for the structural changes, the absorption of the walls of the air-cells, and the tension and obliteration of vessels observed in the latter stages of the disease.

It is, therefore, nearly certain that the source of emphysema is to be sought in a derangement of the mechanism of respiration, and not in any previously morbid condition of the affected part. Every thing denotes that the emphysematous parts of a lung are usually free from all diseased changes, with the exception of those which are the result of inordinate distension. The freedom from œdema and from morbid deposits, when other parts of the lung are so affected; the absence of accumulation in the bronchi, or at least its comparatively slight character, allowing of the perfect and easy inflation of the emphysematous parts when others are collapsed; finally, the habitual seat of emphysema in those parts of the lung which are usually most exempt from other disease,—all tend to prove what I have now stated. The diminished elasticity, the dryness, the anæmia, which have all of them been supposed to be the predisposing cause of this lesion, are manifestly nothing more than the effects of the distension with air upon the circulation and nutrition of the compressed walls of the delicate pulmonary air-cells. Even the

[1] Bulletin de l'Academie Royale de Médecine, vol. viii., p. 705.

small accumulations of granular deposit found by Mr Rainey may be accounted for by these secondary nutritive changes.

But emphysema is not merely a lesion resulting from inordinate distension of previously sound portions of lung; it is, as we have already seen, the product of mechanical derangement in the *sound parts* of lungs *otherwise diseased.* The existence of bronchitic condensation, of induration, of concretions, &c., if not a necessary cause of the production of emphysema in the sound air-vesicles, is at least in some way related to it. The theory of emphysema by Laennec, besides the objections offered to it in the former part of this memoir, in no way accords with the facts now adduced. Mucous obstruction of the bronchi, even if proved to exist, cannot determine, *directly*, both condensation and rarefaction of the lung; and we have already learned, from unquestionable and multiplied evidence, which of these two is its real result. The opinion of Louis, derived, apparently, chiefly from a consideration of the seat of election of emphysema as compared with that of bronchitis,[1] is opposed to the idea of any precise relation between these two affections; but this negative opinion would appear to be sufficiently answered by the numerical facts above adduced.

Some writers, conceiving, like Laennec, that emphysema is produced in the act of expiration, believe it to be the result of violent efforts of coughing, or other forcible expiratory acts. But have we really any direct proof whatever that cough, however violent, or any similar act, can produce emphysema, apart from the other accidents of bronchitis? In croup, in laryngitis, in aneurism of the aorta, we have cough even more violent and distressing than that of bronchitis; yet these affections are not known usually to cause emphysema, and I have repeatedly seen cases opposed to the idea of their having any such influence. The alleged unusual frequency of emphysema among players of wind-instruments is likewise totally devoid of proof, and rests upon one unsupported assertion by Laennec; whereas, if the real cause of emphysema were such as above described, no singer or wind-instrument player could in all probability remain long exempt from this disease. But it would require further to be known whether an increased liability to emphysema in this class is not accompanied with a similar proclivity to other pulmonary affections, before the question could be decided on such grounds.

But the most serious objection to the expiration-theory of this disease is, that the expiratory act is *mechanically* incapable of producing distension of the lung, or of any part of it. The act of expiration tends entirely towards emptying the air-vesicles by the

[1] "Si l'on se rappelle que le *maximum* de l'emphysème ordinairement a son siège au bord tranchant des poumons et dans leur voisinage, tandis que le catarrhe pulmonaire aigu intense a le sien en arrière et en bas, on sera forcé de conclure que si ce catarrhe a une influence quelconque sur la développement de l'emphysème, cet influence est peu considerable et ne s'exerce sans doute que bien rarement."—*Memoires de la Societé Médicale d'Observation*, tome premier, p. 253.

uniform pressure of the external parietes of the thorax upon the whole pulmonary surface; and even when the air-vesicles are maintained at their maximum or normal state of fulness by a closed glottis, any further distension of them by the expiratory force is as much out of the question as would be the further distension of a bladder blown up and tied at the neck, by hydrostatic or equalised pressure applied to its entire external surface. The air-vesicles can sustain no distending pressure from the column of air *within* the tubes, as that air only becomes compressed in virtue of a force acting on the *exterior* of the lung, which opposes exactly as much resistance without as it creates pressure within. It is singular that a theory so radically unsound, and so devoid of direct proof, as this of the production of emphysema by expiration, should have been allowed to maintain a place in medical literature.

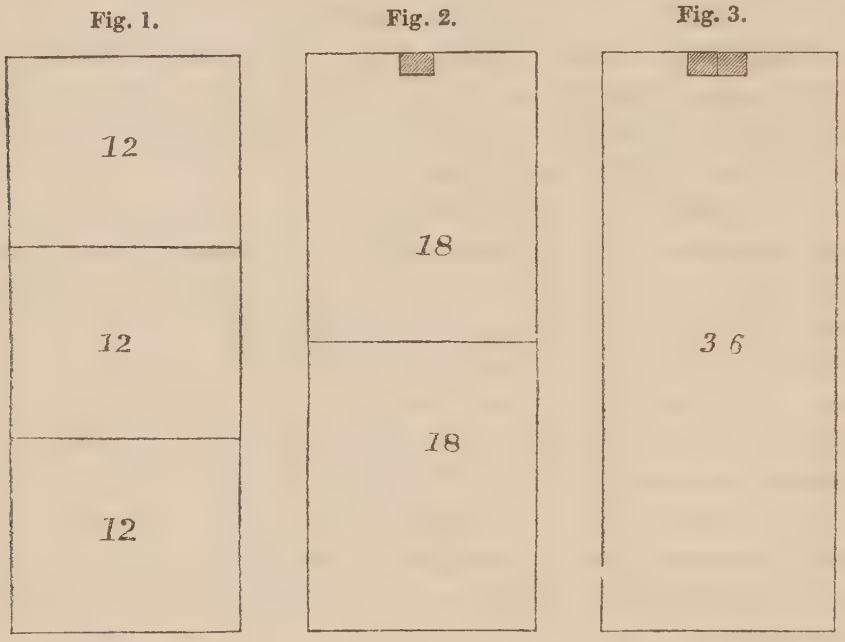
The only theories of emphysema which remain, are those which refer it to the act of inspiration. The most usual form assumed by these theories, is the supposition that emphysema of the lung is a physiological *compensation* for the occlusion of a diseased portion of lung;—a view not only giving no real explanation, but totally inconsistent with the fact, that in truly morbid emphysema there is always a diminished respiratory surface and consequent dyspnœa. Dr Williams, however, and some others, have placed the inspiration-theory in a more tenable position,—supposing that, when certain portions of the lung are occluded, the air is brought by inspiration to penetrate with greater force, and in greater volume, into the remaining parts.

This view is certainly near the truth, and is quite consistent with clinical stethoscopic experience. But it is clogged in Dr Williams's work with a reference to the incompetent expiration-theory of Laennec, as if the author did not see his way clearly to the explanation of all cases of emphysema by his own. Moreover, it is not the whole truth; because certain obstructive lesions have, as we have seen, no appreciable influence in causing emphysema; and also because it is evident that the inspiratory or expansive power of the chest is exactly limited by its capacity, and that even when a portion of lung is impervious to air, as in hepatization, the inspiratory force can no more distend the air-cells to the degree observed in emphysema than it can do so in the normal state. This fact will appear more clear from the following observations.

It appears to me that none of the writers on this subject have clearly apprehended, or at least clearly expressed, the single obvious condition which is necessary to the mechanical completeness of the inspiration-theory of emphysema. Emphysema is, according to this theory, a *complementary* lesion, dependent upon the previous existence of some form of occlusion of the vesicles, and invading the remaining sound portions of lung. Thus far it corresponds with all that we have hitherto seen, to an extent certainly not anticipated by Dr Williams, when, after enunciating his own view, he brings

forward Laennec's theory to account for residual unexplained cases. But there is yet another condition necessary, besides mere occlusion of the air-vesicles in a part of the lung: this is *partially diminished bulk;*—in other words, collapse or permanent atrophy of a portion of the lung.

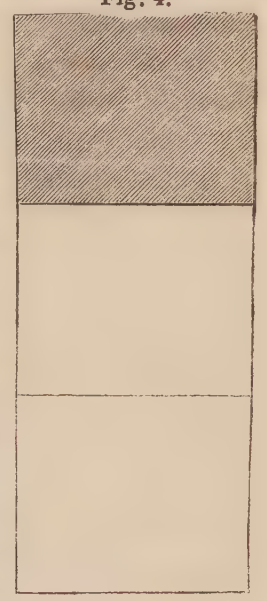
The operation and importance of this condition will be at once seen by the aid of a diagram. Suppose that in the accompanying fig. 1 the three equal partitions represent the *maximum* air-space, in



the normal condition of full inspiration of three lobes or portions of a lung (represented equal for the sake of simplicity). Each lobe holds, on a full inspiration, say 12 cubic inches or other measures of air; and it is adapted normally to hold this quantity, without pressure on the capillary circulation, or risk of violence to the texture of the organ. It is at once obvious that no amount of lesion, which leaves the upper partition or lobe of its normal volume, can at all affect the maximum expansion of the other two. They will continue, under all circumstances, to be capable of receiving their normal 12 measures of air; they will be prevented from receiving more, not by the tendency of the pulmonary texture to *resist* further expansion, but by the inadequacy of the mechanical apparatus *for producing* further expansion. No strain can in this case be thrown upon the walls of the air-cells; these still preserve their normal relation to the capacity of the chest which contains, and, by its dilatation, expands them. The inspirations will indeed be multiplied,—they will also be increased in fulness and force beyond the ordinary condition; but this can have no more effect in producing emphysema in the free air-cells of a diseased lung (under the above conditions) than running or violent exercise can have in relation to a healthy well-organised chest. This is the state which occurs in pneumonia, tubercle, and

all other lesions primarily affecting the air-cells themselves; it is represented in fig. 4, where the upper partition is supposed to be blocked up, at its full volume, with some abnormal deposit occupying the air-spaces. It may be granted that this diagram differs from what occurs in nature thus far, that even in the purest instances of hepatization the volume of the lung is seldom fully maintained; but it will be found, that exactly in proportion as it is so, the liability to emphysematous lesion is less.

Fig. 4.



But now suppose the occurrence of a lesion, in which the air-spaces of one of the partitions are closed by the collapse of its parietes, with diminution of bulk of the lung in this lobe. In this case, it is obvious that the expanding forces of inspiration will act inordinately upon the remaining lobes, and tend to attract into them the air which is prevented from entering the occluded one. If these forces were sufficiently powerful to overcome the resistance offered by the tissue of the sound lung under these circumstances, and if the sound portions of lung yielded equally in all directions, it is obvious that the condition established would be that in fig. 2, in which the lung is expanded to the normal maximum; but the air is differently distributed, being excluded from one lobe, and present in the others to the extent of 18 measures in each, instead of 12 as formerly. In like manner, the occlusion of two lobes, if accompanied with collapse of the tissue, would necessarily lead, in the event of the lung being fully dilated, to the accumulation of the whole 36 measures of air in the remaining lobe, as in fig. 3. A lobe thus distended would certainly suffer obstruction of the capillary circulation, as in the experiment before mentioned of M. Poiseuille; and the original purely mechanical condition would pass into one complicated by those structural changes which are actually produced in chronic emphysema.

It may be well to explain here, that a certain amount of overdistension, *when gradually effected*, is sometimes borne by the lung without the supervention of a distinctly morbid condition. The lung, under those circumstances, probably undergoes a genuine hypertrophy, the air-vesicles becoming slightly enlarged, but with a nutritive adaptation of the vascular and other structures to the changes thus effected. This enlargement of the lung, without the pathological characters of emphysema, is sometimes observed in disease, when the whole of one side of the chest has been contracted from pleurisy, the opposite lung passing, as the stethoscopist well knows, for an inch or two across the median plane in front, and having all its parts seemingly adapted to its increased size and function. A large power of adaptation of the lung to external circumstances is also shown (as has been pointed out by an acute

critic of my original communication on this subject to the Medico-Chirurgical Society of Edinburgh) "among the inhabitants of such lofty situations as the high table-land of South America," in whom "the chest becomes of a size considerably beyond its ordinary dimensions,"[1] owing to the permanent and constant necessity for inspiring a greater volume of air than in less elevated situations.

True hypertrophy of the lung is most readily produced when, as in the above cases, the distending force acts equally on the whole or a large part of the pulmonary tissue, and when it is so gradual as to give time for corresponding changes in the nutrition and circulation of the parts. For this reason, emphysema seldom arises to a marked extent when one lung replaces the function of another destroyed by pleurisy; the expansion of the sound lung remaining limited by the normal conditions until all the structures have gradually accommodated themselves, under the influence of exercise and habit, to the altered circumstances of the system. That emphysema may be produced, however, to a certain extent in such instances, is shown by the case marked XII. 349, as well as in others of a similar kind which I have witnessed. The forms of pulmonary disease in which emphysema is most readily produced, on the other hand, are those where the primary lesions have been much disseminated, so that every part of the chest, in its expansion, acts at once directly upon corresponding portions of lung partially collapsed or atrophied, and yet containing many comparatively unobstructed lobules, which yield readily to the distending force. Hence the most frequent of all combinations with recent emphysema, as may be seen by reference to the table, is a certain extent of collapse of the posterior portions of the lungs, with a number of disseminated lobular condensations between the emphysematous parts. As these lesions are also very rapidly produced, and give rise to dyspnœa extremely urgent, they are apt to induce accelerated and laborious efforts at inspiration, in the midst of which emphysema, either of the interlobular or vesicular kind, or both combined, very readily arises.[2]

The theory here proposed has already been advanced by various writers, and with different degrees of precision of statement, to account for those cases of emphysema which are connected with the cicatrisation of tubercular cavities and other kinds of pulmonary atrophy. It is obvious, however, that its true significance, and the extent of its application, cannot be understood, till it is clearly apprehended that all cases of considerable obstruction in bronchitis bring with them, *as a necessary consequence*, a certain amount of diminished volume in the obstructed parts of the lung; and, therefore, that the connection of emphysema with bronchitis need present no difficulty

---

[1] Med. Times, July 20, 1850, p. 72.

[2] The relation of emphysema to the violence of the inspiratory efforts, rather than to the apparent importance of the pulmonary lesion, is noticed by Rilliet and Barthez, "Maladies des Enfans," vol. i., p. 139.

to the pathologist, even when the latter has not been so violent or long-continued as to lead to any considerable amount of permanent and evident occlusion. That emphysema prevails in the opposite parts of the organ to those in which the direct effects of bronchitis are observed, becomes, in this point of view, one of the strongest evidences of its connection with that affection. That in the great majority of cases it is found in company with bronchitic collapse, or some lesion implying diminished size of the organ, amounts, I think, almost to demonstrative proof of the correctness of the theory here advanced.

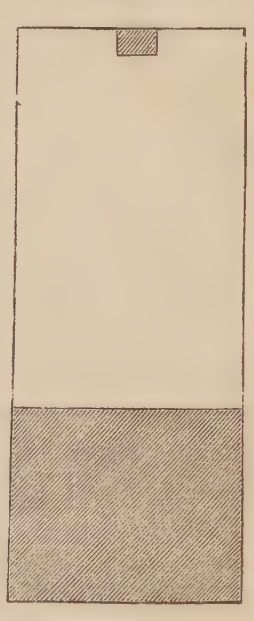
I am prepared, then, to maintain, that emphysema of the lung may, in all cases which I have witnessed, be satisfactorily accounted for by considering it as *a secondary mechanical lesion, dependent on some condition of the respiratory apparatus leading to partially diminished bulk of the pulmonary tissue, and consequently disturbing the balance of air in inspiration.* I, therefore, submit this principle to the judgment of the profession, in the confident anticipation, that it will prove no less constant and satisfactory in the hands of other observers, and will establish itself as the exclusive law of the production of this most important lesion.

A very few facts, in addition to the evidence already adduced, appear to be so striking as to deserve to be placed in an isolated form before the reader. One of these is found in Case IV., formerly narrated (No. for August 1850, p. 134), and also inserted in the table (XII. 360.) A child, in whom the right lung was normal, excepting imperfect bronchitic collapse, had in the left lung a mass of tubercular bronchial glands pressing on the bronchi passing to the anterior prolongation of the lower lobe, which was accordingly perfectly collapsed, void of air, and flaccid. The corresponding prolongation of the upper lobe, which in the acts of inspiration glides into the same angle of the pleural cavity, and the bronchi of which in this case were free, presented very marked interlobular emphysema in its early and perfectly recent condition, and the other parts of the lung were normal. Nothing can be more clear in this case than the relation of the collapse to the emphysema, both being recent. The following case, which occurred to me lately, is an equally striking illustration of this point. An aneurism of the aorta produced sudden death, by bursting into the air-passages. There was reason to think, however, from the symptoms, as well as the post-mortem appearances, that bleeding to a less extent had taken place internally some time before death, without being rejected by expectoration. The bronchi on both sides contained frothy blood, but the *lower* bronchial branches of the left lung were completely stopped up with coagula of blood. The mucous membrane throughout the air-passages was quite healthy, though stained purple. The right lung appeared externally uniformly emphysematous, or at least distended with air throughout the upper and middle lobe, and less so in some parts of the lower lobe. The

surface was marked with purplish irregular mottlings, which could be seen to be quite beneath the pleura, and shining through from the substance of the lung (blood-stains, without condensation). Considerable portions of the lower lobe presented distinctly-marked lobular collapse. The left lung was also generally emphysematous in the upper two-thirds of the superior lobe. The inferior third was partially condensed and flaccid. The whole of the lower lobe was violet-coloured, completely condensed, and flaccid, having all the external characters of carnified lung. The whole sequence of the phenomena is here again most evident: the coagulated blood in the lower air-passages, especially of the left lung, producing obstruction and collapse, while in the upper part of both it had merely produced staining or mottling of the tissue, the bronchi being free, and the tissue generally emphysematous. In like manner, in emphysematous lungs having, as is usually the case, distinctly marked collapsed lobules or portions in the anterior edge, I have frequently been able to demonstrate the excess in the bronchi of the latter of muco-purulent matter; and in all cases the greater amount of obstruction may be demonstrated by the attempt to inflate the lungs, when the emphysematous portions will be found to yield at once, while the others follow slowly and often imperfectly.

Relation of Emphysema to Hepatization and Tubercle of the Lung.—It has already been shown, that no apparent numerical relation exists between emphysema and hepatization or tubercular deposit in the lung; the per-centage of cases of emphysema accompanied by these affections being nearly the same as in the general hospital dissections. These facts agree in all respects with the theory just stated, which shows that morbid deposits, affecting the ultimate tissue of the lung, can have no direct connection with the production of emphysema, unless they lead, in the first place, to diminution of bulk, or atrophy of the parts involved. This is not the case either with tubercle or hepatization in their recent condition, except when connected with bronchitis, in which case they may lead to the condition represented in the diagram, fig. 5. If a lobe of lung be, in the first place, completely hepatised, a subsequent attack of bronchitis may produce the collateral lesions of collapse and emphysema in the remaining lobes, the hepatized part remaining indifferent both to the one and the other tendency. On the other hand, bronchitis and its attendant phenomena may be succeeded by hepatization or tubercle. In either case they exert no direct influence upon the mechanical conditions under which respiration is accomplished.

Fig. 5.



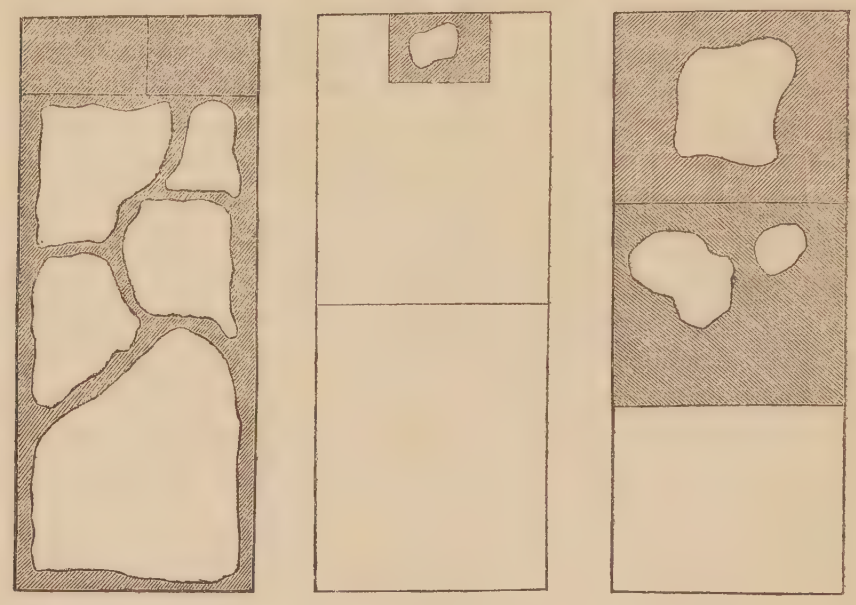
Relation of Emphysema to Excavations, and to Tubercular or Bronchial Abscesses.—The above remarks apply to tubercle in the

state simply of morbid deposit, before it has proceeded to excavation. The formation of cavities, however, in the lung, from whatever cause, is calculated to modify so considerably the mechanical conditions for the production of emphysema, that it is necessary to devote some consideration to this subject. As cavities are usually surrounded by condensed tissue, it becomes important to know the type of condensation found under these circumstances. It is not always easy to assign a distinct specific character to the anomalous forms of condensation found around tubercles in the stage of excavation; but one circumstance is, I think, usually evident enough, that they assume more or less decidedly the appearances of pulmonary atrophy, and are, especially in the later stages, attended with obliteration of all trace of the true pulmonary tissue. Under these circumstances, the mechanical conditions are necessarily established, which, according to the law previously stated, might be expected to lead to a compensating expansion of the sound tissue. In the case of cavities, however, another element comes into play; the expanding force of inspiration may expend itself either in dilating the air-cells, or in dilating the cavities; and it will, of course, act most effectively where it finds the weakest resistance. If the cavities be progressive, ill-defined, with weak and yielding parietes, they will fall under the influence of the inspiratory force, and be expanded to the full amount necessary to compensate the atrophy; and this result will follow the more easily, in proportion as they occupy, collectively, a larger proportion of the lung. On the other hand, if the cavities be healing, and surrounded, as they usually are in such circumstances, by very dense fibrous tissue; and if there be much sound pulmonary tissue, the cavities being few in number, and small, the expansion during inspiration will principally take effect upon the air-vesicles, and emphysema will be produced. These conclusions harmonise in all respects with the results of experience. Most pathological writers have noticed the frequent connection of emphysema with cicatrised cavities and healed tubercle; and even Rokitansky, whose opinions on the incompatibility of the two affections I have already noticed, admits that, with healing or healed (obsolete) tubercle, emphysema is frequently found. Is it too much to suppose that this whole doctrine of alleged incompatibility has been founded on the fact, that rapidly ulcerating and extending cavities, form a condition unfavourable to the development of emphysema; and that consequently, *in this stage*, they rarely co-exist? Such is, in truth, the whole extent to which, I believe, experience will be found to corroborate this hypothesis.

The relation of cavities to emphysema, then, may be shortly stated as follows:—1st, Large or numerous cavities, with flaccid walls, are, even when accompanied by atrophy—(see fig. 6.)—unfavourable to the development of emphysema; 2d, Cavities in process of cicatrization, if few or small in extent, and surrounded by

firm atrophied walls—(see fig. 7.)—are extremely favourable to the production of this lesion; 3d, Cavities, not surrounded by atrophied walls, whether large or small, exercise no particular influence in relation to pulmonary emphysema.—(Fig. 8.)

Fig. 6. Fig. 7. Fig. 8.



The great confusion which has hitherto existed on this subject, may be shown by reference to the best pathological works of the present day. The theory of Rokitansky, which derives one of its chief proofs from the relation of emphysema to tubercle, has been already noticed. Not to mention the absurd propositions made at various times to cure tubercular lungs by establishing emphysema, the following passage, from the careful and truthful work of Hasse, will show conclusively the correct apprehension he had of the facts, and the difficulty of finding a sufficiently general solution for them. "Those much in the habit of examining the dead body, cannot but be struck with two circumstances,—First, The almost invariable existence of emphysema in lungs, which bear the characteristic marks of recovery from phthisis;—and, Secondly, The *proportionate* rareness of tubercular deposits in emphysematous portions of lung. This would seem to show, that dilatation of the air-cells constitutes one of the conditions under which the cure of phthisis is possible, and again, that it forms *an obstacle to the development and progress of tubercle.*" Hasse then gives an accurate picture of the mode in which the cure of phthisis tends to the production of emphysema; and it is therefore clear, that, from the same series of facts, he makes two distinct inferences: one, that the cure of tubercle causes emphysema; the other, that emphysema causes the cure of tubercle. One of the two is, as I need not point out, obviously superfluous. In a subsequent paragraph he says:—"The different forms of active hyperemia are likewise subject *to the control of*

emphysema. Emphysematous portions of lung seldom become affected with œdema. Pneumonia, for the most part, *leaves exempt such lobules as happen to be emphysematous.*" [1] There is here the same inverted inference as in the other case; but no hint of the correct solution of the phenomena. The principle that emphysema is always a secondary lesion, and always complementary of other states involving diminished bulk of the lung, at once solves all such difficulties.

There remains for consideration the relation of emphysema to the chronic forms of pulmonary atrophy and induration, including concretions, and of these lesions to affections of the bronchi. This will form the subject of a concluding paper.

ARTICLE III.—*On the Treatment of Stricture by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.—(*Continued from Vol. XII., p.* 541.)

CAPTAIN ——, R.N., about forty years of age, applied to me, in the month of January last, on account of a stricture at the bulb, of which he gave the following history :—

"It was in the year 1829, when serving as a midshipman on board H.M.S. ——, in the Mediterranean, that I first observed symptoms of stricture. Bougies were then introduced, and in a short time the complaint seemed to be overcome, and continued so up to 1833, when I was in the East Indies, on board H.M.S. ——. I there suffered very much in boat service, — hunting pirates, &c.; and, under these circumstances, was much exposed to wet, which appeared to aggravate and rapidly increase the complaint, obliging me to seek relief on board the ship. But none of the medical men there could succeed in passing an instrument, and their frequent attempts generally caused complete stoppage of urine,—so that it was only from bleeding and hot baths that I obtained any relief. This state of matters continued for two years, until the ship was paid off,—no instrument having ever been passed. When on shore, living quietly, I got a little better. In 1836 I sailed for the Arctic regions, and, up to the end of that year, suffered little, with the exception of making water almost by drops; but then the ice suddenly broke up, exposing the ship to great danger, and obliging the officers and men to expose themselves for considerable lengths of time to a very low temperature—viz., 80° below the freezing point,—and upon some occasions to quit the ship. This had the effect of again sealing me up, and the old remedy of bleeding was again resorted to, as no one could pass an instrument.

[1] Hasse's Path. Anat. (Sydenham Society), pp. 313, 314.

My suffering, up to the end of the voyage (1837), was very severe. I was then sent to Chatham Hospital, and remained there for six weeks, with little or no relief, as nothing larger than No. 4 could be passed, and this produced very severe rigors at every attempt. Having learned to pass an instrument myself, I succeeded in keeping the passage open, until again, in the East Indies, in 1838, on board H.M.S. ——, it became so bad as to oblige me to return to England. In 1839 I sailed as —— of H.M.S. —— for the South Pole, and during the voyage, which lasted until September 1843, suffered very much, so that I was obliged to quit the expedition a few months before its conclusion. My urine then deposited a large quantity of mucus, of such consistency as to remain in its place when the vessel containing it was inverted. I also could neither make nor retain my water, which, issuing by drops and keeping my clothes wet, while the temperature was from 20° to 30° below freezing, rendered my sufferings very great. On my return to England, I went under the care of Mr Guthrie, who succeeded in passing bougies up to No. 6, but no larger. He then used a catheter with a lancet-blade; but this was of little use, as, in a month after, the passage was as much contracted as ever. I suffered severely from rigors while I was under Mr G.'s care. Since then I have been three years on the coast of Africa, and from time to time have suffered much from the horrible effects of my complaint causing excessive weakness, and depression of spirits. Upon two occasions lately I have been obliged to decline service, knowing that, had I accepted, I should certainly have broken down."

In commencing the treatment of this case, I found little difficulty in passing bougies from the smallest size up to No. 5, but then encountered a resistance which, instead of yielding to dilatation, was increased by every attempt to accomplish it, with aggravation of all the symptoms. Under these circumstances, and considering the patient's history, I recommended division of the stricture, and performed it in the usual manner. The operation was completed without the slightest difficulty or delay, with hardly any pain, and with no bleeding. There was no uneasiness or disturbance afterwards, so as even to interfere with reading to make the time pass. At the end of forty-eight hours I removed the catheter, and left the patient apparently quite well. In the evening I was sent for, on account of his being suddenly seized with vomiting and violent shivering, soon after making water for the first time. To any one inexperienced in the operation, the case would then have assumed a grave aspect, and suggested the suspicion of urinary infiltration; but as attacks of the same kind, though seldom so severe, were familiar to me as nervous symptoms attendant upon the urine resuming its natural channel, just as frequently happens after the operation of lithotomy, I begged the patient and his friends to dismiss any apprehension, assuring them that all would be right in a few hours. Accordingly, next morning I found no trace of the evening's dis-

turbance, and from that time forwards every thing went on well. The patient at once felt relieved from a load that had pressed upon him more or less severely for so many years, and quickly regained his health and strength; so that, while still under my care, having an offer of service similar to that which he had previously been compelled to decline, he did not hesitate to accept it; and, although circumstances afterwards prevented the expedition of which he was to have had the command, there was no longer any difficulty on his part. He continues in the enjoyment of perfect health, and in writing to me has thus expressed himself:—"I may now say, too, that no words can express the delight I feel at finding myself free from this horrible complaint, of which upwards of twenty years' experience has assured me that a cure could not have been effected by any other means."

Had the operation which I advocate produced no other fruit than the restoration of this gallant and accomplished officer to the service of his country, I should not regret the trouble which it has cost me.

(*To be continued.*)

ARTICLE IV.—*Case of Strangulated Inguinal Hernia—Repeated Attacks of Feculent Vomiting after the Operation.* By G. R. BALLINGALL, M.D., H.E.I. Company's Service.

23*d February* 1851.—Was sent for this morning to see a native christian servant of Mr Taylor, a strong healthy lad of about sixteen or seventeen years of age. He complains of severe abdominal pain, not constant, but coming on in paroxysms; states that he has had a hernia for about two years, which he was in the habit of reducing himself, but it had gradually been getting larger; and yesterday, while cutting wood with an axe, the exertion brought it down to a greater extent than ever, and he was unable to replace the gut. On examination, he is found to have an oblique inguinal hernia on the right side, extending nearly to the bottom of the scrotum, somewhat constricted above for about a third of its length, and feeling as if the intestine contained principally air. Tongue white and furred; pulse 90, pretty natural in character; countenance rather anxious. The pain is referred principally to the umbilical region, and is slightly increased on pressure; little or no pain in the tumour, except on pressure. States that he had one stool last night, consisting, however, of watery fluid only.

A very cautious attempt at reduction was made, but without the smallest effect. The attempt was not long continued, as pressure produced considerable pain. A large warm water enema was administered, and a dose of castor oil given by the mouth, but instantly rejected. The injection came away shortly after, bringing

away only some small scybala, which he himself described as resembling sheeps' dung. During the afternoon he was put into a warm bath, and reduction was again attempted, but with no more success than formerly.

24*th*.—Was very feverish during the night, and did not sleep. This morning he is in much the same state as yesterday. Has had several enemata, some of them purgative, but without any effect. He was bled nearly to syncope, an enema containing an infusion of 15 grains of tobacco was given, and he was again placed in the warm bath. While there, another trial was made; but although he was as much reduced as desirable, no impression was made on the tumour, although a slight gurgling was once or twice heard. He has had as yet no vomiting, except that caused by the castor oil. As the pain, on pressure, had somewhat increased towards evening, I determined to operate, Dr Hyslop concurring with me in thinking that farther delay was dangerous. Accordingly, about six P.M., chloroform (its first trial in Bagdad) was administered; and, with the assistance of Dr Hyslop, and Captain Jones of the "Nitocris," who kindly took charge of the farther supply of chloroform, and with several servants to hold lights, &c., I proceeded. An incision, about four inches in length, was made over the tumour, and the textures were cautiously divided by horizontal touches of the knife. While doing so, some glands were very troublesome, and it was necessary to enlarge the incision a little upwards. Two small arteries were twisted, and one, a little larger, tied. At length I reached what seemed to be the outer surface of the sac; and, on carefully opening this, about a teaspoonful of light brown coloured serum escaped. The incision was enlarged with a probe-pointed bistoury, and the congested intestine was then seen of a dull red colour, but not very dark. There was nothing in the tumour but one fold of it. The stricture, which was at the internal ring, was then divided with a probe-pointed bistoury; but some obstacle was still found, and, on examination, slight adhesions were found between the gut and the sac at its upper part. These were carefully separated with the finger, and the intestine was then easily replaced. The wound was secured by stitches, and a compress and figure-of-8 bandage applied. No pain was felt during the operation.

I saw him again about ten o'clock, when he was rather feverish, but had had no stool. An enema of warm water was administered, but without effect.

25*th*.—Easier this morning, but still no stool. Tongue very irritable-looking, and he frequently vomits after drinking anything. Several enemas with Ol. Terebinth. were given without effect.

26*th*.—Was feverish again last night, but is easier this morning. Pulse 92, pretty natural in character; tongue much the same as yesterday. A dose of croton oil has been given, which produced much pain and heat in the stomach. In the afternoon, as the pain on pressure over abdomen had considerably increased, he was bled

to 18 ounces, which somewhat relieved him. Enemas repeated without effect.

27*th.*—The symptoms of inflammation having been somewhat aggravated to-day, thirty-six leeches were applied in the neighbourhood of the umbilicus. Abdomen soft and natural in feel, and not distended by flatus.

1*st March.*—Still no good stool; once or twice very small lumps of feculent matter have come away with the enemata. Occasionally slight delirium, especially towards evening. Pulse wonderfully natural, but countenance rather anxious; tongue white and furred. Feels some appetite, and eats a little conjee occasionally, which is generally rejected soon afterwards. Abdomen harder and more distended, but not much pain on pressure. He has had several small doses of calomel, the only medicine he can retain, and also repeated enemata. During my visit, he suddenly vomited a large quantity of greenish fluid feculent matter, and along with it a round worm about eight inches in length. Has frequent eructations of very bad taste.

3*d March.*—Abdomen very much distended. Pulse quick and feeble; tongue still very foul. Removed the stitches from the wound, which is looking well; but, if anything, shows a little want of action. Ligature still firm. Occasional feculent vomiting continues. I was now obliged to leave Bagdad, and the rest of the case is from Dr Hyslop's notes.

4*th.*—This morning discharged per anum a piece of consistent healthy-looking feculent matter, as large as a small hen's egg. Pain not so constant as yesterday; abdomen not so much distended; tongue cleaner and moist; pulse 110, soft; countenance not so anxious. Slept a little during the night, and eat a little this morning. No vomiting since yesterday. A large soap and warm water enema given, and a little Ol. Menth. Pip. on a piece of sugar by the mouth; after which he vomited a large quantity of fluid yellow fæces.

Vespere.—Enema brought away some small pieces of fæces of natural colour and consistence; also some round black scybala. Has vomited fæces four times. Pain much easier, and abdomen softer; pulse 116.

5*th.*—Pulse 96, softer. Slept more. Complains of much abdominal pain. Abdomen not tenser than yesterday; tongue clean, moist. Some appetite; but whenever he eats, he vomits feculent matter. Complains of thirst.

Vespere.—Pulse a little quicker and weaker. Has not had much vomiting. Borborygmi frequent. States that he feels the contents of the bowels driven down to the anus, where they seem to be arrested by something, and then return. He is obliged to sit up during the paroxysms of pain, and after each heaves deep sighs. No medicine given.

6*th.*—Slept a little last night, and pain not so severe to-day.

Abdomen more tense. Convolutions of intestine seen through abdominal parietes. Pulse 108, compressible; tongue white at edges, brown in centre. Vomited twice last night. A large enema containing Ol. Ricini given.

Vespere.—Very low, and slightly wandering; frequently dozing. Pulse very feeble. Has eaten a little, but has been vomiting frequently. Enema brought away some small lumps, and also very fetid fluid matter.

7*th.*—Pulse not so quick, and stronger; tongue clean to-day. Slept a little. Vomiting continues. Appetite greater. Abdomen much slacker. Passes much pale urine. As he continued to ask for medicine, the following was given, principally with the view of relieving pain:—Calomelanos, gr. i., Pulv. Doveri, gr. viij.

8*th.*—Slept after the medicine, and at three P.M. had a small fluid stool while asleep. Since then has had three stools, fluid and feculent. During the night slept little, and had twice vomiting; none this morning. Tongue pretty clean, but dry; little pain, but a feeling of faintness and vacuity at pit of stomach; pulse 98, soft and fuller; some appetite. During Dr Hyslop's visit, he passed a large quantity of almost colourless fluid, with some small lumps of fæces. This passed before he could call for the bed-pan.

11 A.M.—No return of vomiting. Has been twice purged; stools very fluid. Much debility. Pulse quicker.

5 P.M.—No return of vomiting. Has had in all fifteen stools, chiefly fluid, containing grains of rice, and other undigested matters which he has been eating.

9*th.*—Had fifteen drops of laudanum last night. Slept pretty well. Two small stools since. Feels much better. Pulse 96, stronger; tongue not so clean. Has tolerable appetite; no abdominal pain; fulness of abdomen quite gone; thirst. Light diet of arrowroot enjoined.

10*th*—*Vespere.*—Continues to improve rapidly. Tongue clean; pulse 96, stronger. Great appetite; no thirst. Slept well. Has had two small stools of good consistence and natural appearance. No pain in abdomen.

26*th.*—Has continued to improve steadily. Appetite for some time very great; bowels are now regular. Has been walking a little for some days with a truss. Wound quite healed.

ARTICLE V.—*Dangerous Hemorrhage accompanying the Expulsion of a Bunch of Hydatids Interspersed with Moles.* By JOHN BOOKLESS, Esq., Surgeon, Kelso.

EARLY on the morning of the 23d ultimo, a messenger came for me in great haste to proceed immediately to the country, a distance of seven miles, to see Mrs A., who, he informed me, was in a danger-

ous state, and would likely be dead before my arrival. I set off instantly, and when within 200 yards or so of her residence, I heard her screaming in a most alarming manner. Though several attendants stood around the bed, not one could tell me what was wrong; and as Mrs A. was very restless and obstinately taciturn, it was some minutes before I could prevail upon her to let me know how she felt, and what occasioned her so much pain. After a considerable effort, in consequence of extreme exhaustion from the loss of blood, she whispered in my ear that the pain was all in the back, and that for more than a week she had had repeated attacks of uterine hemorrhage. Mrs A. also said she was dying, and her exsanguined appearance certainly betokened great danger. Pulse very quick and very weak, and at times scarcely perceptible; surface of the body cold; breathing heavily. After running my hand over the abdomen, and feeling some slight enlargement of the uterus, my suspicions were immediately directed to the condition of that organ, and I lost not a moment in making an examination per vaginam, when I discovered a large soft mass protruding through the os uteri, which I succeeded in extracting after a little manipulation, and which turned out to be a considerable cluster of hydatids interspersed with moles. Their removal gave her immediate relief.

As my patient had previously lost a great deal of blood, the bedclothes being literally saturated, and as it still threatened to come away in gushes, I had towels wrung out of cold water applied over the lower part of the abdomen and upper parts of the thighs, and had them diligently continued for upwards of two hours. Brandy-toddy was liberally and frequently administered, alternated with doses of a weak infusion of the secale cornutum. Cold water was occasionally dashed over the face, and bottles of hot water constantly kept at the feet. For nearly an hour an occasional gush of blood was discharged, and at times Mrs A. seemed as if she was about to expire. The gushes, however, became less frequent and smaller in quantity; and as the restorative and precautionary measures were rigorously enforced for some hours, I had the satisfaction, before I felt it safe to leave her, of hearing her say that she felt "very comfortable." Her after recovery was highly satisfactory.

Mrs A., aged 49 years, is the mother of twelve children, and all alive. I have attended her in her six last confinements, all of which were unusually protracted and painful. As the catamenia had left her for some months, she was in the belief that *a change* was going on, when she was one evening suddenly seized with pain in the back, accompanied with a discharge of a sanguineous nature, which took place ten days or so before her severe illness; she had seen more or less of it every day up to the time I was sent for. In this case there was no abdominal distension, and no mammary enlargement. Though I have been in extensive obstetric practice for the last seventeen years, having attended nearly 2000 cases, I have only met with three

cases of this description. Obstetric authors do not seem to have met with many such cases, if one may judge from the summary and careless way in which they have been noticed.

KELSO, 4th June 1851.

ARTICLE VI.—*On the Natural History, Action, and Uses of Indian Hemp.* By ALEXANDER CHRISTISON, M.D., Edin., Annual President of the Medical Society of Edinburgh.[1]

INDIAN hemp has been long known in India, Persia, and other Eastern countries as a medicinal and intoxicating agent, but was little known to Europeans until it was brought prominently into notice by Dr O'Shaughnessy of Calcutta, in the year 1839. [On Indian Hemp, &c.; Calcutta, 1839.]

The ancients were almost ignorant of its virtues. The Greek physicians, as we are told by Dioscorides, were acquainted with the emollient properties of the seeds of hemp; but they seem to have been wholly unaware of the narcotic virtues of the plant (Diosc. iv. clv.). Galen and his contemporaries were not much more informed on the subject; for that author merely speaks of its seeds being sometimes used as a whet after supper, to create a desire for wine, but condemns the practice, because, when used freely, they heat the system and cause determination towards the head (Opera, ii. 53, Edit. Basiliæ, 1549; De Facultatibus Alimentorum, c. xlix.). It is alleged, however, that hemp was known at an early period to the Chinese. In a communication to the Académie des Sciences in 1849, extracts are produced from a Chinese work, showing that so far back as A.D. 220, a Chinese physician, named Hoa-Tho, produced insensibility in his patients by means of a preparation of hemp, and that operations were then performed without pain to the patient (*Stanislas Julien*, in Comptes Rendus, &c., 1849, p. 197). This statement would, however, require further confirmation. There seems little doubt that in the year 600 the Hindoos were in the habit of employing hemp; and that it has been in constant use ever since as a means of allaying pain, and more particularly as an intoxicating agent, among the inhabitants of the East.

Its properties have been alluded to by various good European authorities several centuries ago, but by no one more distinctly than in 1695 by *Rumphius*, who describes its effects from personal observation at Amboyna. He says that throughout India the leaves and seeds are extensively used to dispel anxiety and to excite agreeable dreams; that a maniacal state sometimes ensues; that he has known it cause, when smoked with tobacco, a frantic pugnacity in some,

[1] This is an abstract of an Inaugural Dissertation, for which one of the prizes of the Medical Faculty of the University of Edinburgh was awarded in August 1850.

sardonic grins and menaces in others, and lamentation in others; and that its effects vary much with individual temperament (Herbarium Amboinense, Edit. *J. Burmann*, 1750, v. 209). Frequent mention is made of it by authors after that period. But Dr O'Shaughnessy has the merit of having first brought the plant fully into notice in Europe; and since his essay was published, many experiments have been made with it. The expectations held out by him have not yet been fully realised. One explanation of this appears to be, that many have used spurious or ill-made preparations, and that others have not made sufficient allowance for the influence of constitutional peculiarities in affecting its action.

BOTANICAL HISTORY.

From the observations of Dr Royle, it appears that Cannabis is a plant of the Persian region, where it is subjected to great alternations of heat and cold; and that it has passed on the one hand into Europe, and on the other into India.

Two species of Cannabis have been described by many botanists —viz., *C. Sativa*, and *C. Indica;* but repeated comparison has not detected any material difference. Dr Royle remarks that, "like Dr Roxburgh and others, he was unable to detect any difference between the plant of the plain and that of the hills of India, nor between these and the European plant. The Indian secretes a much larger proportion of resin than is observable in the European plant; but a difference is observed in this point even in India between plants grown in the plains and those of the mountains, and also when grown thickly together. The natives plant them wide apart, to enable them to secrete their full powers. In Europe, the thick sowing, and moister, often dull climate, will prevent the due secretion of the peculiar principles of a plant of the Persian region."

As I have not observed that any direct experiment has been made on this subject, it appeared that by growing the seeds of the Indian plant in this country, and comparing the result with the common hemp, the question might be determined; and accordingly the following experiment was made in the Edinburgh Botanic Garden :—A few seeds picked from dried tops, or *Gunjah*, in good condition — which had been obtained the previous year in the bazaars of Bombay by Mr Henry Johnston, of the East India Company's service—were sown on the 17th of March 1849. They germinated in a stove-house, and plants appeared above ground in a few days. In the course of a week they attained a height of 3 inches. Three shoots were planted in the open air, while the remainder were left in the hot-house. On August 1st those without had attained a height of $4\frac{1}{2}$ feet, and it was remarked that they had a peculiar strong minty odour. On the 1st October one of these was $9\frac{1}{2}$ feet high, with several strong, somewhat woody stems, and abundant, dense, coarse foliage; flowering appeared to be commencing; but the advance of the season, with accompanying cold weather,

arrested any farther development. The plants in the hot-house at the same period were only 4 feet high, slender, with more sparse, smaller, and tender leaves, but in full flower. Plants of the common or European hemp growing in the garden had a very similar aspect to the latter, being, however, in full fruit.

I am indebted to the kindness of Professor Balfour for the following remarks, and botanical description of these plants :—"Those in the open air were all female plants; among those in the hot-house were one or two males. I have not been able to make out any specific difference between the so-called *C. Indica* and *C. Sativa* of Europe. The common hemp in the garden has not attained the same size as the plants from Indian seeds, and the segments of the leaves are narrower; in other respects they appear alike, more especially as regards their flowers, glands, &c. Both the Indian and European seeds produce plants which have a strong resinous odour; in this respect the European plants in the garden seem to excel the Indian. On the Indian specimens, even when cultivated in the hot-house, there has not appeared any of the *churrus* described by Indian observers. The racemes and spikes of flowers, however, have a resinous feeling when touched. The following is a description of the plants raised from the Indian seeds :—

"*Flowers* dicœcious.—*Male* plants in the hot-house about four feet high, circumference of stem at the base about one inch, lower part of the stem woody; *stem* somewhat quadrangular, grooved and roughish, surface of the stem at the base of a brownish colour, mixed with greenish streaks; *leaves* opposite, sap-green above, pistachio-green below, quinate to septenate; at the upper part of the stem the leaves become alternate; segments of the leaves feather-veined, with a prominent midrib below, lanceolate, acute, with large serratures; stipules 2, subulate.

"*Flowers*, in cymose axillary leafy clusters, some of them abortive. —*Perianth* of 5 ovate blunt segments, which are of a pale green colour (the margins being white and the centre greenish), with a marked green midrib, covered externally and internally with glandular pubescence, segments of the perianth concave internally. Stamens covered with glandular pubescence, opposite the segments of the perianth. Anthers large, projecting beyond the perianth, oblong, bilocular, erect, with an apicilar process and longitudinal dehiscence, supported on slender filaments, which are shorter than the anthers, and have pyramidal bases. Pollen spherical, with three facets, each consisting of a small ring in the centre of a larger one. In the centre of the flower there is the rudiment of the pistil.

"*Female Plants*.—These are much stronger than the male plants, have attained a greater size, and have a stronger balsamic odour; those in the hot-house attained a height of 5 feet, and those in the open air 9½ feet; stems hollow, 4 inches in circumference in the plants out of doors, with a tenacious stringy bark. Leaves covered with minute vesicular sessile glands, which give out a viscid

resinous-like exudation, and are interspersed with glandular hairs. Flowers in aggregated spikes; usually three or more unibracteate flowers in a cluster in the axil of floral leaves, which are often tripartite.

"*Perianth* monopetalous, convoluted, swelling at the base, where it includes the ovary. Floral leaves, bracts, and perianth covered with glandular pubescence.

"*Pistil* one; ovary one, rounded, containing a single orthotropal erect ovule. Style short, terminal, ending in two elongate filiform pubescent stigmata. *Fruit* a Caryopsis. Seed erect, marked with a coloured hilum. Embryo exalbuminous."

To this description I may add, that the Indian seeds from which these plants were raised were not distinguishable by any character whatever from European hemp-seed.

From this investigation, so far as it has been possible to extend it at present, the following conclusions may be deduced:—That the minute glands, under favourable circumstances, would act vigorously in producing the resin which is so abundant on the plant in India, but which was in very small quantity on the plants grown here:—That these glands secrete with activity only in a certain climate, which we cannot imitate in this country, either out of doors or under glass:—That the hemp plant possesses a peculiar minty odour, which is not sufficiently alluded to by authors:—That the *C. Indica* and *C. Sativa* are one species.

PREPARATIONS OF HEMP.

The principal forms in which hemp is met with in the markets of the East are—1. Haschich; 2. Bhang; 3. Gunjah; 4. Churrus; 5. A variety of pastes, electuaries, &c., in most of which the vehicle, or *excipient*, is butter, or other oleaginous matter; 6. Tincture of hemp.

Haschich is the Arabian name given to the dried tops of the plant grown in Upper Egypt. These tops are gathered some time before the seeds reach maturity. The meaning of the word is "herb," or "*herbe par excellence*," and it is also the name applied by French authors to the different dried preparations of hemp.

Bhang is an Indian preparation, consisting of the larger leaves and capsules. According to Dr O'Shaughnessy, it is the cheapest form used in India, and therefore in common use among the lower orders for smoking. From it is prepared an intoxicating drink, and it forms a part of the confection called *Majoon*.

Gunjah is the principal Indian form of the dried plant, and consists of the dried tops after flowering, from which the resin of the leaves has not been removed. It is therefore identical, or nearly so, with the Arabian haschich. It is chiefly sold in the Calcutta bazaars for smoking, in bundles three feet long and three inches in diameter; the colour is dusky green, the odour agreeably narcotic, and the whole resinous to the touch. The specimens I

have examined, sent to Dr Christison by Dr O'Shaughnessy, consist of a central stem, with branches, round which and at their extremity, are elongated oval masses of compressed leaves and young fruit, about one inch and a half long, and adhering together by adhesive resinous matter. After being steeped in water, these masses may be spread out, and are found to consist of the flowers, seeds, and smaller leaves. *Churrus* is the resinous secretion mixed with variable proportions of leaf-fibres; it is not met with in Europe except as museum specimens. Those in Dr Christison's museum consist of variously sized, nodulated, rounded masses, from the size of a pea to that of a walnut, and of a dirty grayish-green, or greenish-black colour. A specimen from Yarkand, in Thibet, lately received from Mr Jameson of Saharunpore, is as big as two fists. It is collected during the hot season by scraping the leaves and tops. Dr O'Shaughnessy states that in Central India and Nepaul men in leathern dresses brush forcibly through the plants, and the resin which adheres to them is then scraped off. Dr Royle says the glandular secretion is collected from the plants on the hills by the natives pressing the upper part of the young plants between the palms of their hands, and scraping off the secretion which adheres. This is also the account given by Mr Jameson in a late letter to Dr Christison. And Dr M'Kinnon states that in Nepaul the resin is gathered on the backs of naked Coolies. The most esteemed quality in the Bombay market, according to Mr Henry Johnston, is that which is in roundish warty lumps, of the size of a large pea or marble, of a greenish-black colour, and of a resinous appearance.

In the preparation of the *Electuaries*, &c., butter is used as the means of separating the active principles; consequently these compounds are very apt to become rancid. They are thus described by M. Chaniac in the "Annuaire de Therapeutique" for 1846, as used in Egypt:—1. Preparations mixed with honey or melted sugar. 2. A more active form, called haschich kava-mesk (musked drug), containing musk, essence of roses, and almonds, of pasty consistence, and of the colour of impure honey; the quantity used being about the size of a walnut. 3. Two kinds are found at Smyrna, called Israël, the one a fine powder, the other a roll of firm mastic consistence. 4. A black round kind has great aphrodisiac repute among the fellahs; but in this case it is found that cantharides is added to increase the effect of the drug. At Cairo, the compound from which the various conserves are prepared is thus made: Equal parts of well-sifted haschich, butter, and water, are put in a vessel on the fire; after some boiling, the water is dissipated; the residue is twisted in a cloth to isolate the fatty matter, and to this the different spices are added.

Tincture.—Landerer describes a tincture of hemp used at Cairo, called *chatsraky*, made by infusing in spirit for three weeks, with a gentle heat, the varnish-covered bark sliced from the stems when the plants are in flower.

The activity of the preparations of hemp depends on the presence of the resinous varnish on the leaves. Consequently, as the most active leaves must be those which contain the largest quantity of resin, it becomes a matter of great importance to decide upon the proper period for collecting the plant.

M. Gastinell, an apothecary at Cairo in 1849, states that he found the active powers of hemp to depend on a resinous matter which forms on the leaves as the seeds ripen. Again, M. de Chaniac observes, that in Egypt the tops of the plants gathered at the end of flowering, but before complete maturity of the seeds, are used; and Mr Jameson, Director of the Botanic Gardens at Saharunpore, makes a like statement in a letter to Dr Christison, dated 17th August 1849, which contains an interesting account of hemp in that part of India. He says—"In Kimaon and Gurwhal cannabis is grown in large quantities, partly in order to obtain its resinoid secretion, and partly for its bark, from which a strong coarse cloth, called bungila, is manufactured, for the dress of the poorer inhabitants, particularly through Gurwhal. It is sown in July, and gathered in October. From the female plants only the churrus is procured. Towards the beginning or middle of October the seeds begin to form, and when in this unripe state the upper part of the plant is pressed between the palms of the hands, and deposits upon them a yellowish-green secretion, which is scraped off with a blunt knife; this is the well-known churrus. From the male plant bhang and cath are prepared. Bhang is prepared by drying the leaves and other parts of the plant, both male and female being used. It is thus prepared for use:—A small quantity is put into a mortar with a little water, and pounded; the refuse water being thrown away, an additional quantity of water is then added, from a half pint to a pint, depending on the strength required, and well mixed; it is then strained through a fine cloth, the residue thrown away, and the liquid is ready for drinking—a wine-glass-full or more being taken at a time. Gunjah is the third preparation, and is the produce of the upper portion of the stem—that is, about one and a half feet; it is only used in the hookah to smoke. This also applies to the churrus. The Gunjah is carefully dried and mixed with an equal quantity of tobacco, and well rubbed together in the palm of the hand; it is then ready for the hookah. We have thus the three preparations—1. Churrus; 2. Bhang, or Lubzi; 3. Gunjah, or Ghunjah. The first is only prepared on the hills, and the two latter are common to both hill and plain; but bhang is principally prepared in the plain. At Bhaeit, about sixteen miles from Saharunpore, it is prepared in large quantity, and is subject to a heavy duty. From 40,000 to 50,000 maunds are produced yearly (a maund is equal to 80 lbs). The reason why the churrus is not prepared in the plains is, because the plant does not secrete the resinous principle, showing that its secretion is connected with climate. But still the plants are identical in external characters;

and you will, I think, find that the European and Indian plants are also identical. In order to ascertain the fact, I send you a small packet of hemp-seeds, procured at one of the Gurwhal villages, where it is grown in vast quantity. In your letter you say that the active principle forms on the stem and leaves; this is not the case, as it is only procured when the seeds are in an unripe state; attempt to procure it before this period, and none will be forthcoming. It will appear strange how ignorant natives can distinguish female from male plants; were you to see the plant growing, your surprise would soon be removed. The female plant, when ready for making churrus, has at its upper part a "bunchy" appearance, whereas the male plants have become by this time mere stems and leaves, the flowers having fallen off. In October, in crossing the Himalayas from Almora to Mussuri, I have passed through dozens of villages 6000 to 8000 feet above the level of the sea, and seen hundreds of men, women, and children, all employed in making churrus. The plant grows to a height of from ten to fourteen feet."

The plants cultivated in the Edinburgh Botanic Garden present exactly the characteristic difference between male and female described by Mr Jamieson.

From these observations, then, it appears to be undoubted, that the only period for collecting the plant in its active state, is about the time of ripening of the seeds, at which period the tops of the plant are covered with the resinous varnish on which its properties depend.

The preparations of Indian hemp, hitherto alone used in this country, are a spiritous extract, and a tincture made from it. The resinous secretion is soluble in oils and spirit, but not at all in water. Hence an alcoholic extract is alone admissible, and not a watery one. Gunjah is used for obtaining it. Two extracts have been used in this country: one prepared in London, and the other from Calcutta. There is reason to believe, that the good qualities of English extract are prepared by boiling gunjah in rectified spirit, or by the method of displacement without heat. The finest extract I have seen, is one prepared by Mr Robertson, professor of chemistry at Calcutta, which, however, is not in the market. Of this Mr Robertson prepared about thirty pounds. From a hundred weight of gunjah he obtained about eight pounds of extract. His process consisted in passing the vapour of boiling alcohol downwards through the gunjah, packed in a cask,—an ordinary worm leading from the cask to a receiver. The preparing of it cost him much time and trouble; and, on account of the heavy duties upon hemp, and also upon spirit, he reckoned the expense at fifteen shillings a pound. On these accounts he abandoned the attempt to manufacture it in this way. Specimens, however, were sent for experiment to various parts of Europe, and, among others, to Edinburgh, for Dr Christison;—this is now (1849) four years old, yields a beautiful green tincture with rectified spirit, retains all its energy, and is more active than the ex-

tracts of the shops generally seem to be. I have repeated Mr Robertson's process on a small scale with gunjah, and found it to be a very convenient and complete means of rapidly exhausting the plant, while at the same time the consumption of spirit is less than in the process by cold percolation.

Good extract thus prepared, like all other good extracts that I have seen, has a dark greenish-black colour, a firm consistence, and great tenacity, a faint peculiar aromatic odour, and a feeble peculiar bitterish taste. It softens in the hand, and adheres with great obstinacy to the fingers; it is not at all miscible with water. It forms a beautiful deep green tincture with rectified spirit, in which it is easily soluble. I have sometimes seen in the shops an extract, which is emulsive, so as to be easily diffused in water, and even rubbed down with the finger. All such must be spurious, and are probably feeble, if not inert.

Various investigations have been made as to the nature of the resinoid secretion of the leaves of cannabis; and it has been ascertained that a resin can be separated from it, retaining the properties of the plant in full energy. Gastinell, apothecary at Cairo, has prepared this substance, of which he says two grains are as effective as six of alcoholic extract.

M. de Courtive of Paris says, that the resin prepared by him is, in the dose of three-fourths of a grain, as effective as thirty grains of the butter-extract (*extrait pur, au beurre*). He also prepared the resin from Paris-grown Indian hemp, and from French hemp, and he found that six grains of the first, and eight to sixteen of the second, were necessary to produce the effect [*Comptes Rendus de l'Institut*, 1848, p. 509]. I have found one, two, or three grains of Mr Robertson's alcoholic extract equal in activity to three-fourths of a grain of the resin.

The Messrs Smith of Edinburgh have succeeded in concentrating the properties of the extract in a soft neutral resin, which they are disposed to regard as its active principle, and have denominated Cannabin [*Pharm. Journal*, 1846-47, p. 171]. The essential part of their process, which is somewhat complex, consists in removing from gunjah whatever is soluble in warm water, and in a cold solution of carbonate of soda, and then preparing from the residuum an alcoholic extract with rectified spirit. In this way colouring matter, fat, and chlorophyle, are in part removed. The rest of the process, by which it is proposed to purify the resin still farther, by the successive use of milk of lime, sulphuric acid, animal charcoal, and water, has not appeared to me to make any material difference in the preparation.

The Messrs Smith got seven or eight per cent of this resin from gunjah, which is just what Mr Robertson obtained of alcoholic extract. From Dr O'Shaughnessy's gunjah I obtained fully ten per cent. The numerical results were, from 3840 grains, 500 of watery

extract, 640 alkaline extract, 2310 fibrous residue, and 390 purified resin. Messrs Smith found two-thirds of a grain of this resin to produce decided effects. But a single grain of Mr Robertson's extract has also proved very energetic. I fear, therefore, that there is little advantage in substituting for a well-prepared extract the resin obtained by so operose a method as that of the Messrs Smith. Still less can it be admitted, that they have detached the active principle of the plant; and, indeed, they have very properly assigned the name Cannabin only with a query. The discovery of the true active principle, or even of a uniform resin, is, however, a great desideratum in medicine; for until this object be attained, it is scarcely possible to rely on the action of any preparation of the plant.

PHYSIOLOGICAL ACTION.

The physiological action of hemp is, in the first place, stimulant, in small doses, exciting the cerebral and digestive systems; and, secondly, when given in larger quantity, its effects are powerfully sedative and antispasmodic.

The first question which presents itself is, whether this plant is a poison in large doses, and so much so as to occasion death. The prolonged use of it is said to destroy many in India, by gradually undermining the health; but no mention is made by authors of its proving fatal in one or two large doses. The only allusion to such an effect that I have seen, is by Mr Reddie, member of the Calcutta bar, who, in a letter to Dr Christison, dated July 1849, says,—"The plant is a poison, with many of the qualities of opium, and some singular ones peculiar to itself;" and adds, that "it is frequently used at Calcutta as a poison." Dr O'Shaughnessy made a series of experiments on animals, with the view of determining the quantity that it would be safe to administer as a medicine, but in none of these did death occur. In one experiment he gave ten grains of churrus to a middling-sized dog. "In half an hour he became stupid and sleepy, dozing at intervals, starting up and wagging his tail, as if extremely contented, and eating food greedily; on being called to, he staggered to and fro, and his face assumed a look of extreme drunkenness. These symptoms lasted about two hours; but in six hours he was perfectly well and lively." On another occasion, a little dog got twenty grains of alcoholic extract of gunjah, but was not killed. In none of his experiments was any pain evinced, or any convulsive motion. The dose mentioned above was the largest he gave; and the question remains, whether larger doses would have a fatal effect. One point is, however, determined,—that in so large a dose as ten grains, churrus will not prove fatal to a dog; and it will be seen that Dr O'Shaughnessy often administered equally large doses with benefit in treating disease.

An interesting result of Dr O'Shaughnessy's experiments is, that carnivorous animals and fish very speedily underwent the effects of

hemp, while graminivorous animals were very slightly affected, even by large doses of the drug.

The effects of hemp on man will in general be found to correspond, as Rumphius indicates, to the temperament and natural disposition of the individual. In some, mere laziness and stupidity are induced; in others, a pleasing state of reverie, without any other remarkable condition; and many are attacked with loud laughter, fits of dancing and singing, venereal appetite, or inclination to quarrel, according to their various dispositions. In many there is a remarkable desire for food, which the individual eagerly devours, while at the same time he does not appear to be satisfied, and can with difficulty control his appetite. After the stage of excitement, sleep supervenes; and, on waking, the experimenter returns to his natural state, except that the ideas are often confused for a little, and in some cases vertigo is present to a slight extent. It is in consequence of these remarkable properties that the compounds of hemp have come into such extensive use among eastern nations, especially those where wine is not used. An example of the great extent to which the use of hemp is pushed in India, is given by M. Liautaud [*Annuaire de Thérap.*, 1845] in his communication to the Académie des Sciences, as follows:—"The grand feast of Dourga Poudja is terminated by the ceremony of immersing the idol in the river; after which the people retire to intoxicate themselves with a drink from the leaves of hemp, and the whole ends in a scene of disgraceful drunkenness." And, in allusion to the physiological action, M. Liautaud remarks that there is peculiar ecstasy without convulsion,—and that the drinks excite the nervous system more than the powder or smoke. This intoxication has appeared to him much less intense than that of opium, and that produced in the Chinese smoker; the consequences are not so deadly, but the moral degradation the same.

Dr O'Shaughnessy thus describes the delirium induced by the incautious use of hemp in India:—"The state is at once recognised by the strange balancing gait of the patient, a constant rubbing of the hands, perpetual giggling, and a propensity to caress and chafe the feet of all bystanders, of whatever rank. The eye wears an expression of cunning and merriment which can scarcely be mistaken. There is no increased heat or frequency of circulation, and the skin and general functions are in a natural state."

The effects of hemp are, in the generality of instances, highly pleasurable, while in a few cases most disagreeable sensations are produced. The ideas which enter the mind are often very fantastic, and of most interminable variety. In illustration, I have selected the following cases:—

Case I.—In the work of Moreau, "Du Haschich," M. Théophile Gautier, in describing his sensations, says,—"After a feeling of numbness, it appeared to him that his body became transparent, and that he saw within his breast the haschich which he had eaten in the form of an emerald, from which issued millions of little sparks. At

the same time, his eyelashes became indefinitely elongated, and began to roll as gold threads upon small ivory wheels, which revolved with great velocity." A remarkable effect was an increase of his power of hearing, whereby slight noises became as loud as thunder; and he heard the noise of colours—green, red, blue, and yellow sounds coming to him in perfectly distinct waves; he did not dare to use his voice, in case he should knock down the walls, or burst himself like a bomb. His calculation of the time he enjoyed these dreams was about 300 years,—the fact being that only a quarter of an hour had elapsed.

CASE II.—Dr Christison describes the effects upon himself as follows:—"On trying Mr Robertson's extract once for toothache, I found that about 4 grains, taken about three A.M., caused in an hour cessation of pain, a pleasant numbness in the limbs, giddiness, a rapid succession of unassociated ideas, and impossibility to follow a train of thought, frequent intervals of sleep, and slight increase in the force of the pulse";—at the same time he felt no pain, while he was quite conscious the toothache was present. Next morning, there was an ordinary appetite, much torpidity, great defect and shortness of memory, extreme apparent protraction of time, but no peculiarity of articulation, or other effect; and these symptoms lasted till two P.M., when they ceased entirely in a few minutes after taking lemonade.

CASE III.—On the 3d April, at four P.M., a friend of my own took 2 grains of the Calcutta extract prepared by Mr Robertson. At a quarter past six, he felt as if weak, chiefly about the knees, with slight inclination to laugh, stupidity, and forgetfulness, but without reverie. He continued in this state till he retired to bed, when he slept soundly. Next day he was perhaps more stupid than before, but was enlivened by drinking lemonade. He was not exactly himself till the following day. His appetite was strong; but he was not affected in any other way.

CASE IV.—Another friend describes his sensations as follows:—I took 2 grains dissolved in spirit, and 1 grain in the solid form shortly afterwards. This was at four o'clock. At a quarter to six, when seated at dinner, and after taking a copious draught of water, I experienced a buzzing in my ears, with slight giddiness. Two minutes after, I burst into an immoderate fit of laughter without any cause, and was obliged to retire on account of repeated recurrence of the fits. My limbs became unable to support me; and, upon lying down, a variety of delightful dreams came over me,—the laughter continuing at intervals. At eight o'clock I got tea, and ate voraciously; after which I lay down, and recommenced laughing and singing. I slept soundly. Next day the symptoms continued to a moderate degree, and did not fairly leave me till the afternoon, previous to which time I got lemon-juice, which had great effect in restoring my faculties.

CASE V.—On the same day, I myself took 1 grain of the same extract dissolved in spirit; and though only half the quantity used

in Case III., the effects were much more apparent. At a quarter past five, when sitting down to dinner, I felt a peculiar numbness creeping through my body and limbs. Not imagining that this was the action of the cannabis, I began to fancy I was ill, so that I could not eat any dinner. On lying down, the numbness continued; but in fifteen minutes my sensations became agreeable. I laughed heartily several times, answered questions incoherently, and immediately forgot what they were about, and what I had answered. Delightful reveries came over me, and whatever I looked at became lost, as it were, in a maze: the lamp appeared to be slowly turning round; and when I lost sight of this, the red lines on the paper of the room appeared to intertwine in a most beautiful manner. The most remarkable effect was the constant succession of new ideas, each of which was almost instantly forgotten. When roused to tea, I ate ravenously, without feeling satisfied. I slept soundly at night afterwards; and next day was stupid and forgetful, but was much improved by drinking lemon-juice.

Case VI.—This case was remarkable for the intensity of the action of a single grain of extract, and for the remarkable hallucinations produced.

A highland porter, aged fifty, was admitted into the Edinburgh Royal Infirmary on the 4th December 1845, under the care of Dr Robertson, on account of attacks of chronic asthma and bronchitis. As sulphuric ether and laudanum produced little effect, it was resolved to give hemp extract in doses of a grain. The first and only pill was given at nine p.m., and at ten he called the nurse, "for he was about to die." He seemed to have awaked suddenly, and stared wildly about him. The clerk and apothecary found him conversing with great volubility, very happy to see them, and begging them to stay with him, "as he was at the point of death." His face was pale and anxious; pulse good; respiration unembarrassed. He informed them "that he had been transported to heaven," and his language, usually commonplace, was quite enthusiastic. Half a drachm of solution of muriate of morphia was administered, soon after which he became jocund, and like a drunk person; in a short time he fell into a sort of trance, and imagined he was conversing with the Deity, using two different tones of voice to express the conversation. He was now wakened, said he had seen visions, and seemed sensible. He then fell asleep, but was roused, as the respirations became very faint, and he exclaimed, "I have just come out of another fit." It was five a.m. before the intoxication ceased, and even at noon visit he was pale and tremulous; his asthma was, and continued, much relieved.

On the evening of the 6th, the hemp was intermitted for forty-eight hours, but resumed again, in ten-minim doses of the tincture every six hours. The first caused nausea and a want of feeling in the limbs; the pulse became slow, and he fell asleep, without delirium; but he said he dreamed much. These sensations were renewed at the second dose.

At 6 a.m. of the 7th, the usual asthmatic paroxysm was very mild. At noon, five minims had the same effect as the last. At his urgent solicitation, the medicine was for a few days discontinued. He frequently after this took five-minim doses, with great relief to his pectoral symptoms, and without the alarming effects induced by the larger quantity. He was dismissed, relieved.

The action of Indian hemp is liable to considerable uncertainty. This, perhaps, more than anything else, has led to distrust of it as a medicine. While on one person large doses have but little effect; on another, most violent and even alarming symptoms are induced by comparatively small doses; and though it is possible to explain such results, from peculiarity of constitution of the individual, still a doubt remains in what cases these unpleasant consequences may be looked for.

Professor Miller informs me, that he was occasioned considerable anxiety after giving a small dose of the tincture to a young lady. Soon after taking it, she complained of a sense of weight in the region of the heart, coldness of the surface, prostration, and the pulse at the same time very feeble. These symptoms, however, gradually passed off, and she recovered completely. A gentleman labouring under chronic pleurisy, a patient of Dr Christison, complained that small doses, although they made him drowsy, occasioned violent concussions in the brain, as if he was threatened with apoplexy.

In the "Monthly Journal" for November 1844, several cases are detailed by Dr Lawrie of Glasgow, where alarming symptoms presented themselves, but my present limits will not admit of their insertion. The individuals so affected were invariably females.

Similar cases have been met with occasionally by most of those who have used the hemp; these might be detailed here, but perhaps enough has been said on the subject to prevent any one from being taken by surprise by such occurrences.

Still I must insist, that these cases are only exceptional ones; that they are for the most part met with in persons of nervous or excitable constitution, particularly females; and that, therefore, the practitioner should not be deterred from administering hemp for this reason alone. It will not do, however, as a means of avoiding such consequences, to administer small doses. It appears to be much better to give full narcotic doses at once, by which means this stage of excitement will be as much as possible avoided.

The means of modifying or increasing the effect of the drug is a matter of considerable interest and importance.

Lemon juice is stated by various authors to have great influence in modifying the action. From my own observation, I am persuaded that it possesses this property to a high degree—that it will arrest the action entirely in many cases, but not during the intensity of the effects,—and that, where it does not arrest it, great modification will be produced. It is stated by Landerer, that at Cairo the

administration of an emetic of salt water is found effectual in removing at once the highest state of intoxication. On the other hand, among substances that increase the action, it is well known to the inhabitants of the East that the admixture of tobacco has a marked effect, while the practice of coffee-drinking is generally admitted to increase and sustain the action. The last observation on this head I have to make is, that some difference will be observed, according as the extract is given in the solid form, or dissolved in spirit. The action in the first case is gradual and less energetic; in the second, the effect is rapidly induced, and of more marked character.

An error is, I believe, often committed in giving small doses, when larger ones would lead more quickly to the sedative action of the drug.

THERAPEUTIC USES.

Notwithstanding the strong recommendations of Dr O'Shaughnessy, the therapeutic uses of hemp have been little studied hitherto in this country.

Dr O'Shaughnessy used it in the following diseases:—In three cases of rheumatism he found it apparently beneficial. In one of these, it seemed to produce great insensibility, and a state resembling catalepsy; but on this state passing off, the man was found to be thoroughly restored to health. In an epidemic of cholera, it was thought serviceable; but although it seemed to stimulate the circulation, and check diarrhœa, it is doubtful whether any ultimate good resulted. In a case of hydrophobia, a soothing effect, with diminution of the spasms, and greater facility in drinking, was kept up for four days; but the patient died. Several cases of tetanus were also treated by him in this way, with apparent success. In one, ascribed to cauterization of the hand, by a quack mixture of incandescent charcoal and tobacco, a state of intoxication was excited by large doses of the extract of hemp, and the spasms were gradually put an end to; but death ensued in the end from mortification of the hand. Another patient consumed 134 grains of the extract, and was ultimately discharged from the hospital cured. A third case, with similar results, is detailed. At the native hospital at Calcutta, Mr O'Brien treated seven cases of tetanus in this way, and in four of them he employed ten-grain doses. The result was, almost immediate relaxation of the muscles, and interruption of the convulsive tendency. Four of these cases recovered. A case in the practice of Mr Richard O'Shaughnessy is also detailed, where the disease was connected with suppurating wounds of the scrotum. The hemp had no effect for four days, and then the patient became tranquil, with fewer paroxysms, and the appetite good. When the hemp was intermitted, the symptoms became aggravated; latterly, the hemp caused much excitement, and was therefore discontinued. The last case is one of infantile convulsions, where very large doses

were given, and where the narcotic action greatly relieved the symptoms. The child recovered. This gentleman is confident that the resin is capable of arresting the progress of tetanus, and that, in a large proportion of cases, it will cure the disease.

It would certainly appear from the above facts, that Indian hemp has proved of service in the treatment of tetanus, as it occurs in India. How far this result has been obtained in Europe, I shall now describe.

That I may not extend my observations to too great a length, I shall limit my remarks to the treatment of tetanus, as observed in cases in private practice, and in the Edinburgh Royal Infirmary.

Professor Miller has provided me with the following remarks :—

"My own experience speaks loudly in favour of the hemp. I can now record three fortunate cases under its use—all traumatic tetanus—and a case which proved fatal, but where great alleviation of suffering was produced.

"The first of these was a girl, aged 7, admitted to the Royal Infirmary October 18, 1844. She had received an extensive injury of the middle finger of the right hand a fortnight previously. Inflammatory swelling and pain became intense, and there was a tendency to spasmodic flexion of the fingers and wrist. On the 23d she was observed by the nurse to take a 'kind of fit,' becoming rigid, having difficulty in opening the mouth and in swallowing, and complaining of pain in the jaws. At visit, she seemed perfectly well. A brisk purge was ordered, and, lest the case should prove tetanus, ten drops of tincture of hemp were prescribed to be taken every four hours. Next day the symptoms were well marked, without any influence from the hemp. The finger was then removed, and the simplest dressing applied to the wound. The dose of hemp was increased to 20 drops, and after five doses, she slept; but the following day the symptoms were aggravated. Turpentine enema was ordered, and ice to the spine—30 drops of cannabis to be given hourly. In the evening there was rigidity, but no spasm; the hemp to be given every half-hour; after which she became drowsy, and at 12 next day she was much improved. Aconite was now substituted; but as the spasmodic attacks became more severe, hemp was again given, with the effect of producing sleep. She continued to improve till the 25th November, the dose of hemp being gradually reduced; producing, when given, drowsiness, or calm sleep; it was soon discontinued, as it then seemed to excite the circulation. Throughout the whole period of its use, its effect on the appetite was most obvious, the craving for food being at times absolutely voracious. After this no more medicine was given, and recovery was complete.

"The second case, occurring in private practice, was that of a boy, about the same age, who had simple fracture of the thigh, with compound and comminuted fracture of the great toe. The treatment and result were the same.

"The third was a boy, rather older, who had compound fracture of the bones of the arm. Treatment again resulted in cure.

"In these cases a few doses generally induced sleep, with marked mitigation of the spasms. The period of narcotism did not exceed two or three hours; the sleep was deep and unbroken, and seemed to be refreshing. It was followed by no headach, or other apparent inconvenience. The most remarkable effect observed, was the tolerance of the remedy, whereby a girl, aged 7, took every half-hour, and sometimes many hours in succession, doses of hemp sufficient to narcotise an adult."

In these cases, Mr Miller is inclined to give the hemp credit for a chief share in the cure.

In 1846 the virtues of hemp were tested in a case of tetanus in the Royal Infirmary, in the wards of Dr Duncan. In 1847 another case presented itself, where hemp was administered. At that time sulphuric ether was much used as an anæsthetic, and it was thought probable that it would be of service in this case. The patient inhaled it at frequent intervals during a whole afternoon, with decided, but only temporary, relief. After this cannabis was given, without its physiological action being attained by nearly an ounce and a half of the tincture; it was not persevered with. Ether was again tried, and also opiates, with some benefit. The patient died on the 13th day.

The first of these cases was very accurately observed, and the following report of the case from the journal will be found to have considerable interest:—

James Mackay, a railway labourer, was admitted under the care of Dr Duncan, October 20, 1846. He had received a slight lacerated wound of the hand a week before, and tetanus had commenced on his admission. The wound appeared to be healing. He complained of great general uneasiness, particularly about the neck and spine, of some rigidity of the jaws, which could only be separated three-quarters of an inch, of inability to protrude the tongue, and of commencing spasm of the neck and upper part of the back. He complained also of a "burning about the heart." His expression was anxious, with but little "risus." His thirst was great, but swallowing difficult. He perspired profusely. The spasms, of short duration, recurred once or twice every minute; pulse, 115 to 120, soft. Opening medicine was ordered, and at 11 o'clock tincture of hemp was given, repeated in doses of 15 or 20 drops, with appreciable effect. On the 21st the bowels were not opened, though a turpentine enema was administered. The spasms were more violent and general, and a touch caused general spasm. He had not slept; 120 to 140 drops had no effect. The doses were increased to 60 or 80 drops every three-quarters of an hour, and croton oil was given, producing free action on the bowels; and in the evening the spasms abated,

but the hemp caused only slight dozing at intervals. The tincture was ordered to be continued, and strong beef-tea to be drunk.

On the 22d swallowing was easier, the spasms less violent, but not less frequent; 100 drops were given at half-past 11, and continued about every half-hour till 4 o'clock, when drowsiness was quite decided; he was not readily roused, even by the spasms which, though as frequent, were not so intense. At 9 o'clock drowsiness was passing off; copious stools, coloured as by the medicine, were brought away by injection; 130 drops were given, and repeated at midnight, at which time he was much relieved, but suffered from cough. On the 23d the spasms were again gaining strength, no hemp having been given for nine hours. A drachm of the tincture was given, and repeated at 11, when he became quiet. The doses were continued till evening, when he took mince-collops and beef-tea without difficulty, and the bowels were copiously relieved.

On the 24th, at visit, the spasms were absent, but the chest symptoms were worse, with general mucous râle, and frothy sputa mixed with blood. Drowsiness had been kept up by doses of a drachm to a drachm and a half. In the evening he was much weaker, but quite sensible, with a desire for food. On the 25th he was perfectly free from spasm, but was evidently dying, from accumulation of mucus in the chest. Very little hemp was given. He died at 8 p.m.

In this case, six ounces of O'Shaughnessy's tincture of Indian hemp were given in all, being equal to 144 grains of the extract. The extract for the tincture was reputed the best in Edinburgh. The doses at first were evidently too small. The examination of the body was not permitted.

It is a safe conclusion, from these facts, that Indian hemp deserves further trial in the tetanus of Europe, as well as in that of hot climates. I would particularly urge, however, the necessity, in all such trials, of making certain, by experiment on healthy persons, that the preparation to be used is good. For the present, there is no other satisfactory test of quality.

As to the use of hemp as a calmative and hypnotic in diseases in general, I may mention that, while acting as clinical clerk in the Royal Infirmary in 1849, I had several opportunities of administering hemp in different diseases as a hypnotic. The object was in general attained, and no evil results followed. I regret there is no record of these cases, as at the time I did not pay particular attention to the subject. Hemp is frequently given in the other wards of the Infirmary for a like purpose. In cases of phthisis and other lingering diseases, where opiates have for a long time been administered, but have ceased to produce sleep, Indian hemp may often be given with advantage; thus, in one case of advanced phthisis, doses of five to ten drops of the tincture were successful in procuring sleep when other means had failed.

Dr Christison has administered hemp in many instances. He gives the following account of two of them :—

A gentleman had suffered from palpitation of the heart for twenty-one years, and at night the attacks were generally most severe. He had used one medicine after another with the hope of relief, but he did not derive any benefit. Dr Christison advised him to try Indian hemp. The patient's wife states that he passed the night on taking it without suffering from the palpitation, though still he was perfectly conscious of its presence; and that the attack left him entirely at 8 A.M., instead of continuing twenty-four hours, as previously it did.

In the other case, a gentleman was afflicted with severe eczema over the whole body, with intense itching. A large dose of solution of the muriate of morphia caused extreme sleepiness, but so much increased the itching that he was kept awake by the necessity of scratching. Twenty-five drops of cannabis tincture gave him six hours' sleep, and he continued to enjoy sleep from four to six hours every night for six weeks, without increasing the dose, until the eruption was nearly removed; during all this time the itchiness continued as before when he was awake.

Dr Christison has observed that, in the generality of cases, hemp has had the effect of causing sleep without disturbing the function of the stomach or bowels. Given where morphia and hyoscyamus had failed, it has also repeatedly failed to cause sleep; but in one or two cases he has found it to succeed where morphia and opium disagreed.

An interesting series of cases by Mr Donovan will be found in the "Dublin Journal of Medical and Chemical Science" for 1845. This gentleman was convinced of the beneficial effects of hemp, particularly in cases of neuralgia. Mr Donovan had himself suffered occasionally since early life from neuralgic pain of different parts of the foot, lasting one or two days, or sometimes a week. Immersion in cold water gave entire relief, but no other treatment did so, till he took five drachms of weak tincture of hemp; in twenty minutes the pain was gone; at the same time, "he had hardly any consciousness of the motion of his limbs when walking—they appeared not to belong to him." On another occasion, he took six drachms without effect; but on the third night, after taking twelve grains of weak extract, he was free from pain, and slept four hours; and in several other attacks he derived similar benefit.

A gentleman was attacked on going to bed with excruciating pain in the left upper jaw, which kept him awake till morning; after a short sleep, he awoke in torture, and in the evening, upon taking his third dose of fifteen minims of weak tincture of hemp, he slept profoundly till eight next morning, when the pain was much abated. At night he repeated the remedy, with similar results, and next evening he took twenty minims, which deadened the pain; but it soon became as bad as ever. Embrocations of laudanum and

camphor spirit were then tried, with another dose of twenty minims, and he immediately fell asleep; in the morning the pain was nearly gone, and it soon disappeared.

Another gentleman had excruciating sciatica for thirteen weeks; his sufferings caused groans, cries, and tears, and he passed sleepless nights. The only relief he obtained was from firm pressure on the hips, and, for a short time, from laudanum. Two doses of hemp, at short intervals, produced sound sleep for eight hours, and on awaking he was perfectly relieved. Five doses more so completely subdued the pain that it gave little farther trouble. He experienced a slight but transitory return on entering a cold room.

A number of other cases will be found in Mr Donovan's paper, in which the hemp, if it did not effect a cure, yet was of great service in the treatment of the complaints to which he alludes; but in several cases no good followed, and, on the contrary, unpleasant effects were produced. Thus, a lady suffering from neuralgia of various parts of the body was ordered five drops of strong tincture at night; next morning she was giddy and weak, and, without authority, took five drops more. She became faint and universally cold, had some apprehension of death, and remained disagreeably affected during the whole day; the pain was not relieved, and the effects of the hemp reappeared at intervals for two or three days.

Another patient, who was accustomed to take hemp, on one occasion had alarming depressing symptoms; he sat, greatly agitated, with his eyes open, and his head reclining on his chest. The respiration was tremulous, with interruptions of sobbing; his whole frame was in an indescribable shudder, and he seemed to shiver with cold. The pulse was good all the time, and in half an hour he recovered.

Indian hemp in different forms has been recommended, principally by the older writers, for several other purposes, as in the treatment of diarrhœa, gonorrhœa, and locally as an anodyne lotion, or in the form of poultice for hæmorrhoids. For these purposes, I am not aware that it is now used; but there is one affection where it has lately been applied with advantage — viz., uterine hæmorrhage. Dr Churchill says ("Diseases peculiar to Women," Ed., 1849)—"We possess two remedies for these excessive discharges, at the time of the menses going off, which were not known to Fothergill—ergot of rye, and tincture of Indian hemp. The former has been long known to possess the power of restraining hæmorrhage after delivery, &c., but the property of hemp of restraining uterine hæmorrhage has only been known to the profession a year or two. It was accidentally discovered by my friend, Dr Maguire of Castleknock, and since then it has been extensively tried by different medical men in Dublin, and by myself with considerable success. The tincture of the resin is the most efficacious preparation, and it may be given in doses of from five to fifteen or twenty drops three times a-day, in water. Its effects, in many cases, are

very marked, often instantaneous, but generally complete after three or four doses. In some few cases of ulceration, in which I have tried it on account of the hæmorrhage, it seemed to be equally beneficial."

These effects seem to me to be allied to the action of hemp on uterine contraction during labour, to the consideration of which subject I shall next proceed.

(*To be continued.*)

Part Second.

REVIEWS.

Souvenirs d' Un Voyage dans la Tartarie, le Thibet et la Chine pendant les Années, 1844-45-46. Par M. Huc, Prêtre-Missionaire de la Congrégation de St Lazare. Paris. 1850.

We are hardly entitled to notice this book in our pages. It is not a medical work, and the reviewing of a narrative of travels, when not professedly scientific, belongs rather to the literary than the medical journals. It is, in fact, not our object to review it. We may shortly say, that, having been much interested by the work before us, we commend it to all those who can enjoy a pleasant book, written in rather humorous and very expressive French, and who have sufficient catholicity to enable them to admire the zeal and self-devotion of a missionary, though his faith may be that to which they cannot accord their approbation.

Without any formal criticism, then, we venture to lay before our readers a translation of one passage, which gives a graphic, and withal curious, account of how to cure an intermittent. Here is M. Huc's illustration of the practice of medicine in Tartary :—

" The profession of medicine is exercised in Tartary exclusively by the lamas (Buddhist priests). As soon as disease shows itself in a family, they run to the nearest *lamasérie* to call a physician. He repairs to the patient, and commences by feeling his pulse; he takes, at the same time, a fist of the patient in each of his hands, and moves his finger upon the arteries somewhat in the way that the fingers of a musician are moved upon the cords of a violin. The Chinese manner of feeling the pulse differs from the above, the Chinese doctors feeling the pulse successively in the two arms, and not in both at once. When the lama has sufficiently studied the nature of the disease, he pronounces his opinion. As, according to the religious belief of the Tartars, it is always a *tchutgour*, or demon, who torments the affected part by his presence, it is necessary, as a preliminary, to prepare, by medical treatment, for the expulsion of this devil. The lama doctor is also, at the same time, apothecary; mineral

chemicals play no part in the preparation of the specifics employed by the lamas. Their remedies are always composed of powdered vegetables, which they infuse, or make up in the solid state, and roll into the form of pills. When the little store of the vegetable pills is exhausted, the lama doctor is no way disconcerted on that account; he inscribes on some little scraps of paper, in Thibetian characters, the name of the remedies, then he rolls the paper between his fingers, after moistening it with saliva; the patient takes these little boluses with as much confidence as if they were true pills. To swallow the name of the remedy, or the remedy itself, say the Tartars, comes essentially to the same thing in the end.

"After the medical treatment employed to facilitate the expulsion of the devil, the lama orders prayers conformable to the quality of the devil who has to be dislodged. If the patient is poor, the *tchutgour* is evidently small; and, in that case, the prayers are short, and of little solemnity; sometimes they limit themselves to a simple formula of exorcism; often, indeed, the lama contents himself with saying, that there is no need either of pills or prayers; that they must wait with resignation till the patient either recovers or dies, according to the decree pronounced by *Hormoustha*. But if the patient is rich, if he is the possessor of numerous flocks, things take a very different course. In the first place, they must make up their minds that the demon whose presence has given rise to the disease is a powerful and terrible devil—without doubt, he is one of the chief of the evil spirits; and, as it is not decent for a great *tchutgour* to travel like a mere imp, they must prepare for him fine clothes, a fine hat, a fine pair of boots, and, above all, a young and vigorous horse;—if all this is not forthcoming, it is certain the devil will not go away; it would be vain to administer remedies and repeat prayers. It may even happen that one horse is not sufficient; for sometimes the devil is so exalted in point of dignity, that he brings in his suite a great number of servants and courtiers; in that case, the number of horses which the lama exacts is unlimited,—that depends always on the greater or less wealth of the patient.

"Everything being arranged conformable with the programme drawn out by the doctor, the ceremony commences. They invite several lamas from the neighbouring lamasérie, and the prayers are continued for a week or a fortnight, until the lamas perceive that the devil is no longer there,—that is, as long as they choose to live at the expense of the family whose tea and sheep they consume. If, when all is done, the patient happens to die, there is then the most certain proof that the prayers have been well recited, and that the devil has been put to flight. It is true that the patient is dead; but he may be no loser for that. The lamas assure them that he will transmigrate into a state of greater fortune than that which he has just quitted.

"The prayers which the lamas repeat for the cure of diseases are sometimes accompanied by lugubrious and frightful ceremonies. M. Huc, being in charge of the little christian community of the *Valley of the Black Waters*, had the opportunity of making the acquaintance of a Mongolian family, which he visited from time to time, that he might learn the customs and language of the Mongoles. One day the old aunt of the noble *Tokoura*, the head of this family, was seized with intermittent fever. 'I would at once call the lama doctor,' said Tokoura; 'but, if he declares that there is a *tchutgour* in the case, what will become of me? The expense will ruin me.' After delaying some days to call the doctor, his anticipations were not unfulfilled. The lama announced that the devil was there, and that he must be got rid of as quickly as possible. The preparations were then made with the greatest activity. That evening eight lamas arrived, and set about fashioning with dried grass a large manikin, which they named the devil of intermittent fevers; and, by means of a stake which they had stuck in between his legs, they made it stand upright in the tent where the patient was.

The ceremony commenced at eleven o'clock at night. The lamas arranged themselves in a circle at the further end of the tent, armed with cymbals,

conch-shells, bells, tambourines, and divers instruments of their obstreporous music. The circle was completed towards the front of the tent by the Tartars of the family to the number of nine; they were all squatted on the ground, and pressed closely upon one another. The old woman, on her knees, or rather sitting on her heels, was placed fronting the manikin who represented the devil of the fevers. The lama doctor had before him a large copper basin, filled with small millet and some little figures, made of flour paste. Some lighted argols[1] threw a fantastic and flickering glimmer over this strange scene.

"At a given signal, they executed a musical overture capable of terrifying the most intrepid of devils. The *black men*[2] beat their hands in cadence, as an accompaniment to the clattering sound of the instruments and the howling of the prayers. When this infernal music was terminated, the grand lama opened the book of exorcisms, which he placed upon his knees. As he chanted, he drew from the copper basin some grains of the millet, and threw them here and there about him, according as it was marked in his rubric. The grand lama generally prayed alone, sometimes in a lugubrious and stifled tone, sometimes with long and loud outbursts of voice. Sometimes he abandoned the cadenced and rhythmic style of praying; he appeared then to break all at once into a violent fit of rage, uttering brisk and animated summonses, which he addressed with gesticulation to the straw manikin. After these terrible exorcisms, he gave a signal by stretching out his two arms right and left. All the lamas shouted out immediately a rattling chorus in a precipitate and rapid tone; all the instruments of music were in full play; the people of the family rushed out suddenly, one after the other in a line, running round about the tent, which they struck violently with cudgels, whilst they uttered yells which made our hair stand on end. After having three times performed this diabolical rondo, the string of people re-entered the tent precipitately, and each one resumed his place. After this, whilst the assistants covered their faces with their hands, the grand lama rose to set fire to the manikin. As soon as the flame began to arise, he uttered a loud cry, which was repeated by all the other voices. The 'black men' took hold of the burning devil, and, rushing out with it, carried it to the fields, to a distance from the tent. Whilst the *tchutgour* of intermittent fevers was burning away, amidst cries and imprecations, the lamas remained squatted in the interior of the tent, and chanted their prayers in an agreeable, grave, and solemn tone.

"When the people of the family returned from their courageous expedition, the chants ceased, and gave way to joyous exclamations, interlarded with loud bursts of laughter. Presently the whole party issued tumultuously from the tent, and each one taking a lighted torch in hand, they marched off: the 'black men' went first; then came the old woman with the fever, supported under the arms by two members of the family; behind her marched the eight lamas, who made the air resound with their hideous music. They then conducted the old woman into a neighbouring tent; for the doctor lama had decided that for one whole month she must not return to her old abode.

"After this comical treatment, the patient was entirely cured, for the attacks of fever did not recur. As the period for the recurrence of the paroxysm was exactly the hour when this infernal performance commenced, it is probable that the fever had been cut short by the violent excitement occasioned by this, the most fearful and fantastic scene which can be imagined."—Tom. i., p. 107.

The pathology and therapeutics of intermittent disease still form one of the mysteries of physic. That there must exist in the body, when under the influence of intermittent morbid action, a chain of cir-

---

[1] The Tartars give the name of argols to the dung of animals, when it is dry and fit for fuel.

[2] The Tartars give the name of hava houmou (black men) to laymen, probably on account of their hair, which they allow to grow. This is in contradistinction to the white heads of the lamas, who are obliged to shave the scalp.

cumstances which determines the periodicity, is obvious; and that this chain can be broken in various ways, perhaps at various points, is equally evident from the very different nature of the agents which have the property of acting as anti-periodics; but what the links of this chain are, and how they are snapped by remedies of such regularly diverse nature, is what nobody has yet been able to explain to us, and what we do not profess to be able to explain to anybody. That anti-periodics operate through the nervous system, is a favourite doctrine, and, so far as it goes, is probably a correct theory, and is supported by instances, such as that recorded by M. Huc, where powerful mental emotion has appeared to check the course of periodic disease. We wish that he had told us how long the above old lady remained unmolested by her *tchutgour*. We do not believe that her disease was eradicated by the orgies of the lamas; but, at all events, it was arrested, and that this could be in no other way than by a mental impression, as M. Huc supposes, will, we think, be readily admitted. A case in point, illustrating the arrestment of intermittent fever by mental excitement, was narrated to us by the medical officer who attended the patient. A brigade-major in the Portuguese army under the Duke of Wellington, had, during a series of active operations which preceded the battle of Vittoria, been severely affected with tertian. The night before the fight, he was delirious in the hot fit, and on the morning of the action, he was so weak as to require to be supported when on his horse. Mental excitement, however, got the better of bodily weakness. The doctor left the major with instructions to go to the rear, and thought he had gone, but not long after, to his astonishment, he saw his patient gallantly leading his men into action. He bore his full share of the fatigues and dangers of the day, escaped unhurt, and had no return whatever of his intermittent, till his division went into quarters, which was not till after a lapse of several weeks.

Having given M. Huc's illustration of the practice of physic in Tartary, we shall quote one short passage regarding the state of medicine in Thibet:—

"The Thibetian doctors are as empirical as those of other countries, perhaps even a little more so. They assign to the human body 440 diseases, neither more nor less. The books which the lamas of the faculty of medicine are obliged to study, treat of these 440 diseases; they describe their characters, their diagnosis, and their means of cure. The books consist of a collection of aphorisms, more or less obscure, with a host of special recipes. The lamas have not so great a horror at blood-letting as the Chinese doctors; they sometimes use blood-letting, and frequently employ cupping. For this last operation they first subject the skin to some slight scratches; then they apply over it cow's horns pierced at the top. They suck out the air with the mouth, and when they have got a good vacuum, they close the hole by applying over it with their tongue a piece of chewed paper, which they keep in reserve in their mouth; when they wish to remove the cupping horn, they have only to remove this luting.

The lama doctors attach much importance to the examination of the urine of the patient. They require several samples of it, collected at different periods of the day. They examine it with the most minute attention, and take accurate notice of all the changes in colour which it undergoes. They beat it several

times with a wooden spatula, and then apply the vessel to the ear, to listen to the sound which it makes, for they suppose that, according to the condition of the patient, the urine is sometimes *dumb*, sometimes *speaking*. A lama doctor, to be accounted clever, and well versed in his business, must be capable of curing a patient without having seen him. The inspection of the urine solely, ought to suffice for directing the prescription."—Tom. ii., p. 179.

In reading these extracts from M. Huc's book, we are of course at once led to compare the miserable state of these countries as regards medicine, with the enlightened and advanced condition of physic in our own land; and, with the pharisee's self-complacency, we are thankful that we are not like these Tartars and Thibetians. But before we sneer at them, let us look at home, and see whether there are not among us absurdities fully comparable with those which exist in Tartary and Thibet. "Oh," we hear some indignant patriot exclaim, "we don't believe in the Grand Lama and Tchutgours." True, not exactly; but sometimes we believe in Reichenbach and odyle, and what great odds are there between the two spirits, except that the tchutgour is perhaps the less irrational one? "But we don't swallow a bit of paper with the name of a drug upon it, and believe that it answers as well as taking the physic itself." True, not exactly; but what are the homœopathic globules which we sometimes confide in, but swallowing "the name of the thing" instead of the thing itself? It is admitted that the deglutition of a globule of sugar of milk is less disgusting than that of dirty paper insalivated by a lousy lama (for lousy these gentry are, if M. Huc's account of them is true); but, in point of absurdity, is the Tartarian practice a bit worse than the Hahnemannian? "But we don't make a manikin of dried grass, set fire to it, and think that we are curing disease in that way." True, not exactly; but what is the spermatorrhœa with which so many dissipated men are made to believe themselves affected, but, in nine cases out of ten, or more, a man of straw set up by the doctor in the mind of the patient? And, as if to make the parallel complete, have we not the burning also; not, to be sure, with actual fire, at the risk of consuming the habitation, but with active caustic, at the risk of destroying the urethra? Verily, John Bull, you ought to look at home; don't sneer at Tartars and Thibetians—

"——Quid rides? mutato nomine de te
Fabula narratur."

See how you are befooled with quackeries of all sorts, and have a care lest, when you are congratulating yourself on being out of the clutches of the lama, you do not catch a tartar after all.

But there is one reflection suggested to us by M. Huc's book, which we must approach in a more serious style. We have been forcibly struck with the great advantage which might have accrued to him and his brother missionaries, had a medical man formed one of the party. We regret, in the first place, the excellent opportunities which have here been lost of gaining for science some valu-

able information. They travelled through lands, the natural products of which are very little known to us, and yet they do nothing more for us than to tantalise us by occasional glimpses at interesting matters, which they do not care, or are not able, to investigate. They were in a valley, the inhabitants of which were entirely devoted to the chase of the musk—yet they give us no information as to the habits or mode of capture of this interesting animal, which they might easily have learned. They must have been at or near localities from which some of the rhubarb of commerce comes, and yet they never name this important drug, of whose history we know nothing. They tell us of a root, sweet, feculent, abundant, and needing little culture, and yet of this, which appears to be a valuable esculent, and might be an important addition to our means of sustenance, they tell us nothing but that it exists. On all these points, a medical man could hardly have failed to bring us some useful knowledge. But beyond all this, we regret the opportunities which they lost of strengthening the very cause in which they were embarked; and we rise from the perusal of their narrative with a doubly strengthened conviction, that medical missionaries are yet destined to be among the most efficient workmen in this great field of enterprise. We cannot peruse the passage which we have quoted, with its graphic account of the diabolical orgies, without feeling how little effect would the mere preaching of the ablest priest, Protestant or Catholic, however zealous, have upon so deeply-rooted a system of superstition; and we hesitate not to affirm, that in the instance of the old woman with the ague, so far as human means could go, a few drams of quinine, skilfully applied, would have proved by far the most trenchant weapon for cutting down Buddhism, and uprooting those "doctrines of devils" to which those priest-ridden nations are "giving heed."

Traité Pratique des Maladies Cancéreuses, et des Affections Curables Confondues avec le Cancer. Par H. LEBERT, M.D., &c. &c. 8vo. Pp. 892. Paris, 1851.

Practical Treatise on Cancerous Diseases, and on the Curable Disorders Confounded with Cancer. By H. LEBERT, &c. 8vo. Pp. 892. Paris, 1851.

WE were the first in this country to recognise the merit and general exactitude of M. Lebert's researches, and this at a time when an unacquaintance with the histology of morbid growths led an influential contemporary to regard them as false or imaginary (*Monthly Journal*, and *British and Foreign Review*, July 1846). But as we predicted, the "Physiologie pathologique" of M. Lebert justly led him to be considered as one of the first pathologists of the day; and to him the medical school of Paris is mainly indebted for its rational

progress in recent times. During the last five years M. Lebert has extended his observations, and rendered them more precise and systematic. In addition to various isolated memoirs, he has published a surgical work in German, comprising more extended observations on tumours (*Abhandlungen aus dem Gebiete der Praktischen Chirurgie*, 1848.) That portion of his original treatise which referred to scrofula and tubercle, has also received many additions, and been republished separately (*Traité Pratique des Maladies Scrofuleuses et Tuberculeuses*, Paris, 8vo, pp. 811, 1849). This most excellent monograph obtained the Portal prize of the National Academy of Medicine. Lastly, there has just appeared the work the title of which is placed at the head of this article, being an extension, with many additions, of his previous treatise on the same subject, and which, as far as the special consideration of cancer is concerned, must be regarded as the most complete that has yet been published.

The treatise is divided into two parts. The first contains the general history of cancer, comprised in four chapters. 1. The definition, physical, microscopic, and chemical characters of cancer. 2. The unity, development, propagation, and frequency of cancer and cancroid. 3. General pathology. 4. Hygienic, medical, and surgical treatment. The second part contains the special history of cancer and the diseases confounded with it, and consists of ten chapters. 1. Cancer of the generative organs. 2. Of the digestive organs. 3. Of cancer and cancroid of the skin. This portion of the work has been published as a separate memoir in the "Mémoires de la Societé de Chirurgie," and deserves very careful study. 4. Cancer of the subcutaneous cellular tissue. 5. Of the superficial glands. 6. Of the osseous system. 7. Cancer and disorders confounded with cancer in the nervous centres. 8. Cancer of the eye. 9. Of the urinary passages. 10. Of the organs of circulation and of respiration.

It is not our intention to follow M. Lebert through the extensive field he has travelled over. Such a course in a limited article is obviously impossible. We propose dwelling only on one or two points which have excited considerable discussion. In the present state of science, the question perhaps that practitioners are most ready to ask is, What has the microscope really accomplished? Has the minute study of the structure of morbid growths enabled us to detect cancerous and incurable from non-cancerous and curable tumours more readily than before? In reply to this latter question, all modern pathologists are agreed, and answer in the affirmative. But when they are asked to point out by what element or elements they are capable of doing this, then a slight discrepancy is observable in their writings,—a discrepancy, however, which we believe to be more apparent than real, and which we shall endeavour to get rid of.

In his "Physiological Pathology," vol. ii., p. 254, M. Lebert thus expresses himself:—"The cancerous globule has decided characters which distinguish it from every other kind of morbid production. This proposition is repeated in the same volume (p. 426), in the

following words:—"The cancerous globule is the part which distinguishes cancerous tumours from all other morbid productions." Now if the cancer-cell really had characters so distinctive, nothing of course could be easier than to determine the nature of cancer. But it is denied by other pathologists that such is really the case; and it is especially pointed out by Dr Bennett (*On Cancerous and Cancroid Growths*, p. 149, *et seq.*), that isolated epithelial, cartilage, and fibro-plastic cells may, under certain circumstances, so closely resemble cancerous ones as not to be distinguished from them. Now if this latter proposition be true, the value of M. Lebert's statement at once vanishes when it is most required. On the other hand, it has been contended that groups of cancer-cells can, in the majority of cases, enable us to form a correct conclusion, especially when attention is paid (as should invariably be done) to their relation to the accompanying elements of the growth. These and similar observations seem to have made an impression upon M. Lebert; for without acknowledging a change of opinion, he now expresses himself as follows, in the work before us, p. 16:—"An isolated cell being given, can we always recognise, by microscopic examination, if it belong to a cancer or no? We do not hesitate to answer *in the negative*. But the question we have always endeavoured to resolve is this,—a morbid tissue being given, can we recognise, by means of a microscopic inspection, if it be cancerous or not? On this point we do not hesitate to answer in the affirmative."

It follows, therefore, that M. Lebert has greatly modified his views on this essential point. He no longer attaches the same importance to the cancer-cell as a means of diagnosis; but considers this element, in conjunction with those others which make up the morbid tissue, to constitute the only foundation for a sound anatomical ground of diagnosis. Whilst, then, M. Lebert is now brought to agree in the accuracy of Dr Bennett's opinion, he accuses the latter pathologist of holding a view which he never dreamt of, and enters into a laboured argument to show (pp. 32, 33) that he, as well as Vogel and Virchow, deny the specificity of the cancer-cell. In other words, he has confounded together two very different questions,—viz, the diagnostic character of a cancer-cell; and the theoretical opinion as to whether one cell can be transformed into another. But even here he has mistaken Dr Bennett's language, who distinctly says, after arguing this question, "It may therefore be doubted whether the true cancer-cell *be ever* formed by transformation of a previously existing one." (*Op. Cit.* p. 149.)

It results, however, from this criticism, that notwithstanding the confusion pathologists are led into from differences in modes of expression, and from incorrectly appreciating the idioms of a language with which they are not very familiar, that the discrepancies existing in various works published in England, France, and Germany, are, as we have previously seen, more apparent than real: more verbal than essential. Indeed, there can be no doubt that the

results of microscopic observation are now tolerably uniform amongst accurate histologists in all countries; and that the microscope is daily becoming more necessary, not merely as an instrument of scientific research, but as a means of diagnosis.

Another point of importance in the present state of our knowledge is, what we should understand by the term "cancroid." This term has been extended to all growths which are capable of being mistaken for cancer. In such a sense it is evidently provisional, inasmuch as no sooner do our means of distinguishing these become more certain, than they cease to resemble cancer. This applies to fibrous, sarcomatous, tubercular, and other growths, which are still too frequently confounded with cancer by surgeons. On the other hand, we conceive it to have been distinctly proved that there is a class of growths essentially differing from cancer in structure, and yet which has a tendency to return, to attack the lymphatic glands secondarily, to soften, ulcerate and kill; in short, to present all the characters usually grouped together under the term "malignant." To this class belong certain epithelial, cartilaginous and fibro-nucleated growths, and probably other forms which have not yet been described.

It is in the accurate description and study of this form of growths that we consider much is yet to be done in order to complete our knowledge of cancer, and of the disorders which may be confounded with it. To M. Lebert, science is greatly indebted for his labours; but much more is yet to be accomplished. If the young histologist, but especially the house-surgeons of our large hospitals, would first learn how to examine accurately, and then record in a methodical manner the results of their observations, we should soon possess sufficient data to resolve many important questions connected with the nature and treatment of morbid growths. We are happy to see that this plan is followed out by some of the clerks in the Edinburgh Infirmary, who are in this way not only advancing the cause of science, but laying up for themselves a solid foundation for future eminence in their profession. On some other occasion we propose to communicate the result of their investigations.

Part Third.

CLINICAL REPORTS, LECTURES, ETC

CLINICAL SURGERY—PROFESSOR SYME.

Gentlemen,—I regret my absence for the last ten days, but hope that the information acquired since I saw you, will conduce to the object we all have in view. I have long suspected, and am now fully satisfied, that neither Paris nor

London possesses any hospital which affords so good a field for surgical observation as the Royal Infirmary of Edinburgh. The knowledge of this fact should stimulate your exertions, and prevent them from being relaxed through the vain expectation of elsewhere finding a better opportunity for practical study, while it greatly encourages me to cultivate with redoubled energy the valuable means of instruction placed within my reach.

DIVISION OF STRICTURE.

The first case I may notice to-day is that of A. S., aged 33, who was sent here little more than a fortnight ago from Liverpool, labouring under what seemed to be an incurable stricture. He had suffered from it for upwards of fifteen years, and passed a large portion of this period in various hospitals, including three months in that of University College, under the care of Mr Liston. Finally the stricture had become tighter than ever, and a fistulous opening in the perineum allowed the greater part of his urine to escape. I divided this stricture in your presence, under chloroform, so that there was no pain, and without the loss of more blood, either at the time of the operation or after it, than at the very most two teaspoonfuls. Fourteen days have elapsed since the catheter was taken out; and now, you remark, the patient considers himself perfectly well—making his water with ease in a full stream, and having ceased to pass any by the fistulous opening. He goes home to-morrow, with instructions to introduce a full-sized bougie occasionally.

While the most false and misleading statements have been, and I regret to say still are, published, in regard to the treatment of stricture by external incision, so as to delay the general reception of this procedure, you possess the great advantage of being able to judge from your own observation, and determine without any risk of mistake, how far the operation in question merits the confidence which I have expressed in it, or the abuse which has been thrown upon it by others. The privilege which you thus enjoy imposes an obligation on your part to assist in diffusing the truth on this subject, not by embarking on the stormy sea of controversy, but by frankly and clearly stating what you have witnessed whenever you have a fair opportunity of doing so. It is thus, and thus alone, that the force of prejudice can be overcome.

EXCISION OF THE ELBOW-JOINT.

The female from whose arm I, yesterday fortnight, removed the articulating extremities of the bones which form the elbow-joint, is now, you may observe, nearly well. The operation, you recollect, was performed, not on account of existing, but to remedy the effects of former disease, which had been caused by an injury two years ago, and occasioned anchylosis; the arm was consequently rendered perfectly rigid in the straight position, so as to be nearly useless for the support of the patient, who was therefore very desirous of having something done to restore the power of motion. In these circumstances I undertook the operation, as a warrantable measure, but still only as an experiment; since, although I have performed excision of the elbow-joint for caries upwards of eighty times, my experience of it for the remedy of anchylosis has been too limited for the induction of any certain conclusions in regard to the resulting effect. The procedure, as you would remark, was much more difficult and laborious than on the other occasions in which you have witnessed its performance; but, so far as we can judge, promises to be completely successful. The patient has not suffered the slightest constitutional disturbance, the wound is nearly healed, and the elbow admits of free motion.

There is at present in the hospital another case requiring this operation, the patient being a man, 37 years of age, from Perthshire, who has suffered very severely from caries of the elbow-joint. The limb, you may observe, is greatly swelled, with numerous openings, which discharge matter profusely, and allow the probe to reach diseased bone. At the elbow there is also that distinct mobility in a lateral direction, which affords the most unequivocal evidence of articular disorga-

nisation. The disease commenced three years ago, with the symptoms of ulceration in the cartilage, and advanced to suppuration six months ago.

In performing the operation to-morrow, I shall proceed as usual by making an incision of the form H, to obtain free access to the joint; and in dressing the wound, shall take special care to favour union by the first intention between the edges of the transverse part of it. The resulting condition of the limb depends so much upon the absence of a cicatrix in the situation of the olecranon, which must of course materially interfere with the freedom of motion, that I am surprised doubts should ever have been entertained as to the inexpediency of cutting in any direction calculated to require or increase the chance of granulation, instead of primary adhesion. In the last operation for caries of the elbow, which I performed about three months ago, you recollect how completely the symmetry of the arm was preserved, how quickly the wound healed, the patient, a middle aged female, being dismissed at the end of four weeks, and how perfectly she retained the use of the limb.

AMPUTATION AT THE ANKLE-JOINT.

The man who had his foot amputated at the ankle a fortnight ago, on account of the shattering of all the toes and adjacent parts, which resulted from the fall of a piece of iron, weighing three tons, is doing extremely well now. For some time after the operation his fate seemed uncertain, from diffused inflammation and suppuration of the limb. Judging from these symptoms, I entertain little doubt, that if amputation of the leg had been performed, the patient would not have recovered, the risk in such cases seeming to be much greater when the dense bone is divided, and the medullary canal exposed, than when the cancellated osseous texture alone is concerned. In addition to the great advantages of amputation at the ankle in regard to comfort of the stump and usefulness of the limb, the diminution of danger therefore should be kept in recollection, when the merits of the operation are weighed against those of amputation of the leg.

There is another case in the hospital requiring this operation, the patient being a young man, who has been sent from the Charity Workhouse on purpose to undergo it. The joint, as you perceive, is greatly enlarged with numerous fistulous openings and lateral mobility. It was sprained about eight months ago, and for the last six months confined the patient to bed. I propose to perform the operation to-morrow or next day, and trust that the result will be no less satisfactory than those you have previously witnessed. It is very surprising to me, that so much should be said about the difficulty, or at least the laborious nature, of this procedure. You have repeatedly seen me complete the disarticulation, without any hurry, in less than a minute, which is surely sufficient proof as to the facility of the process.

LITHOTOMY.

J. H., aged 61, suffers from the symptoms of stone in the bladder, and one, about the size of a chesnut, has been found upon examination. It might be crushed, and so removed, but, in my opinion, with less safety than if extracted through incision. The patient was under my care, for the same complaint, last year, when, in compliance with his desire, I broke down a stone, of which the fragments voided by him implied a nearly equal magnitude with that of the existing concretion. He remained free from uneasiness for a time, but for some months past has been again complaining. It is most probable that a fragment has remained in the bladder, and formed the nucleus of another stone, of which the rapid growth affords evidence of a more disordered state of the urinary organs than that formerly displayed by them. In these circumstances, the repeated introduction of crushing instruments would be apt to excite chronic inflammation of the bladder, which is much more painful to endure, and difficult to remedy, than the stone itself.

I therefore propose to perform the operation by incision, which, when done under the influence of chloroform, I believe admits of being rendered perfectly free from danger. It is now universally admitted, that in addition to a free incision of the

integuments, with ample division of the muscles and fasciæ of the perineum, the parts at the neck of the bladder must be cut to a certain extent, in order to prevent the fatal inflammation, which is sure to result from their being torn or bruised; while, on the other hand, there can be no question, that the space required for extracting any stone beyond the smallest size from the bladder, must be obtained chiefly by separation, and tearing of the textures, which readily yield after the resisting portion has been divided. You lately saw me remove a stone, weighing very nearly three ounces, from a boy of 14; and when you consider the size of the prostate at this age, must be satisfied that the aperture extended far beyond the limits of this part, and yet the patient recovered without a single bad symptom. The great nicety of the operation thus consists in cutting enough, and no more; and when this was done on patients who, writhing under pain, involuntarily strained the levator ani, so as to elongate the neck of the bladder, and withdraw the prostate beyond the reach of the operator's finger, as the part to be cut could neither be seen nor felt, I thought it necessary to employ a knife, so fashioned as to ensure the proper amount of section through the simple straightforward action of its blade. But now, when the patient lies as passively as the subject on a demonstration table, the finger may always guide to the requisite limit of incision, and thus remove any trace of uncertainty from the process.

P. S.—The operation was performed a few days afterwards, and the patient is now nearly well. The stone, which was of the size anticipated, when divided showed that its nucleus consisted of a small fragment from the former concretion.

Part Fourth.

PERISCOPE.

MEDICINE.

ON SOME OF THE CAUSES OF PERICARDITIS. BY DR JOHN TAYLOR.

In this communication, which appeared in the "Medico-Chirurgical Transactions," vol. xxviii. p. 453, the main object of the author is to determine what are the principal causes of pericarditis, and to ascertain their frequency, both absolutely and relatively to each other.

He does not profess to investigate all the causes of pericarditis. He has first inquired what were the causes actually observed in all the examples of the disease which have fallen under his notice; in the next place, he has investigated, more in detail, their frequency, both absolutely and relatively to each other, as well as some other of the circumstances connected with each of the causes so observed; lastly, he has examined, incidentally, the influence of the same causes in producing inflammation of other internal organs, both in connection with, and independently of, pericarditis.

The cases of acute and severe pericarditis examined are thirty-five in number. Of these nineteen occurred in the progress of acute rheumatism; ten in connection with Bright's disease of the kidneys; three others may have had Bright's disease, but if not, the cause is unknown; one occurred with malformation of the heart and consequent cyanosis; two were produced by the extension of inflammation from a neighbouring texture,—in one from the liver and diaphragm, and in one from the left pleura.

These severe cases of pericarditis may again be conveniently subdivided into two smaller groups.

1st. Those occurring in persons previously in good health or in the course of an acute disease; and 2nd. Those occurring in persons in bad health or in the progress of some chronic disease. A remarkable and important difference will be found in these two divisions, in relation to the causes of the disease. Of twenty-nine cases examined, with a view to this difference, sixteen belong to the first, and thirteen to the second, of the two divisions just described. Of the cases in the first class all were complicated with acute rheumatism, and none of them, so far as is known, with Bright's disease. Of the cases in the second class, only one was complicated with acute rheumatism; whereas fully two thirds were known to be associated with Bright's disease, and all of them may have been.

The two great causes of pericarditis, therefore, appear to have been acute rheumatism, and Bright's disease of the kidneys. The author next enters into some considerations intended to show that these two diseases owe their power of inducing pericarditis to the same ultimate cause, viz., an alteration in the composition of the blood; but he does not attempt to determine, whether the alteration in the blood be essentially the same, in relation to the production of pericarditis, in the two diseases referred to. If it be assumed that the pericarditis, which was associated with cyanosis, likewise depended upon the state of the blood in that disease, it will then appear that only two *generic* causes of the inflammation of the heart were observed in the thirty-five cases under consideration, viz., a morbid condition of the blood, and extension of inflammation from a neighbouring texture.

The author next examines the cases of adhesion of the pericardium, and of white spots upon it, with reference to the causes of the inflammation producing them; and he arrives at the conclusion, that in every case in which any information is given upon the subject, there had previously been either acute rheumatism, or pleurisy, or there was found actually existing, either Bright's disease, or some other disease of the kidneys.

The two chief causes of acute pericarditis which were thus observed, viz., acute rheumatism, and Bright's disease of the kidneys, are next examined with more detail.

I. *Of Acute Rheumatism as a Cause of Inflammation in the Heart.*—The frequency of acute rheumatism, as an observed cause of pericarditis, has been already stated; it was observed in two-thirds of all the cases of the latter disease.

Of seventy-five cases of acute rheumatism, treated by the author in University College Hospital, thirty-seven, or about one-half, had morbus cordis of some kind or degree; the rest had probably none.

Among these seventy-five cases of rheumatism, there occurred six of acute pericarditis of considerable severity, besides two very slight cases. The proportion of the former, therefore, was one in twelve and a half cases. In the same seventy-five cases of rheumatism, there were thirty-two cases of valvular disease of the heart, either old or recent, besides two known to be recent. There was, therefore, one case of valvular disease in about every two cases of rheumatism.

The author next compares these results with those of various writers upon the same subject, and from this comparison he concludes—

1. That acute inflammation of the heart has occurred less frequently, as a complication of acute rheumatism, in his experience, than it has been believed to occur in the experience of those writers whose opinions seem to have been most generally adopted by the profession.

2. That the frequency of inflammation of the heart, even in his cases, has been such as abundantly to show the great influence of acute rheumatism in its production.

An attempt is next made to ascertain the real amount, and the causes of the difference between the observations of the author and those of the writers referred to. The result, as it respects most of these writers, may be briefly stated to be,—

1st. *Of Pericarditis.* In those instances in which such data have been given as enable us to compare similar cases, the results are very nearly the same. In

various instances, however, no comparison can be fairly made, either from the want of figures, from the mixing together of cases of endocarditis and of pericarditis, or from a great difference in the age of the subjects.

2nd. With respect to *Endocarditis*, the discrepancy is much greater than in the case of pericarditis, and one of the chief causes of the difference appears to the author to be, that most writers have given the proportion of cases of valvular disease in acute rheumatism in such a manner as implies (when it is not directly stated) that they were all cases of acute disease—omitting, therefore, to distinguish the proportion of them which were of older date.

The proportion of cases of valvular disease of all dates observed by the author is nearly the same as that observed by the chief writers referred to; but he attempts to show,—

1st. That the greater number of these are examples of old valvular disease.[1]

2nd. That, at all events, in most cases it is very difficult to distinguish when the disease is recent and when old; and, 3rd. That, as far as he has been able to ascertain, acute endocarditis is less frequent than acute pericarditis in rheumatism.

The frequency of morbus cordis in chronic rheumatism is next inquired into, and compared with that in acute rheumatism; and from this inquiry it appears, 1st. That the total number of cases of morbus cordis, old and recent, is nearly the same in the two kinds of rheumatism. 2nd. That acute inflammation of the pericardium and of the endocardium is much more common in cases of acute than of chronic rheumatism.

The frequency of other internal inflammations in the course of acute rheumatism is next examined, and compared with that of inflammation of the heart, and the result is that the last mentioned inflammation exceeds every other in the frequency of its occurrence.

The next subject of examination is—the circumstances which favour the occurrence of inflammation of the heart in the progress of acute rheumatism.

1st. *Metastasis.*—Metastasis of the rheumatism did not occur in any one of the cases observed; hence it is inferred that this is not the ordinary nor even a frequent mode in which rheumatism produces cardiac inflammation. It does not, however, follow from these facts that metastasis never takes place, and it is attempted to be shown that its occasional occurrence is both consistent with theory and established by observation.

In this part of the paper the author refers—1. To some cases of rheumatism in which the inflammation of the heart appeared before that of the joints. 2. To some cases of what has been termed "rheumatic fever without arthritis," *i. e.*, cases presenting all the symptoms of acute rheumatism except the affection of the articulations. 3. To one of his own cases in which he thinks it probable that there may have been acute rheumatism, and in which there was pericarditis, but no affection of the joints throughout.

2nd. *Form of the Rheumatism.*—Adopting the divisions of rheumatism given in the treatise of Dr Macleod, we find that all the cases of rheumatic pericarditis occurred in connection with the *fibrous* as distinguished from the *capsular* form of rheumatism. In estimating the influence of this circumstance, however, it is necessary to remember that the fibrous variety of rheumatism is much more common than the capsular.

3rd. *Intensity of the Rheumatism.*—From the cases examined, it appears to result that the violence and fatality of rheumatic pericarditis, are generally greater in the cases in which the accompanying rheumatism is very acute than in those in which it is sub-acute. Whether pericarditis be more frequent in the more severe than in the less severe form of rheumatism, the author's cases do not enable him with confidence to determine. As far as they go, however, they are

---

[1] The condition of the heart is referred to in 1026 cases of all kinds of disease indiscriminately, to show the proportion in which the heart suffered.

opposed to such a view, for three-fourths of the examples of rheumatic pericarditis occurred in sub-acute rheumatism.

4th. *Stage of the Rheumatism.*—In more than half the cases of rheumatic pericarditis, the affection of the heart appeared on or before the fourth day of the disease. With one exception, the pericarditis did not appear sooner in those cases in which it was very severe than in those in which it was much less severe.

5th. *Influence of Repeated Attacks of Rheumatism.*—In the cases examined, pericarditis was found to be both more frequent and more severe in the first than in subsequent attacks of rheumatism.

6th. *Previous Disease of the Heart.*—Ten out of fifteen patients had no previous disease of the heart, and among these were found all the most severe cases of pericarditis.

7th. *Age.*—Of fifteen patients, nine, or about two-thirds, were only twenty years of age or under; five were between twenty and twenty-six; and one was above forty.

8th. *Sex.*—Of fifteen patients, nine were males and six were females. It is necessary, however, to remember that rheumatism is more common among men than women.

9th. *Influence of Venesection.*—Twelve of the patients had not been bled before the pericarditis appeared; the remaining three were bled, one eleven days, one five days, and one three days, before the pericarditis supervened.

Mode in which Rheumatism produces Pericarditis.—Upon this question the author adopts the following hypothesis as consistent with all the facts he is acquainted with:—

The cause of acute rheumatism is probably the presence of some morbid matter in the blood, which has an especial affinity for the fibrous and fibro-serous tissues of the body, and which, by fixing itself in one or more of these, induces various local inflammations. The similarity of the structures implicated, is probably the reason why rheumatic pericarditis or endocarditis often occurs at the same time with or succeeds to rheumatic inflammation in the joints, just as rheumatic inflammation in one joint occurs with or succeeds to that in another; and the heart is more frequently (?) and more severely affected in severe cases of acute rheumatism, for the same reason that more joints are affected and more severely affected, and also that more fever is present in such cases; which reason may not improbably be a greater abundance of the materies morbi in the blood.

II.—*Of Bright's Diseases of the Kidneys as a Cause of Inflammation in the Heart.*—We have already seen that of thirty-five cases of pericarditis, Bright's disease was the only assignable cause of the inflammation in thirteen, or more than one-third. It remains to institute the corresponding and complementary inquiry, into the frequency of pericarditis and endocarditis in Bright's disease.

1. In the bodies of fifty patients, who had either died of Bright's disease, or who were ascertained to have this disease in an advanced stage, acute pericarditis was found in 5, or in 1 out of 10, and acute endocarditis in 4, or in 1 out of 12.

2. On the other hand, in 142 bodies, in which the kidneys were not affected with any appreciable disease, acute pericarditis was found in 4, or in 1 out of 35, and acute endocarditis in 2, or in 1 out of 71.

Pericarditis and endocarditis, therefore, being four times more frequent in fatal cases of Bright's disease, than in fatal cases without renal disease, it seems clearly to follow that the influence of Bright's disease in producing these inflammations is unquestionable and great.

III. The frequency of other internal inflammations in fatal cases of renal disease, is next examined and compared with their frequency in fatal cases without renal disease. From this comparison it appears,—

1. That the proportionate number of acute internal inflammations, exclusive of those of the heart, is twice as great in the series of cases with renal disease, as in that without such disease; the numbers being respectively ninety-six and forty-two per cent.

2. That the proportion of patients, likewise, among whom these inflammations

were distributed, is greater in the former than in the latter series of cases; the numbers being respectively sixty and thirty-six per cent.

Hence we may safely infer, that Bright's disease has a great tendency to produce other internal inflammations besides those of the heart.

IV. A further examination of the same facts shows, that the relative frequency of various internal inflammations, is different in fatal cases of Bright's disease and of other diseases, taken indiscriminately.

The following are the various inflammations inquired into, arranged in the order of their frequency, as they were calculated to be due to renal disease, or to the causes operating in other fatal diseases.

1. *Inflammations due to renal disease.*—Cerebritis, pneumonia, pleuritis, pericarditis, endocarditis, meningitis, peritonitis. 2. *Inflammations independent of renal disease.*—Pleuritis, pneumonia, peritonitis, meningitis, cerebritis, pericarditis, endocarditis.

V. From a comparison of the numbers given in this paper, we may calculate the tendency to produce various internal inflammations of the causes operating in fatal cases of Bright's disease as compared with those present in cases without renal disease. If we use the term Bright's disease, to represent all the causes operating in fatal cases of Bright's disease, and then compare these with the causes in operation in fatal cases without any renal disease, we shall find that Bright's disease produces:—1. *Endocarditis,* almost 5 times as often as all other causes put together; 2. *Cerebritis,* fully $3\frac{1}{2}$ times as often; 3. *Pericarditis,* fully $2\frac{1}{2}$ times as often; 4. *Pneumonia,* just 5 times as often; 5. *Pleuritis,* just $\frac{3}{4}$ times as often; 6. *Meningitis,* 3 times less frequently; 7. *Peritonitis,* 100 times less frequently. The author next inquires into the comparative efficacy of acute rheumatism and of Bright's disease, in producing pericarditis and other internal inflammations.

In comparing these two affections, we meet with some difficulty, arising from the fact that one of them is an acute disease, and is seldom fatal, whereas the other is chronic and generally fatal. It appears to the author that the best mode of avoiding this difficulty is, to compare fatal cases of Bright's disease with ordinary cases of acute rheumatism. If the object were to ascertain the proportion of cases, in which traces of previously existing inflammation were found, this method would be objectionable, because the one disease having run a much longer course than the other, it would have had much more time to produce any inflammation which it had the power to produce; but, if cases of actually existing inflammation alone be counted, then the objection does not exist, and the result should not be far from the truth.

Of seventy-five cases of acute rheumatism, eight, or one in nine and a half, were complicated with pericarditis acuta. Of fifty fatal cases of Bright's disease, five were complicated with pericarditis acuta—or one in ten.

Hence, Bright's disease in the advanced stage, and acute rheumatism, appear to have caused acute pericarditis in an equal proportion of cases.

An examination of twenty cases of old adhesion of the pericardium, however, shows, what the considerations stated above might have led us to anticipate, that old adhesions of the pericardium have been produced twice as often by Bright's disease, as by previous attacks of acute rheumatism.

From considerations which could not readily be made intelligible in this abstract, the inference is next drawn, that acute rheumatism has a greater tendency to produce pericarditis than has Bright's disease in its earlier stages, and consequently that the tendency of Bright's disease to induce pericarditis, and probably also other internal inflammations, increases in proportion as the affection of the kidney is more advanced.

The conclusion thus arrived at is quite in accordance with the *modus operandi* of Bright's disease in producing local inflammations, which has been assumed in an earlier part of the paper; for, if this effect of renal disease depend upon a morbid condition of the blood, arising from the excessive accumulation of urea, we should expect the effect to increase in proportion as the structure and the

functions of the kidneys, and the consequent composition of the blood, deviate more from the healthy condition.

In conclusion, some remarks are made upon the probable occurrence of pericarditis in other blood diseases, besides those observed by the author.

Likewise some observations on the importance of the constitutional or predisposing causes of inflammation, as distinguished from the exciting causes.—*Abridged from Braithwaite's Retrospect.*

TUMOUR OF CEREBELLUM.

[The following interesting extracts from the Swedish Journals have been kindly communicated by a valued correspondent :—]

Widow N. N., 45 years of age, came under treatment in the middle of July, last year. She stated that she had previously enjoyed good health, and that it was first about Midsummer of the same year that she began to suffer from pains in the head, then of moderate severity, but which soon increased, and were ultimately accompanied with a stupifying sense of heaviness, so that she could scarcely hold herself upright. In her efforts to attend to her usual occupations, she was frequently attacked with giddiness, and sometimes so suddenly as to fall headlong. The seat of the pain she describes to be "behind the ears," but passing up to the vertex, and spreading itself round the whole head, especially in the erect position. With the exception of a certain amount of irritability, there is nothing to remark in her psychical condition; and the organs of the senses are unimpaired in their functions, unless that, when the pain is excessive, she complains of sounding in the ears and dimness of vision. The pupils are fully contractile. The sense of touch is perfect; but she alleges a regularly-increasing debility in the organs of motion. The pulse is regular; and the other functions present nothing remarkable.

As the anamnesis excited no suspicion of metallic poisoning, and the symptoms evidently indicated the brain as the seat of the disorder, the diagnosis was an indefinite one of *organic disease of that organ.* The treatment during the summer, and up to autumn, consisted of counter-irritants behind the ears, and a seton in the neck (which was kept discharging for five or six weeks), with the internal use of alteratives, *arnica,* &c. In the meantime, the pain in the head went on uniformly increasing, with severer paroxysms of suffering. The functions of the organs of the senses gave way gradually and successively, first the hearing, then the sight, and the capacity of motion became also more and more limited, so that, towards Christmas, she neither heard, nor saw, and could not assume or maintain the sitting posture without assistance.

Her mental faculties were still but slightly, if at all, affected. She could certainly give little information as to her condition, because she could be reached by no questions, and could recognise none of those surrounding her; but her conversation was resigned and religious, with the expressed hope that she should soon be freed from her sufferings. These were occasionally, especially during the night, so intense, as to force her to utterance of piercing and continuous shrieks. Throughout the whole of this time, and towards an advanced period of the following spring, she retained perfectly her smell, taste, and touch, with a moderately good appetite, which she seemed to take pleasure in having gratified through variations in her diet. In the latter part of spring, however, some change began to evince itself in her mental condition; sometimes rising into religious ecstasy, with proneness to address admonitions, partly extemporaneous and partly from memory, to the bystanders; and sometimes subsiding into apathy, from which she could be easily aroused, when she would explain her sensations in her own manner, say that she saw light flashing through the darkness, heard delicious music, &c. She could now, and henceforward, not even raise herself on her elbow or turn in bed; and her stools, her appetite for food still continuing good, were passed involuntarily; while the drowsy, but never thoroughly comatose, state predominated, and was periodically accompanied by fits of screaming.

In the month of June all the remaining faculties became obviously deadened; and the patient expired calmly on the 18th of July, having never spoken during the preceding day.

On opening the body, there was found beneath the cranium, within the usual site of the glandulæ Pacchioni, two or three wart-looking bodies, of the size of peas and of grains of hemp-seed, the external appearance of which, with regard to their position and otherwise, scarcely attracted my attention, as they seemed to be merely the organs referred to in a hypertrophied condition;[1] but, on removing the *dura mater* from the brain, it was discovered that at each of these spots the membrane was perforated and traversed by a tubercular excrescence, proceeding from the brain, and which, to the naked eye, appeared of brain-like structure, the cerebral mass itself seeming to have undergone no change in the vicinity. A similar *hernia cerebri*, proceeding from the anterior lobe of the right hemisphere, had not perforated the *dura mater*, but was connected with it by loose cellular tissue. In the anterior depression of each of the middle cranial *fossæ*, there was observed, also attached to the *dura mater*, a small portion of brain-like substance, about an inch in length, which had evidently been torn from the lobes of the brain in lifting the mass, the rest of the cerebral substance having here a perfectly healthy appearance. The optic nerves presented no perceptible alteration.

In both the lateral ventricles, and in that of the *septum lucidum*, there was a considerable accumulation of clear serum. Beyond this there was nothing abnormal either in the hemispheres, the *pons*, or in the nerves at the base of the brain.

On removing the *tentorium*, and proceeding to examine the cerebellum, there was observed in the left hemisphere a mass, of a dense consistence, extending as far as, and somewhat over, the vermiform process. The cerebellum being raised, it was discovered that this tumour was not situated within the cerebral substance, but was firmly attached to the *dura mater* beneath, by a basis, without pedicle, of about half an inch in diameter. It was of the size of a duck's egg, of a nodulated outline, and covered with an organised, vascular, translucid membrane, with a degree of consistence approaching that of scirrhus. The substance of the left hemisphere had been compressed by it into not more than half the volume of that on the right. The cineritious substance had especially suffered, having apparently totally disappeared, and no trace remaining of the figuration of the *arbor vitæ*. The lamellar convolutions were thin and compressed; and the cerebral substance was not softened, but rather firmer than on the opposite side, and remarkably exsanguinous. The right hemisphere, *crura cerebelli*, and *medulla oblongata*, appeared perfectly sound. The rest of the body was not examined.—*Dr P. A. Levin*, in *Hygiea, Med. och Pharm. Maanads-skrift*, February 1850.

TANNIN IN CHOLERA.

During the recent epidemic of cholera, tannin was recommended and employed by the younger Græfe of Berlin. He prescribed it in upwards of thirty cases, several of which were in the state of collapse, and of these two only proved fatal. The dose was from five to ten grains, half-hourly or hourly. Dr Willebrand, of Finland, in reporting the treatment to the Medical Society of Sweden, states that he was himself a witness of this brilliant success.—*Förhandlingar vid Svenska Laakare-Saallskapets Sammankomster*, published in *Hygiea, Med. och Pharm. Maanads-skrift*, January 1850.

---

[1] The pressure of enlarged *glandulæ Pacchioni* is well known to produce occasionally absorption of the *dura mater*.—*Tr.*

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

QUESTION OF THE COMPATIBILITY OF SKIN-DISEASES WITH HEALTHY LACTATION. BY M. CAZENAVE.

There is a great number of skin-diseases which, speaking absolutely, are not incompatible with lactation. Such are the non-specific eruptions, in the acute stage; erythema, herpes, eczema, pemphigus, impetigo, ecthyma, having, in this stage, no other force than that of an accidental transitory inflammation, and exercising no appreciable influence either on the lacteal secretion, or on the health of the infant. Considered relatively, some of these diseases may, under certain circumstances, have peculiar effects. Thus acute eczema, if intense and general, may have the effect, through its general influence on secretion, of diminishing the quantity of milk. Impetigo, denoting a morbid exaggeration of the lymphatic temperament, may also be incompatible with lactation; and so also may lichen and ecthyma. But, setting aside certain relative peculiarities, and certain individual circumstances, or organic relations between the nurse and the infant, which impress on these eruptions a character of relative severity, of which the value can only be ascertained by examining each case by itself, M. Cazenave thinks that, in general, most of these eruptions, when in the acute stage, cannot be considered as a reason for rejecting a woman as a wet-nurse, and still less for discontinuing the suckling of an infant by one in whom any of them has appeared during lactation.

As to these affections in the chronic stage, and as to those various forms of skin-diseases which are confounded under the name of *dartres*, M. Cazenave thinks that there is a large number which are compatible with the function of lactation, and of which the presence, either in the mother or in the nurse, should not be considered a source of danger to the infant, nor a reason for changing its food. Any influence which these may, under certain conditions, exert on the infant, is only relative.

The cutaneous diseases which have a negative absolute value in the choice of a nurse, or which ought to become an obstacle to the continuation of lactation, belong to several categories. Eczema, impetigo, or lichen, may become incompatible with lactation, when in the chronic stage, and when their extent and intensity are more considerable than usual. But there are some diseases of the skin which of themselves, independently of all circumstances of duration or intensity, produce in general, though in different degrees, a complete change in the economy. And this change, although accidental and possibly only transient, is such as to impart to these eruptions, even in the acute state, an absolute value, as contra-indicatory of lactation. Such are chronic ecthyma, which always denotes a cachectic state of the constitution; rupia, pemphigus and purpura, which likewise reveal a deterioration of the system.

There are, further, some diseases of the skin, which are very important in the point of view which we are now considering, and which are characterised by degeneration of the tissues, and by their tendency to destroy the affected parts; such as the two forms of elephantiasis, molluscum, lupus, keloid, etc.

The contagious skin-diseases (itch and herpes tonsurans) cannot have any influence on lactation; but it is otherwise with favus, which is one of the most incompatible. There remains but one form of skin-disease—the syphilitic—which is directly transmissible by suckling, and consequently is the most incompatible of all.

It is no longer allowable, in making choice of a nurse, to proscribe, in a general manner, the diseases of the skin known under the name of *dartres*. With regard to lactation, skin-diseases have different values, not only according to their nature, but also by reason of certain accidental conditions of individuality, extent, intensity, etc. The syphilitic affections alone should be excluded as virulent. Several cutaneous affections have a negative absolute value, as producing a generally serious modification of the organism: these are precisely the diseases farthest re-

moved from those called *dartres*, and which do not properly constitute eruptions. The eruptions, properly so-called (*dartres*), are generally compatible with lactation. Some may accidentally lose this quality, by being of unusual intensity, or by transmitting a deterioration of the economy; but they are few in number. The eruptions which, in their nature and local conditions, are perfectly compatible with lactation, may cease to be so relatively, in consequence of certain peculiar relations between the nurse and the infant.—*Bulletin Général de Thérapeutique*, December 15, 1850; and *London Journal*, February 1851.—[We cannot agree with M. Cazenave in the opinions herein enunciated. In making choice of a nurse, we would certainly proscribe every one affected with the itch, or with erythema, herpes, eczema, pemphigus, impetigo, ecthyma, even though they were non-specific, and in the acute stage.]

STERCORACEOUS ACCUMULATION MISTAKEN FOR PREGNANCY. BY M. LARONDE.

Madame L., æt. 48, in otherwise good health, came to consult her physician concerning a swelling which had appeared and was increasing in the hypogastrium. Pregnancy was diagnosticated,—the menses appearing very irregularly, and at long intervals. But at the end of nine months nothing made its appearance; while the tumour continued gradually but slowly to increase in size.

When M. Laronde was called in, the patient was despaired of by her attendants. He found her condition as follows:—pulse small and weak; *decubitus* dorsal, prostration very great; face pale, emaciated and characteristic of a chronic affection of the intestinal canal; breath fetid, gums soft and bleeding;—the abdominal tumour rising as high as the umbilicus, hard and rounded; but in place of rising from the sides it seemed intimately connected with them. Vaginal examination was not permitted. The tumour felt like a mass of half-dried earth or clay. She went to stool once or twice daily, and was occasionally troubled with diarrhœa.

Glauber salts, &c., &c., were administered; an immense quantity of fecal matter was discharged, and the tumour disappeared.—*Journal de Médecine et de Chir.*, November 1850.

CASE OF BIRTH AFTER THE DEATH OF THE MOTHER.

Dr Schneider relates, that being summoned in haste to a woman in labour, he found her dead on his arrival. On placing the hand on the yet warm abdomen, he felt the uterus contracted and sunk in the pelvis. By an examination per vaginam, a foot was detected, and by rapidly completing the delivery, he had the satisfaction of bringing into the world an apparently still-born child, which, however, soon revived.—*Casper's Wochenschrift;* and *British American Medical and Physical Journal*, April 1851.

THE OBSTETRICAL EXTRACTOR. BY DR JOHN EVANS.

Under the above name Dr E. describes a new instrument of his invention, which "it is hoped will be free from many, and the most serious, of the objections to the forceps."

"The principle upon which it operates is plain and simple, being that of placing a band around the head of the child above its largest diameter, and fixing the ends near together by steel fingers, so that it cannot be drawn off. From this band straps pass down to the vertex, and out through the os externum, to be grasped by the hand, and upon which the extractive force is exerted."

The fingers of steel are "each 11 inches long." They are "$\frac{1}{4}$th of an inch thick at the junction with the handle," and "rapidly become thinner towards the end, until they are *as much attenuated as they can be*, to allow of two strong hinge joints in it."—*Trans. Amer. Med. Association*, Vol. III. 1850.

[We have said enough to show that Dr Evans's instrument is neither simple nor its principle plain, and we believe that attenuated steel fingers, 11 inches long, present objections to this complicated lac (for such it seems to be) greater than any that can be urged against the forceps.]

TOXICOLOGY.

DETECTION OF LEAD IN THE BRAIN AND LIVER OF A MAN WHO DIED OF ACUTE SATURNINE APOPLEXY. BY MM. CHATIN ET BOUVIER.

Desirous of affixing their opinion on a point of medical science still doubtful, the authors undertook experiments with a view to finding the lead of poisoning in the brain of a man who had just died, as if by thunder stroke, from acute saturnine apoplexy (*encèphalopathie saturnine aigue*), the second day after he had quitted the manufactory of a maker of white lead. As they had to do, however, with an investigation which necessarily involved delicate operations, from the very minute quantity of lead which could be present, they made a preliminary experiment on the liver.

Liver.—300 grammes were cut into very small pieces, and boiled for two hours with 2000 grammes of water acidulated by a tenth of pyroligneous acid. The solution having been evaporated, and carbonised with a little nitric acid, the charcoal was heated with boiling distilled water, containing a twentieth of nitric acid. After having driven off the excess of acid from the nitric solution by concentration, they passed a current of sulphuretted hydrogen through it, which produced an abundant black precipitate; treated with boiling nitric acid, this precipitate furnished a solution of lead very nearly pure—which, deprived of excess of acid, and reduced to 10 grammes, gave the following re-actions:—With sulphuretted hydrogen, a black precipitate; with sulphate of soda, a white precipitate; with ferrocyanide of potassium, a white precipitate; with iodide of potassium, a yellow precipitate.

Brain.—The half of the cerebral mass was subjected to the preceding treatment, only the solution which was to be subjected to the re-agents had been reduced to one gramme, on account of the very small quantity of lead found in the course of operations by sulphuretted hydrogen gas. The following were the re-actions:—Sulphuretted hydrogen, a brownish black precipitate; sulphate of soda, a slight white precipitate after some minutes; ferrocyanide of potassium, a light white precipitate after some minutes; ioduret of potassium, a very slight yellow deposit after some hours.

The result of these experiments is, that the brain, and more especially the liver, contained lead; and if it is considered that the ioduret of lead is soluble at ordinary temperatures in 1000 to 2000 parts of water, it may be admitted approximatively that the 300 grammes of liver contained a demi-centigramme (0.005), and the brain a demi-milligramme (0.0005) of this ioduret. The equivalent, of lead (1294.5), and of iodine (1579), shew that the poisonous metal constitutes half of the weight of the ioduret.

The process which has been followed, is that recommended by M. Orfila, for the detection of the lead of poisoning, to the exclusion of the lead called normal.

These researches prove that we can recover from the brain of workers in lead, who have died suddenly from saturnine apoplexy, an appreciable trace of the agent which has caused their death.—From *Journ. de Chim. Méd.*, in *Journal de Pharmacie*, March, 1851.

POISONING WITH SULPHATE OF IRON. BY M. P. LIMOUSIN-LAMOTHE, OF SAINT AFFRIQUE.

A man, named Matet, returned home on a Thursday evening, carrying a faggot of wood; after this his wife shut the door of the house, and did not open it to any one, in spite of the inquiries of the neighbours, who were attracted by sounds of groaning. On the Saturday morning, before daylight, the woman called for help; the neighbours came and found Matet dead in his bed. As the domestic relations of the couple, and the character of the woman, were of bad repute, the authorities were informed, and repaired to the place with the scientific men who were called in. The latter then learned that the woman had, previously to their arrival, and even during the night, washed a large quantity of

linen; and that, according to the wife's statement, Matet had complained of a pain in his side, after which he had died. On dissection, beyond some slight lesions of the alimentary canal, there were found some adhesions of the pleura, and softening of a part of the lung. The ordinary organs for chemical analysis were taken away, and along with them some patches of the bed-cover, stained with fœcal matters, and the authorities handed over to them a bottle containing about two wine glassfuls of water, with some bread, a pint bottle of wine, and a packet containing a white powder. Some days after, the author, along with Dr. Ancessy, made a chemical analysis, of which the following are the more important results:—

1st. Arsenic sought in the liver. No result.

2d. A fourth part of the stomach was carbonised by sulphuric acid, the carbonaceous residue treated with nitric acid to oxidise any metals it might contain. The nitric acid driven off, and the residue taken up in water. A fourth part of this was treated by Marsh's method, without any result.

The remainder of the liquid was subjected to various re-agents, and gave the following results:—Sulphuretted hydrogen at first produced no effect; but the operators, thinking that some sulphuric or nitric acid might be present and interfering with the action, a few drops of ammonia were added to neutralize it; this at once produced a copious black precipitate, which was washed and collected. To determine whether this was due to copper, lead, silver, or iron, a portion of the liquid was tested with prussiate of potash, and gave an abundant blue precipitate. Gallic acid, which, from the presence of free acid, would not act at first, gave a deep brown colour on the addition of ammonia. Ammonia and carbonate of soda gave the characteristic precipitate [of oxide of iron]. Copper, lead, and silver were sought for without results.

An important question now arose. Could these precipitates be due to normal iron of the blood contained in the vessels of the organs which were engorged, or were they due to iron swallowed during life? The amount of the precipitates left little doubt that they could not be due to normal iron, their united weight being in fact 15 grains from about an eighth part of the stomach, and the portions of the small intestines which they possessed. The author therefore proposed to seek for the iron in other articles, and to determine the state of combination in which it had originally existed.

3d. In order to obtain more precise ideas as to the origin and amount of the iron which they had found, they treated a portion of the lung with nitric acid. The residue was lixiviated with distilled water and concentrated. In this fluid hydrosulphuret of ammonia and gallic acid only produced a deepening of the colour, and ferrocyanate of potash also only deepened the same. In an hour there were slight deposits from these re-agents, which were only a little more apparent and appreciable next day; yet the lungs were obviously more charged with blood than the stomach and intestines. By way of comparison, 1500 grains of human blood, were treated as above, and gave still fainter results with the re-agents.

4th. One-half of the pieces of bed-cover which were stained with fœcal matters, were treated with boiling distilled water and filtered. The filtrate passed with great difficulty, and gave very little result with the various re-agents Presuming that the iron might exist in the state of protoxide, or in the form of an insoluble sulphuret, and had remained upon the filter, the portion which had not passed through was acted on by nitric ucid. In this fluid, the presence of iron was abundantly shown by prussiate of potash, hydrosulphuret of ammonia, &c. In these fœcal matters there could be no reason to suspect that the iron existed normally; they therefore concluded that it had been swallowed during life. But in what state of combination? This remained to be ascertained.

Sulphuric acid could not be tested for in the liquids from the stomach, because it had been carbonised by sulphuric acid. They therefore sought it in the liquid from the fœcal matters, and it was found in small quantity by the action of chloride of barium; but it still remained to be determined, whether the sulphuric acid might not come from other sources than sulphate of iron.

At this stage of the proceedings, the authorities brought to them a bottle con-

taining vomitings of the deceased, which had been found under his bed. This bottle contained 2½ ounces of a thick paste. If sulphate of iron had been swallowed during life, it ought to be found in the undigested matter.

5th. One half of the vomitings was taken; they consisted chiefly of bread in the form of a thick pulp, with fatty matters, a piece of bacon, &c. The viscosity of the mass prevented its being treated simply with water; it was therefore carbonised by nitric acid, and treated like the other matters. The product of the filtration was divided into two parts, in one of which iron was detected with equal readiness as in the other operations; in the other, sulphuric acid was detected in notable quantity by means of nitrate of baryta. All their doubts were now resolved, but a comparative trial for sulphuric acid was made with ordinary bread, which gave no results.

6th. The clear bouillie used for making a tisane [we presume this means the two wine glassfuls of water with bread found in the bottle] was tested, and sulphate of iron detected in it.

The little packet of white powder was found to be flour; the bottle of wine contained nothing unusual.

From all the facts detailed above, they drew the following conclusions:—

The stomach and intestines contained sulphate of iron.

This salt could not be derived, at least the whole of it, from normal iron contained in the blood, for the lungs yielded it in infinitely smaller quantity.

The fœcal matters contained sulphate of iron.

The vomitings contained sulphate of iron.

The bread and the liquid used as tisane contained sulphate of iron.

This salt had been swallowed during life.

If it was given before the illness of the deceased, it might occasion severe disorder in the economy.

If given during the illness, it might not only aggravate the disorder, but even hasten the death.

In the particular circumstances of the case, it was impossible to determine in what dose, or at what time, the sulphate of iron had been swallowed.

At the trial, long and difficult scientific questions arose; such as,—Is sulphate of iron in itself poisonous? At what dose does it become so? Can legal medicine furnish any precise information on the subject? Can we readily distinguish normal iron from iron swallowed? Upon these and a host of other questions, the opinions of several persons, more or less competent, were laid before the court.

The jury, convinced of the poisoning, and considering the character of the prisoner, brought in a verdict of guilty, and sentence of death was recorded; but from the contrariety of the opinions given respecting the poisonous qualities of sulphate of iron, and the dose in which it could be considered as such, the court and jury have recommended the woman to government for commutation of sentence.—*Annales d'Hygiène*, January 1851.

POISONING WITH MURIATE OF BARYTA. BY DR C. WOLF.

A student of medicine, who supposed that he had swallowed three teaspoonfuls of sulphate of magnesia, was seized with tormina and vomiting; the pulse was weak and irregular; the tongue natural. Dr Wolf at first supposed the case to be one of "summer cholera," excited by the purgative; but as in half an hour more he found his patient with ice-cold extremities; pulse 65, irregular and feeble; hands and feet powerless; paralysis of the left eyelid; the voice weak, and incessant griping—he analysed what remained of the so-called sulphate of magnesia, and found it to be muriate of baryta. The patient got only some sulphuric acid and castor oil, by which the griping was soon removed, but there came on retention of urine requiring the catheter. The muscular powerlessness soon disappeared, and in three days the patient was quite well. He had taken altogether three drachms of the salt. Dr Wolf detected the chloride of barium in the evacuations by incinerating them.—*Casper's Wochenschrift, in Schmidt's Jahrbücher*, No. I., 1851.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING VI.—*Wednesday, 16th April* 1851.—Dr BEGBIE, President, in the Chair.

IMPACTION OF A FOREIGN BODY IN THE PHARYNX, WHICH HAD BEEN PRODUCTIVE OF FATAL ASPHYXIA.

Dr Mackenzie showed a specimen of impaction of a foreign body in the pharynx, which had been productive of fatal asphyxia. The parts were removed from the body of a cab-driver, who, while eating his dinner a few days previously, was suddenly choked in swallowing a large morsel of meat. He immediately walked across the street to a chemist's shop; shortly after entering which he fell down in a state of asphyxia. Unfortunately, surgical assistance was not rendered on the spot. He was conveyed a short distance to the Infirmary, where, however, he did not arrive till about twenty minutes had elapsed from the occurrence of the accident. Tracheotomy was immediately performed by Dr Pinkerton, and attempts made to restore animation by means of artificial respiration, but without effect. The foreign body (a large mass, apparently of tough and gristly beef) was seen filling nearly the entire pharynx from opposite the cricoid cartilage below, and projecting forwards as far as the uvula. It rested on the glottis, pressing the epiglottis forwards. The trachea and lower part of the larynx being laid open in front, part of the mass was seen to have entered the larynx, and to have descended a little beyond the *rima*. Dr M. remarked, that the propriety of using forceps in the removal of foreign bodies from the pharynx, and the danger of employing the probang for this purpose, were well illustrated by the preparation. The use of the probang in the present instance could scarcely by any probability have dislodged the portion of the mass which had entered the larynx, while a single attempt with a pair of dressing forceps would almost certainly have been attended with success.

Dr M. showed another preparation, of a somewhat similar kind,—a large piece of meat impacted in and above the *rima glottidis*, and which, in the same way as in the present instance, had produced immediate death by asphyxia.

EMPHYSEMA OF THE LUNG.

Dr W. T. Gairdner exhibited a specimen of emphysema of the lung, showing one very large emphysematous vesicle, with numerous smaller ones, near the apex, in a case in which much of the upper lobe had undergone hypertrophy and induration. The atrophied portions were marked by deep furrows between the emphysematous lobules. The case was a good illustration of the mode of generation of emphysema, which Dr G. believed was never a primary affection of the lung, but always the result of other lesions, as explained in his paper on bronchitis, read to the Society last year.

Dr G. also exhibited a portion of apparently bony or calcareous deposit, about one and a half inches square, and half an inch in thickness, imbedded in the diaphragmatic pleura.

TREATMENT OF ERYSIPELAS BY THE TINCTURE OF THE MURIATE OF IRON.

Dr Charles Bell read a communication on the treatment of erysipelas by the tincture of the muriate of iron. The paper consisted of two parts. The first

by George Hamilton Bell, F.R.C.S.E., and the second by Dr Charles Bell, F.R.C.P. (This paper appeared in full in the June Number of this Journal.)

Dr Seller said he had felt great interest in the paper just read. It was difficult to reconcile the treatment proposed with the acknowledged principles on which inflammation was to be treated. The fear of the exciting influence of iron on the heart's action was at one time so great, that it was forbidden whenever there was even the name of inflammation. That day had gone by; but still the approved rule was, to avoid it in acute inflammation. Acute erysipelas was, however, a peculiar inflammatory disease. The treatment in it was not to be judged of by common rules, even when the symptoms were unequivocally those of high excitement. There was reason to think, that erysipelas was usually kept up by some peculiar internal cause of irritation. And it would be desirable to ascertain how the muriate of iron acted in this disease on the urinary secretion,—whether, for example, it had any effect on the urine analogous to that of colchicum, which was one of the acknowledged remedies in the same disease. Many facts had been ascertained of late years, showing how mischievous was the effect in the course of acute, and in particular of eruptive, diseases, of the retention of urea in the living system; and he referred to papers formerly read before the Society, by Professor Henderson and Dr Douglas Maclagan, bearing on this very important subject. Could the muriate of iron be shown to operate on the chemical character of the urine in erysipelas, there would be less difficulty in coinciding with the opinion of the authors as to its beneficial effects.

Dr Andrew stated, that, so far as hospital experience went, erysipelas, it appeared to him, was a disease which did not bear the antiphlogistic treatment. On the contrary, wine and alteratives were generally required. He had seen the happiest effects result from the external use of the nitrate of silver. He had commenced a trial of the muriate of iron in a case yesterday. He would at some future time let the Society know the result.

Dr Mackenzie concurred with Dr Andrew, that in hospital practice wine and stimulants were often of the greatest service in the treatment of erysipelas, which, in many instances, from the first presented the character of a true *inflammatio debilis*. In some of the London hospitals (St Thomas's, for instance) the disease was treated throughout by stimulants. There was another acute form of disease,—viz., phagedenic ulceration of the penis,—in which Dr M. had found iron, according to Ricord's recommendation, of great use. The preparation employed was tartarised iron.

Dr Simpson stated, that he had tried the chalybeate practice mentioned in the paper, in two cases of erysipelas of the head and face, and both patients had done well. The first case was one peculiarly fitted for it. At the time of the attack, the patient was completely blanched and anemic, from long-continued menorrhagia in connection with intra-uterine polypi. Dr Christison had seen the patient with him. When the erysipelas first appeared upon the face, it had this strange peculiarity, that it was truly a white erysipelas, there not being apparently enough of red blood to give the usual red colour to the inflamed surface. The disease consisted entirely in a gradually spreading swelling, covered with scattered vesicles. No case could be more likely to benefit under the treatment by iron; and, as the patient disliked it in the form of drops of the tincture, she used pills made of the citrate, and apparently with excellent effects. But it was to be recollected, that patients in erysipelas seem sometimes to prosper under the most opposite plan of treatment; under antiphlogistic means in the hands of some, under bark and stimulants in the hands of others, and that pursued as a general and invariable practice. Such effects were calculated to lead one to doubt much the utility of constitutional treatment altogether in erysipelas; and to believe, that probably the disease generally progressed well, not from the type of constitutional treatment adopted, but in despite of it.

PUERPERAL FEVER.

Dr Arneth read a communication on a cause of the puerperal fever at the Lying-in Hospital of Vienna. (This paper appeared in full in the June Number of this Journal.)

Dr Moir, after referring to the obligation the Society lay under to Dr Arneth for his very interesting communication, proceeded to make some remarks on a few of the epidemics of puerperal fever in hospital and private practice, as detailed in the late Dr Hamilton's lectures, and on some facts tending to prove the contagious nature of that disease.

1. During 1814 and part of 1815, nine women had puerperal fever in the Edinburgh Lying-in Hospital, and only one recovered.

2. In 1830, between 18th September and 5th November, out of forty-eight patients delivered, eighteen had the disease in a well-marked form, of whom sixteen died; the two who recovered were the last cases, and insisted upon being taken home to most miserable apartments. They both slowly recovered,—one after the discharge of a considerable quantity of dark offensive matter per vaginam.

3. During the above epidemic in 1814-15, three cases occurred in the private practice of the matron of the hospital, who at that time was much employed as a midwife.

4. In Dumfries and neighbourhood, during 1813, twenty-seven individuals in the better ranks died from this affection in about six weeks; and only one affected with it recovered,—a discharge of pus from the umbilicus having in her occurred about two months after delivery.

5. In the summer and autumn of 1823, Dr Kellie had a number of cases in Leith and its vicinity. The first case occurred at Inveresk, early in May; and the fifteen patients whom he delivered in succession afterwards, or sixteen in all, were affected with the disease, and all died.

6. Dr Hamilton saw one of these cases in consultation on 6th May. On the 8th, he delivered a lady of her third child in Gayfield Square; she had the disease, and died on the eighth day.

7. In May and end of June of the same year, Dr H. saw two cases with the late Dr Beilby, one of whom recovered. On the 7th July, he delivered a lady of her first child, after an easy labour. On the fourth day she was seized with the disease, was bled copiously more than once, and sunk next day.

8. Dr M. mentioned several instances of individual pupils of the hospital having had a succession of cases in various quarters of the town; while other pupils, as extensively engaged at the same time in midwifery practice, and in the same localities, had none.

9. Dr M. read the copy of a letter, sent in Feb. 1824 to Dr H. by a Dr Reid, an old pupil, in which he says:—"That puerperal fever is contagious, I think I shall be able to adduce some tolerably good proofs. In 1817 I visited Ireland, and attended gratuitously a number of poor. The third woman I atttended in labour was attacked with puerperal fever; and from that time not one escaped it who came under my care. After my return to London in the autumn, I was called to a labour, on which occasion I happened to be dressed in some of the clothes I had worn in Ireland; and that woman was seized with the fever, as were two others, whom I attended a few days subsequently. On a voyage to New South Wales, several weeks afterwards, in the Atlantic ocean, a soldier's wife was taken in labour, and being badly provided with necessaries for the occasion, I gave her some old sheets, and a piece of garment, which had been packed in one of my trunks at the time I was attending the puerperal cases in London; and she was attacked with the fever two hours after delivery. Four other women were confined on the voyage, and all had the disease."

After commenting upon the contagious nature of this fearful disease, as indicated by the preceding facts, Dr M. stated, that it was mentioned in Dr Hamilton's lectures, that when puerperal fever prevailed in the lying-in ward of the

Royal Infirmary, all the sores in the surgeon's ward, which was on the same floor, were in a very bad condition; and erysipelas followed every operation, however trifling; and that Dr Kellie had also observed, that during his attendance on the cases occurring in his practice in 1823, there was inflammation of one or more veins of the lower extremities, accompanied in some with erysipelatous efflorescence on the surface. From which facts Dr H. had at one time concluded, that there was a strong connection between erysipelas and puerperal fever, though he changed this opinion in after-life, from not finding them always to go together.

Dr M. next related at length the particulars of three cases which had occurred lately in his own practice, and which in several ways deserved the consideration of those engaged in obstetric practice. He delivered a patient, A., on the 8th February, at midnight; another, B., early on the morning of the 9th; and on the 10th, in the afternoon, a third, C. The infant of A. having died early on the morning of the 10th, and it being wished to ascertain the cause of death, Dr M. opened the body that evening, some time *after* having delivered C., and, much to his surprise, found the cavity of the right pleura so completely filled with a sero-purulent effusion, that no air had penetrated that lung, which was otherwise quite healthy. He ordered the nurse not to approach his patient till she had changed her clothes, and washed carefully with chloride of lime; and he did the same. On the morning of the 14th this nurse was found feverish, and suffering considerable pain in one arm, from inflammation of the lymphatics. It was ascertained that she had pricked her finger a short time previous to the opening of the baby, some of the matter from which had doubtless been absorbed, and given rise to the above symptoms. Dr M. afterwards visited A. and B. about ten o'clock; and did not see C. till next day. On the evening of the 12th, A. had slight rigors; on the 13th and 14th, pulse was very rapid; but on the 15th fell to 80, and she seemed altogether better; but that evening rigors again supervened, the pulse became rapid and skin variable. Erysipelatous efflorescence appeared soon after on the left leg. Tympanites also supervened some days before death; but there was from the first little or no pain or uneasiness of abdomen, or of any part of the body, except the knee of the affected leg, and the back of the shoulder and right side for three or four days before death, which occurred on the 24th. The appearances on dissection were unusual, there being no trace of peritoneal inflammation or its consequences, and no apparent affection of the uterus, its appendages, or vessels, except redness of one of the Fallopian tubes, and in two small spots a substance, of a whitish colour, apparently like pus, where the placenta seemed to have been attached. The left knee-joint was distended with sero-purulent fluid. The muscles and cellular membrane on the back of the shoulder and right thorax were infiltrated with a dark sanious fluid. The large veins were distended with blood, and their walls thin and almost transparent, but their internal coat healthy. The blood in the great veins very fluid, watery, and uncoagulated; and, when examined microscopically, containing white corpuscles in greater numbers than natural.—B. was seized, early on the 13th, with rigor and violent pain of abdomen, which continued for nearly a fortnight; during which time effusion took place into one pleura, and soon after into the other; but, though exceedingly prostrated and worn out, she is still (about a month from the attack) alive, and it is hoped may recover.—C. was also seized with rigor early on the 13th; pain confined to the uterus, which was hard and painful on pressure; was leeched over it, and so far relieved that on the 15th her pulse was in the morning down to 90, and soon after mid-day to 72; yet the symptoms became aggravated, and she died on the 19th.

Dr M. called the attention of the members to the great difference of the local symptoms in all the three cases; the pain in C. being chiefly confined to the uterus; in B. to the peritoneum generally, the whole abdomen being exquisitely tender to the touch; while in A. there was neither pain of the abdomen nor of its contents during life, nor morbid appearances after death, with the slight excep-

tion already mentioned. And he lastly requested their particular attention to the probable exciting cause in the three cases, or to the manner in which the disease had been communicated; because believing, as he did, in its contagious nature, it appeared to him a question of great importance to trace out the cause. At first sight, it might appear that the most probable cause was the communication of some morbid matter from the opening of the fœtus, it being admitted that the application of the inflammatory products effused on mucous or serous surfaces may communicate the disease, as amply proved by Dr Arneth's paper, as already read; but, on more particular attention to the subject, there were several great difficulties in admitting this as the cause in these cases, as all the patients had been delivered *prior* to the opening of the body; as the hands were well cleaned with chloride of lime, and the clothes changed, before *two* of the patients were visited that night, and as the *third* was not seen till next day, and as in none was any vaginal examination made by Dr M. *subsequent* to the sectio. Dr M. then submitted what to him appeared, on a careful review of all the cases, as the cause. He believed that there must have been some peculiar condition of A.'s system, which had been not only the cause of the disease discovered in her infant, but also of the subsequent symptoms in her own case, and which had communicated to Dr M. some virus or *materies morbi*, which had been the means of inducing the disease in the other two patients. In support of this, there was,—1st, the state of the infant, which was well grown, and in every way healthy, with the exception of the sero-purulent effusion into the right pleura; and that this was dependent on some morbid condition of the mother, was borne out by there being, 2d, for some days previous to labour coming on, a feeling of occasional faintness and of listlessness, very foreign to her usual habits; 3d, by the difference in the nature of the labour, which on former occasions was generally very rapid, and without premonitory symptoms,—whereas, on the present occasion, besides supervening a fortnight before her full period, there was a threatening of labour for four days, during the whole of which time Dr M. was frequently in attendance on his patient, often for a considerable period at a time; 4th, by the scanty or almost total want of secretion of milk, whereas on all previous occasions it was very abundant; 5th, by the slight rigors, which in attacks of puerperal fever are generally more severe; 6th, by the total want of the usual local pain in the regions of the uterus, its appendages, or the peritoneum; and 7th, the appearances after death. From the preceding facts, Dr M. inferred that the disease in this case, A., was the result more of some primary morbid state of the system than connected with any local cause, such as the application of morbid matter to the vagina; that, moreover, this condition had existed prior to delivery, as indicated by the state of the infant, and the symptoms both prior and subsequent to delivery; and lastly, that, as he had been in such close attendance on this patient for four days, he most probably was the means of conveying some infectious matter to B. and C., the former of whom was delivered about two hours after he left A.'s house subsequent to her delivery, and the latter on the next day.

Dr Simpson expressed a similar opinion of Dr Moir's series of cases, to what Dr Moir himself had given—viz. that the original focus of contagion in them was to be traced to the diseased blood and tissues of the mother who was first delivered and first attacked,—that her blood had affected the infant which she carried within her,—and that probably the vaginal secretions and discharges from this said patient during labour had unhappily formed the virus or material which had been unwittingly carried by Dr Moir, so as to affect his other patients. It was only by careful and searching analysis of cases of puerperal fever, like Dr Moir's, when they did occur, that we could hope ultimately to arrive at a knowledge of all the various ways and means in which the disease may originate or be spread, and consequently of all the different means which may be adopted to prevent its spreading. Dr Hill of Leuchars has described one instance which was interesting in this respect, that, as in Dr Moir's, both the mother and the child seemed affected before delivery. A carpenter had his hand wounded and poisoned

with the discharge issuing from a dead body whilst placing the corpse in the coffin. A severe attack of erysipelas followed. Subsequently his wife had a similar attack of erysipelas. Their daughter living with them, and in the seventh month of pregnancy, was then taken with an attack of fever. In a day or two she gave birth to a dead child, whose body had all the appearance of being affected with erysipelas, as the arms of the mother's parents previously were. The mother herself died within twenty-four hours, with the symptoms of malignant puerperal fever. On his road home from visiting this patient, Dr Hill was called to a case of labour, and this other was also attacked with puerperal fever. Dr Arneth's very valuable paper adduced what was apparently incontrovertible evidence of puerperal fever being propagated in the way he suggested—viz. by medical men carrying on their fingers matter capable of producing it from bodies which they were dissecting, and inadvertently inoculating that matter into the mucous membrane of the vagina of patients in labour. In these cases, the fingers of the accoucheur acted like the ivory points or ivory lancets of the old inoculators and vaccinators,—that is, when once dipped in the poison they might retain it, till they had again inoculated that poison into the bodies of other healthy subjects. The vaginal mucous membrane was generally stretched and abraded in labour, the perineum was often slightly torn, and the whole, afforded a surface in a condition easily inoculable. But if students and practitioners, with their hands containing some portion of morbid matter act thus, by inoculating that matter on the abraded surface of the vagina, produce puerperal fever, no doubt, under similar circumstances, surgeons could and did inoculate into the wounds which they made or dressed, similar matter producing the similar disease of surgical fever in their patients. If it could be inoculated into the abraded surface of the vagina, it could be inoculated into a recent wound. If it produced fever in the one set of patients, it would produce fever in the other. And since bringing under the attention of the profession the communicability of surgical fever, Dr Simpson stated, that he had heard various facts in regard to it, all of which more and more convinced him that surgeons, like accoucheurs, were occasionally the unhappy media of inoculating their patients with morbid matter, producing in them surgical fever, as in puerperal patients, obstetricians, by the same means, produced in their patients puerperal fever. He had no doubt that it would take many long years fully to convince surgeons of this fact; but still it was his conviction, that surgeons would ultimately both believe and act upon it, and that their doing so would be a means of preventing many of the numerous deaths which now occur after operations, particularly in hospital surgical practice. The mortality in most lying-in hospitals upon the Continent of Europe was very much higher than the mortality in the same institutions in Great Britain and Ireland. He did not think that the diminished mortality amongst us was in any way owing to the fact of superiority on our part in obstetric practice, but it was owing to the fact of British accoucheurs generally having a belief in the contagious communicability of puerperal fever, and taking their measures accordingly. He was sincerely of opinion, that the want of that belief was a great cause of mortality in the continental lying-in hospitals generally, and that in them many lives were yearly sacrificed to medical prejudice, in the want of a proper belief, on the part of the medical men in charge of them, in the contagious communicability of puerperal fever; and in such a fatal disease it was ever to be recollected that prevention was a far mightier object than cure. Already, by Dr Semmelweiss' belief in the contagious propagation of puerperal fever, and by the hygienic means which that belief suggested, several hundred maternal lives have within four years been saved in the Lying-in Hospital of Vienna. If one hospital could afford such results in four years, how many maternal lives might be saved over the Continent of Europe, provided all the practitioners of Germany, France, Denmark, &c., could be as thoroughly impressed with the contagious character of puerperal fever as British practitioners and hospital accoucheurs now generally were. Continental accoucheurs generally did not understand exactly the kind or description of

evidence upon which British practitioners founded their belief in the contagious communicability of puerperal fever. Some of the continental writers on this subject seem to imagine that British obstetricians believed that puerperal fever was usually propagated directly from one patient to another, and not seeing this occur, when a puerperal fever patient, in their continental hospitals, lay by the side of another healthy woman, they imagine that from this fact they had a disproof of the opinion of the contagious communicability of the disease. But in this country we do not believe that the disease is usually propagated in this way, directly from individual to individual, but indirectly, through the medium of a third person, and that person generally the medical attendant or nurse. But that it was so propagated by the medical attendant or nurse, we further believe upon the following species of evidence—viz., that it was, as in Dr Moir's late cases, and in most other instances, distinctly and precisely limited to the practice of one or two practitioners only, out of a large number of medical practitioners, practising in a large community. Many examples were recorded, and many more unrecorded were known to the profession, of the disease being thus limited to the practice of a single practitioner in a town or city; all, or almost all, the patients of that practitioner being affected with it, where none of the patients of other practitioners were seized with any attack of the disease. In these cases we could not believe it to be owing to any morbific influence present in the air, or emanating from the locality in these cities or towns. For if so, it would affect indiscriminately the patients of all practitioners. But it had been often seen, as it was just now remarked, to haunt the steps of a single practitioner, and a single practitioner only, in a community. Many instances of this were known and published. One would suffice for illustration. Dr Roberton, of Manchester, tells us, that in 1840 upwards of 400 women were delivered by different midwives in connection with the Lying-in Hospital in Manchester. These 400 women were delivered in different parts of the town at their own houses: 16 of them died of puerperal fever; all the others made good recoveries. Their production could not have arisen from any general epidemic, or atmospheric, or telluric influence; for the fatal cases occurred in no one particular district, but were scattered through different parts of the town. Now these 400 and odd women were attended in their confinements by twelve different midwives. Eleven of these twelve midwives had no puerperal fever amongst their patients. The sixteen fatal cases had occurred in the practice of one only of the twelve. The disease in fact was limited entirely to her patients. There must have been something, then, connected with that one midwife, in which she differed from the other midwives, inasmuch as all her patients took the disease, whilst the patients of all the other midwives escaped from it. And in medical philosophy we cannot fancy that this something consisted of aught else, than some form of that morbific principle or virus, to which pathologists give the name of contagion. Further, that the disease is really in such instances propagated by this third person (the physician or the nurse) carrying to the parturient patients a virus capable of producing the disease, is shown by this kind of additional evidence;—That when the disease has broken out in the practice of one accoucheur, it will spread to the practice of others of his obstetrical brethren, provided they put themselves in a condition so as to carry off the contagious virus from the patients of the first practitioner. In 1836 or 1837, Mr Sidey of this city had a rapid succession of five or six fatal cases of puerperal fever in his practice,—at the time when the disease was not known to exist in the practice of any other practitioners in this locality. Dr Simpson, who had then no full and proper belief in the contagious propagation of puerperal fever, attended the dissection of two of Dr Sidey's patients, and freely handled the diseased parts. The next four cases of midwifery which Dr Simpson attended were all affected with puerperal fever, and it was the first time that he had seen it in practice. It was upon evidence of this kind that British pathologists generally reckoned in founding their belief on the contagious communicability of puerperal fever. And it was evidence of this kind which had intuitively driven them to adopt those means of prevention or avoidance, which are so highly necessary, in order to ar-

rest the propagation of this fearful malady. The measures proposed and so successfully adopted by Dr Semmelweiss in the Vienna Hospital, were beautiful from their mere simplicity, but they were full also of a great lesson to us all. They proved, in a manner beyond all dispute, the great importance of carefully ridding the fingers from all matters in the least degree likely to prove hurtful, if inoculated into the vagina of a puerperal patient. And no doubt, as Dr Arneth had remarked, such matters were always present in the fingers as long as, despite even of common ablutions, they emitted a disagreeable animal odour, the presence of that odour being a perfect proof of the presence of morbid matter capable of producing the odour. Drs Semmelweiss and Arneth recommended, for the purpose of ridding the fingers of this morbific matter, the use of chloride of lime. Dr Simpson had used for the same object for years daily (or rather generally often during the day), a solution of cyanide of potass, which was more effective even than chloride of lime, and had this other advantage, that it removed readily and at once all such stains as the fingers of the accoucheur were apt to receive in treating uterine diseases—with nitrate of silver, iodine, and the like. Dr Semmelweiss believed that animal matters, in a state of *putrefaction*, was the material which constituted the inoculable virus capable of being transmitted to puerperal patients, and of producing puerperal fever in these patients. Dr Simpson had strong doubts as to the idea of this matter being necessarily putrid being correct. We see cases in which animal substances are allowed to putrefy within the vagina, and to be applied to the mucous membrane of that canal, without producing puerperal fever. When a polypus, for example, was ligatured and left in the vagina, it often was killed and putrefied there for days before the stalk was completely cut through by the applied ligature. And yet in these cases the patient had little or no liability to attacks of disease like puerperal fever. Besides, in these cases, the other condition is present, of an abraded surface, as well as putrid matter in contact with that surface, for the vagina was sometimes no doubt more or less injured in its mucous surface while passing the ligature; and the ligature itself always made a raw, open, and inoculable surface, as it cut through the pedicle of the tumour. Surgery on other parts of the body admitted of many similar proofs against this doctrine. Dr Simpson had always believed and taught another theory, but not perhaps a perfectly correct one, in regard to the nature of the contagious material. He believed that generally, if not always, the material which, when carried from one subject to another, could produce puerperal or surgical fever in a newly inoculable subject, was an *inflammatory secretion*, just as the inoculable matter of small-pox, cow-pox, syphilis, &c., was an inflammatory secretion. The case adduced by Dr Arneth, of puerperal fever breaking out in the hospital apparently in consequence of matter being conveyed from cancer of the uterus to a series of puerperal patients, was not so strong an argument against this view as might at first sight appear. For the cancer patient was, according to Dr Arneth's own account, several days in labour, the carcinomatous degeneration of the cervix preventing the opening of the os. And there can be very little doubt, that by the end of several days, the carcinomatous structures were in a state of inflammation, and probably gangrenous decomposition, from the protraction of parturition. At all events, if the carcinomatous cervix was really putrid, it was in all likelihood putrid from the result of gangrenous inflammation in its compressed and irritated structures. But be this the case or not, it was important to remark, that obstetricians had now very decided proof of various kinds of morbid matters which were capable, when inoculated into the vagina, of leading on to puerperal fever. For, first of all, when the bodies of patients who died of puerperal fever were opened, the inflammatory effusions in the abdomen and elsewhere, when brought in contact with the fingers of the accoucheur, were capable of producing the same disease in other healthy patients, upon whom they were accidentally inoculated. In other words, the morbid effusions of puerperal fever in one woman were capable of producing puerperal fever in another woman, when inoculated into her system. But secondly, the same seems to hold true with regard to the secretions coming from the bodies of such patients, even when they

did not die and were not dissected. Dr Simpson alluded to the cases, for example, of nurses and midwives, whose fingers came into contact with the discharges from the vagina of puerperal patients, giving the disease to other parturient women, and who had not of course, in the way of post-mortem examinations, been bringing their fingers in contact with the more internal secretions. Dr Gordon mentions more than one case of this kind in relation to midwives, in his history of the Aberdeen puerperal epidemic. Thirdly, he believed that the cases recorded by the late Mr Storrs, Hutchinson, Ingleby, and others, sufficiently proved that the inflammatory secretions in some other inflammatory diseases besides puerperal fever, when carried by the medical attendant, and inoculated into the maternal canals of a parturient female, were sometimes capable of producing in such females true puerperal fever. This seemed more particularly true with regard to the inflammatory effusions in erysipelas and gangrenous inflammation of the limbs, scrotum, vulva, or other part of the body. That the morbid matters thrown out in those more subacute forms of disseminated or phlebitic inflammation, which sometimes occur after delivery, were capable of producing puerperal fever when inoculated into puerperal patients, was a fact of some importance to hold in view. And the following recent case will perhaps impress the truth of it. A short time ago, Dr Simpson was requested to see a case of pelvic abscess in a patient delivered four or five weeks previously. The abscess was artificially evacuated, but only with partial relief; as there were evidently other local inflammations going on, both in the abdomen and chest. The patient died about six or seven weeks after delivery. The practitioner who originally attended her, and who had no puerperal fever cases in his practice, was not able to be present at the dissection. Another able medical practitioner—whom he had called to the case after the inflammatory attack had begun—opened the body. Though an excellent and well informed physician, he rather decried any fear about the possibility of contagion, when Dr Simpson suggested it to him as he came into the room, and found him opening the body. This gentleman had no puerperal fever cases in his own practice; but within fifty hours after opening this body, he happened to be called to five cases of midwifery. Four of these patients were attacked with puerperal fever, three in a very severe, and one in a mild or abortive form. The fifth patient altogether escaped, the child having been born before the practitioner's arrival. Fourthly, there were one or two recorded circumstances which would lead one to the belief that some varieties of febrile exhalations received by inhalation into the blood of a newly delivered woman, are capable of producing in her a disease analogous to, if not identical with, puerperal fever, the effect being the same as if morbid matter had been introduced into her blood, not by inhalation into her lungs, but by inoculation and imbibition into the vagina, just as in the spreading of small-pox we see the disease liable to be produced in two ways,—first, by the direct inoculation of the morbid inflammatory matter contained in pustules on the arm of a healthy individual; or secondly, by individuals inhaling the morbid effluvia from the bodies of patients labouring under the disease, without being inoculated into them. Dr Collins mentions an instance in which a patient was admitted into the Dublin Lying-in Hospital, labouring under a bad form of typhus fever. Two puerperal females, who occupied the adjoining beds, were attacked with puerperal fever, and died. In another instance, in the same hospital, a similar accident happened. A patient labouring under typhus fever was admitted into one of the small wards of the house, which contained only some four beds,—all the three other women were attacked with puerperal fever, and two of them died. But we had no very decided evidence, as far as Dr Simpson knew, from hospital observation, that a woman, labouring under puerperal fever, could, by the exhalations from her body, infect with the same disease other patients lying near her in the same ward. Fifthly, some accoucheurs believe in the possibility of the imbibition of the effluvia from typhus or puerperal fever patients by the clothes of the medical attendant, and that the subsequent inhalation of such matter by the parturient female, might be a means of artificially infecting that female with the disease. Dr Simpson could not

doubt that the saturation of the bed-clothes, &c., with the discharges of a puerperal fever patient, might give the same disease to another puerperal patient who was laid in them. This, and one or two other circumstances, were enough to show that, for safety's sake, it was always well to act upon the possibility of the clothes even of the medical attendant being thus a medium of contagion. In some observations on the subject of the contagion of puerperal fever, Dr Merriman states, that he once attended the dissection of a puerperal patient, but did not touch the body or any of the parts. The same evening he attended a lady in labour, and she was attacked with the disease. In his account of the Aberdeen fever, Dr Gordon mentions that a man-servant appeared to carry the infection of the disease from his sister in Aberdeen to his wife in the parish of Fintry, six miles from Aberdeen. The midwife who attended this woman infected two other parturient patients in the same parish soon afterwards, both of whom died. If a statement of this kind could be established as a fact, by careful analysis of the requisite evidence, it would be a matter of importance, as adding to our knowledge of the modes in which this disease may be propagated. In the instance which Dr Moir had mentioned, of Dr Hamilton visiting the patient of another practitioner affected with puerperal fever, and immediately after having several cases in his own practice, it was not at all unlikely that he had made some examination of the patient, or, at all events, without proof that he had not, it would not be proper to conclude that the disease in that instance could be carried by the clothes of the physician acting in the way of *fomites*. The history of the other case adduced by Dr Moir, of the fever breaking out on ship-board, when bed-clothes had been used which had been employed previously in the beds of women who had died of puerperal fever, would be exceedingly important in the way of proof, if it had been more substantially reported and authenticated. One can scarcely believe that such clothes should be shipped off after being thus used, without having been previously thoroughly washed and cleansed. Dr Simpson had also been informed of an instance by Professor Patterson, in which a medical gentleman, after having lost several cases of puerperal fever, got rid of the disease in his practice by changing his clothes, and using chloride of lime, &c., but it again returned to him when he happened to deliver a patient, immediately after wearing a pair of gloves which he had used during the time of the puerperal epidemic; and certainly, if there was any piece of dress more apt to retain the contagion than another, it was this useless and superfluous appendage to our attire; for it might retain the morbid secretions that were originally on the fingers of the accoucheur, just as our vaccinating glasses would retain the cow-pox matter. Again, in a small ward or small hospital, one could almost, as it were, produce puerperal fever at will, by crowding a great number of puerperal patients together in the same ill-ventilated room. The discharges from the different patients in a few days render the air of such a room, so loaded and morbific, as to be oppressive to all entering it, and capable of producing febrile action by the inspiration of it, in those puerperal patients who occupied its beds. This no doubt was true when this experiment was driven, as it sometimes accidentally had been, to an extreme. But it was true also in its lesser degree; for Dr Simpson believed that one great cause of weed, ephemera, and febrile attacks during puerperal convalescence, was the still too slight attention that was paid to the ventilation of the lying-in chamber. He had repeatedly, he thought, seen more or less slight febrile action set up in a patient, from her curtains being closely drawn around her bed for eight or ten hours during the night, being thus obliged to breathe an air loaded and affected with the morbid animal discharges from her own body. Dr Arneth had not alluded to the question, Whether the disease was ever caused or not, or a predisposition at least given to it, by epidemic influence? Dr Simpson believed that we ought not to forget altogether the possibility of epidemic influences acting directly or indirectly in the causation of it. During the present century the disease had nearly, in two or three instances, as in 1819-20 and 1829, prevailed in most of the cities and lying-in hospitals of Europe. And it was difficult or impossible to account for this simultaneous existence everywhere,

without believing that everywhere there was some general epidemic cause tending to its production. In this the history of puerperal fever did not differ from the history of other contagious febrile diseases. During the latter part of the last century, for instance, small-pox contagion existed in almost every town and village in England, because in almost every one of them there were artificial causes operating to produce and perpetuate the disease, inoculation being very generally practised. But it was only in particular years, and sometimes at a considerable distance of time, that the disease became epidemic. And when it did so, it was owing to other causes being in action in addition to the mere inoculation. Nay more, in some conditions, as during the blowing of the Harmattan wind, we know that small-pox and cow-pox cannot be propagated even by direct inoculation,—facts, showing us the influence of epidemic constitutions in effecting a greater or less tendency to the production and spread of particular diseases. One predisposing cause to attacks of puerperal fever, was no doubt the state of the constitution of the patient immediately after delivery. Dr Collins' cases in the Dublin Hospital showed, not only that the disease was far more apt to attack those who were worn out by long labours, than those women who had escaped with parturitions short in their duration; but also, that the malady, when it did appear, was much more fatal in the former than in the latter class of patients. The Society was aware that it had been proposed by various pathologists of late years to give various prophylactic medicines to puerperal patients after delivery, and to surgical patients after operations, in order to prevent the attacks of puerperal or surgical fever. All these measures, such as sulphate of quinine, muriate of iron, &c., had the object in view of strengthening the constitution of those to whom they were exhibited, so as to diminish or destroy the predisposition to these feverish attacks. And we could understand their proposed mode of action when we reflected upon the fact, that a predisposition to such attacks was given by any unusual degree of exhaustion or debility in the patient. Every patient exposed to the contagion, and even to the inoculation, of small-pox, for example, did not take small-pox. There were other means by which the predisposition to that disease was reduced or removed, than by previous variolation or previous vaccination. And perhaps, particularly, or otherwise, by medicine, we may be able to reduce or remove the predisposition to puerperal fever, as well as to scarlatina, measles, &c. Lastly, Dr Simpson observed, no doubt sporadic cases of puerperal fever did, and were from time to time occurring, traceable to no contagion or any other cause capable of being averted; but owing, as in Dr Moir's first case, to morbid actions going on in the constitution of the patient even before delivery; or to morbific agencies capable, under other circumstances, of producing fever or inflammation acting upon the patient in delivery. Dr Arneth had particularly called the attention of the Society to the connection which was generally believed by British accoucheurs to exist between erysipelas and puerperal fever; and he had stated that the relation between these two diseases had not been observed in Vienna. Dr Simpson, however, expressed his opinion, that now that Dr Arneth's attention had been directed to it, he and his compatriots would find such relations existing between these two diseases, which English accoucheurs spoke of. We, all of us, often overlook such facts in pathology till our attention happens to be prominently called to them. Dr Simpson had long believed and taught that there was a pathological connection between the two diseases in question, as to their pathological nature, their pathological anatomy, their symptomatology, and their causation. The two diseases had in Britain been repeatedly observed to prevail at the same time, in the same town, in the same hospital, or even in the same wards. There were various accurately recorded instances in our British journals, which he had already alluded to as showing this,—that when the fingers of medical men were impregnated with the morbid secretions thrown out in erysipelatous inflammation, the inoculation of these matters into the genital canals of our parturient females, produced puerperal fever in them in the same way as the inoculation of the secretions from patients who had died of puerperal fever itself. The effused morbid matters in the one disease, as in the other, were capable of pro-

ducing the same effect when introduced into the vagina of a puerperal patient. In an instance recorded by Mr Hutchinson, two surgeons, living at ten miles distance from each other, met half way to make incisions into a limb affected with erysipelas and sloughing. Both practitioners touched and handled the inflamed and sloughing parts; and the first parturient patients that both practitioners attended within thirty or forty hours afterwards, in their own distant but respective localities, were attacked with, and died of, puerperal fever. The late Mr Ingleby mentions an instance of a practitioner making incisions into structures affected with erysipelas, and going directly from this patient to a patient in labour. This patient took puerperal fever and died. And within the course of the next ten days, seven cases of puerperal fever occurred in the practice of the same practitioner, almost all of them proving fatal. And various other cases, similar to the preceding, were well known to the profession. Again, however, the reverse of this was equally true. Not only was the morbid matter in erysipelas apparently sometimes capable of producing puerperal fever, but the secretions and exhalations from puerperal fever patients seemed, on the other hand, sometimes capable of producing erysipelas. In the series of puerperal cases met with by Mr Sidey in this city about 1837, and which had already been alluded to, the morbific matter carried from two or three patients seemed, as has been previously stated, to produce the disease, both in Dr Simpson's own practice, and also in the practice of Dr Paterson. The morbid effusions of these patients created the same disease of puerperal fever in other patients to whom that matter was carried. But the morbid secretions and exhalations from these same patients appeared to do more—viz. they produced also erysipelas in several of the nurses, relations, and attendants upon the patients. Four or five cases of erysipelas followed upon a single one of these puerperal cases in Mr Sidey's practice, and that during the week subsequent to the puerperal patient's death. The patient's mother-in-law, who was in constant attendance upon her, was attacked with fever and erysipelas in the face and head. One of the patient's sons, a boy five years of age, was attacked with erysipelas of the face; a daughter was seized with fever and sore throat, with dusky redness, which continued for some time; and the patient's sister-in-law was attacked with acute gastric symptoms, and great abdominal irritation, under which she sunk in a few days. Here we have apparently the same focus of contagion producing puerperal fever in puerperal patients, and erysipelas, inflammatory sore throat, &c., in patients who were not in a puerperal state. Dr Hill, of Leuchars, had published in the Monthly Journal of last year two very important series of cases, showing, in a similar way, the connection between puerperal fever and erysipelas, in the identity of the poison that was capable of producing these two diseases. We have already seen that the inoculation of the morbid matters from erysipelatous structures into parturient patients will occasionally produce puerperal fever. But furthermore, the converse of this is so far also true, viz., that the inoculation of morbid matters or secretions from puerperal fever patients into other healthy individuals, will occasionally produce in the latter attacks of erysipelas. A considerable number of instances have been published by Dr Duncan, Mr Travers, and others, in which medical men have died from punctures received in dissection, or rather from erysipelatous inflammation of the arm and side following such punctures. The history of a large proportion of these cases shows further, that the matter thus inoculated, and which produced the fatal erysipelas and fever, was a puerperal fever secretion; as in most instances the disease resulted to these medical men from opening the bodies of patients who had died of puerperal fever. Whilst thus arguing for some pathological connection between erysipelas and puerperal fever, Dr Simpson further stated, that though, in a few cases, patients labouring under puerperal fever had been attacked with erysipelas of the skin and cellular tissues; and on the other hand, patients who were delivered when suffering under erysipelas, were sometimes subsequently attacked with puerperal fever, yet these results were not always observed. He alluded to cases where erysipelas had attacked women before delivery, but was not followed by puerperal fever. And he had seen more than one patient labouring

under puerperal fever have inflammation of the skin, but that of a pustular type, like ecthyma, and not of an erysipelatous character. In stating this, he wished to express his belief, that the diseases were not in all respects pathologically identical, though the morbid secretions in the one were capable of producing, in those predisposed to it, the other disease, — erysipelatous effusions producing puerperal fever, and puerperal fever secretions producing erysipelas. Other febrile and inflammatory products, beside those of puerperal fever, when inhaled through the lungs into the blood, or inoculated into the blood through the vagina, may, as already stated, probably produce puerperal fever in addition to those we have principally spoken of, viz., the secretions from puerperal fever and erysipelatous patients. And he thought the whole subject one of extreme importance to have more thoroughly investigated ; because, in a case like puerperal fever, it was the means of prevention that we were to look to, and to expect success in, more than the means of cure. It was here, as elsewhere, evident that human life would probably be saved to a far greater extent by studying the means of preventing the causation of disease, than by any study of the means of treatment, after disease was once actually commenced. And when it was remembered that about 3000 women still died in child-birth in England and Wales alone every year, and that a large proportion of these 3000 maternal deaths were deaths from puerperal fever, he thought he need not make any further observations on the importance of studying the means of prevention in such a fatal and formidable disease, nor offer any apology for the length of the imperfect remarks which he had ventured to offer on this important subject.

Dr Peddie said, that he had given a good deal of attention to the subject of puerperal fever, from the occurrence of some cases in his practice, upwards of five years ago; but at that late hour would confine his remarks to two or three of the points under discussion. He quite agreed with Dr Arneth in his excellent remarks on the danger of obstetrical manipulation, after being recently engaged in post-mortem examinations. That, he thought, was now a pretty generally admitted opinion in this country. Dr Robert Lee of London, Dr Merriman, and others, had, many years since, mentioned cases in proof of this source of danger; and, of late, accoucheurs in this city had exercised much caution. To point out still more strongly the risk of accoucheurs handling morbid preparations, he would notice what he believed to be a fact, that the series of puerperal fever cases which Dr Simpson had mentioned as occurring in his practice some years since, from assisting at an autopsy of one of Dr Sidey's puerperal cases, did not end there, but that a practitioner in Leith, having examined, in Dr Simpson's house, a portion of the uterus obtained on that occasion, had immediately thereafter three fatal cases of the fever in his own practice. With regard to the power of the solution of the chloride of lime as a disinfecting wash for the hands of those engaged in midwifery practice, Dr Peddie had no doubt; and in that belief he had personally used it much. He said, that it might be in the recollection of many of the members of the Society, that the cases of puerperal fever which had occurred to himself, and regarding which he had then read a communication, took place in two series. The first series of these cases occurred in such a way as to take him by surprise, and before the necessity for precaution was suggested. Now, however, he changed his clothes, and washed often with the solution of the chloride of lime; and afterwards, at intervals of a few days, delivered three patients who made most excellent recoveries. The first patient, however, of the second series was a personal friend of the last patient of the first series, resided close by, was with her during her confinement, continued visiting for some days after the fever had shown itself, in spite of urgent remonstrances, and even assisted in the sick-room arrangements, changing articles of clothing, removing evacuations, &c. Thus, in Dr Peddie's opinion, she obtained the fatal virus direct from her friend, and anew communicated it to him, who, in turn, unwittingly was the medium of propagation to another patient within the next twenty-four hours. Since then, Dr Peddie had been much on his guard when engaged in surgical dressings, when attending bad cases of fever, erysipelas, or

post-mortem examinations, considering it his duty to wash well thereafter with the solution of the chloride of lime. Whether in consequence of these precautions or no, he had not since met with any such cases in his practice. As to the question of the possibility of puerperal fever being propagated through the medium of clothing, Dr Peddie mentioned a case, reported in the "Provincial Journal" (No. 166), as occurring to Dr Allen of York. The doctor, after seeing the last of a long list of fatal cases, was surprised to find a new instance of the disease, two months afterwards, in the practice of his assistant. It was ascertained, on investigation, that that gentleman had put on the pea-jacket which Dr A. had last worn when in attendance on a case of the fever. Dr Peddie was not prepared to assert the correctness of this explanation; but thought that it was better that accoucheurs should not dismiss from their minds the possibility of this fearful malady being communicated by articles of dress.

EDINBURGH OBSTETRICAL SOCIETY.

MEETING IV. — *March* 26, 1851 — (*Continued.*)

CHLORIDE OF ZINC IN ULCERATION OF THE OS AND CERVIX UTERI. BY DR PEDDIE.

Dr Peddie wished to call the attention of the Society to the power of the chloride of zinc as a caustic-alterant in ulceration of the os and cervix uteri. In the aphthous or slighter forms of excoriation and erosion, the sulphate of copper and nitrate of silver are generally found sufficient; but the latter has little effect on those ulcers most generally brought under observation, as it very slightly affects the vitality of the tissues concerned, and fails to impart a healthy action to the sore,—which in this situation evinces a peculiar tendency to chronicity—presenting a mixed character of indolence and irritability. The nitric acid is of higher value in such cases; and the actual cautery ought to be still more potent, but cannot be so conveniently employed. The caustic, therefore, which has been of greatest repute is the potassa fusa. From the experience, however, which Dr Peddie had had of the chloride of zinc, in the treatment of ulcers on other parts of the body, as well as of the uterus, he was confident that, on a more extensive trial, in sores affecting the latter organ, it will be found, in the majority of cases, as efficacious as the potassa fusa, and less hazardous in application, in respect to the integrity of the neighbouring parts. It is a favourite remedy of Professor Syme, in rebellious, pseudo-malignant ulcerations on the surface of the body, and it was likewise much employed by Mr Liston. It was in consequence of observing, upwards of two years ago, its efficacy in the case of a very frightful-looking ulcer of the penis, which was slowly extending, in spite of a variety of applications, that its utility, in ulcers of the uterus, was suggested; and since then, Dr Peddie had employed it frequently, with invariable success in such cases. In the application of the chloride of zinc to ulcers on the surface of the body, the usual mode is to mix it, in the form of a powder, with a little flour, and work it into a paste when it has deliquesced sufficiently. It is then spread on a piece of lint the size of the ulcer, and applied for some hours, after which the part is poulticed, &c. Dr Peddie has, however, used it of late in such cases, like the nitrate of silver, solid, and with great benefit; and, of course, this is the only mode in which it can be applied safely to uterine ulcers. Hitherto he had applied a small piece of the cake of the chloride, by means of a pair of long forceps through the speculum; but he had no doubt that the caustic could be prepared in moulds, like the nitrate of silver.[1] The caustic is hard, and

[1] Since then, Messrs Duncan, Flockart, & Co., have procured it in sticks, which may be very conveniently applied.

will not crumble down in the instrument with which it is applied, and may be used freely and leisurely ere it can deliquesce to an inconvenient extent. The effect of the application is soon visible, as, like the nitrate of silver, it turns the part white. This Dr Peddie considered an advantage over the potassa fusa—the effect of which, on the darkish coloured tissues, is not so apparent, and consequently the operator is not so sure when enough, or when too much, has been applied. But another and greater advantage is, the less tendency which it has to run over upon the surrounding parts; and, when this does happen, there is also very much less disposition in the fluid to excoriate the neighbouring healthy textures—which, it is well known, the liquor of the potassa, mixed with the sanguinolent exudation from the rapidly decomposed surface of the ulcer does,—eating immediately into the sound as well as the unsound parts. Should, however, any fluid flow beyond the margin of the ulcer, all that appears necessary is to soak it up with a dossil of lint, introduced by another pair of forceps—a process more simple and manageable than the vaginal washings with dilute acetic acid, which are required immediately after the use of the potassa fusa, to protect the healthy parts from injury. The chloride of zinc burns much more deeply than the nitrate of silver, and not so much as the potassa fusa; but enough, in Dr Peddie's opinion, to destroy the unhealthy surface, and to establish a new action in the sore. There is little or no pain in its application in this situation, and very trifling afterwards; and when the slough is cast off—which generally happens in from three to six days—healthy granulation is apparent, and cicatrisation goes on rapidly thereafter.

Dr Simpson stated that last year he had chloride of zinc made up for him, for applying to unhealthy ulcerations of the cervix uteri, by Mr Hunter. But instead of using it as Dr Peddie did in flat plates, he had then got Mr Hunter to run it into moulds—so as to be formed into round sticks like those of nitrate of silver or potassa fusa, when it could be much easier used. When quite pure, it very rapidly deliquesced and was very apt to run. As, however, we can always readily restrain and arrest potassa fusa from spreading by partially filling the speculum with vinegar, and injecting freely acetic acid into it immediately after the potassa was applied, so he found that chloride of zinc could be similarly rapidly decomposed and restrained by using, instead of vinegar, a solution of carbonate of soda. Less pure preparations of the chloride, like the potassa cum calce, did not deliquesce so rapidly, and he believed Dr Peddie had used such a preparation. He doubted if a slough separated by chloride of zinc left a more healing granulating surface than a slough separated by potassa fusa, or potassa cum calce. But he believed chloride of zinc would be found a manageable caustic, particularly if the neighbouring parts were protected with an alkaline solution,—or the preparation were made impure by being mixed with other material so as to prevent it deliquescing very rapidly: it was usually sold in this impure form.

Dr Simpson read the following communication to the Society:—

CASE OF FŒTAL MONSTROSITY, IN WHICH THE PLACENTA WAS AFFIXED TO THE REGION OF THE ABDOMINAL WALLS, &c. BY MR DICKSON.

Mrs D., aged 37, was recovering from an attack of influenza of six weeks' standing. Was in the seventh month of her ninth pregnancy. Quickened six weeks ago, and, since then, frequently felt the child's motions unusually active and severe, and always towards the *right* side.

Nothing particular was remarked in any of her previous labours.

About 5 P.M. of July 4, 1850, without any known exciting cause, she was seized with pains like those of labour, each pain being followed by more or less discharge of blood from the vagina.

Visited her next morning at 4 o'clock. Found the pains weak but regular, and increasing in frequency; the hemorrhage small, no fever nor sickness. Abdomen unusually large for period of pregnancy. Os uteri high, and dilated to the size of a shilling; membranous bag protruding; unable to detect presenta-

tion. Until half past 6, the pains continued with increasing energy and frequency, having the effect of opening the os to the size of a crown piece. The bag now protruded a great way into the vagina, and, during a strong pain, it was forced externally. Could not feel presentation through it, although, in the intervals between the pains, I felt a soft resistant body beyond it, which it was impossible to diagnose with certainty. In an hour afterwards, I ruptured this bag, and a great quantity of watery fluid escaped, which I at first thought was the liquor amnii. On examining, to ascertain what presented, was surprised to find another bag almost as large as the first, protruding from the os; but no part of the child could be detected. I then supposed that the first bag contained the hydroperione, and the second the true liquor amnii; or else, that each was the separate membranes of separate fœtuses.

During the next two hours there was an abatement in the severity and number of the pains, and the woman had a short repose.

About 10 they recommenced with increasing activity, and the bag protruded further at every uterine contraction. The os being now fully dilated, and, as I conceived that the bag was hindering the process of labour, I ruptured it, and a very large quantity of fluid escaped. The abdominal tumour subsided considerably after this. Introducing the finger again, to know the presenting part, I was doubly surprised on meeting with a third bag. This, however, was denser than the others; its coats had a thicker and rougher feel, and the uterine pains had no effect upon its resistance. On tracing it upwards, keeping to its lower side, the finger came in contact with the apex of the scapula of the fœtus, and searching about, I found an arm. The exact position of the fœtus was this: its head was placed towards the left foramen ovale, and its inferior extremity towards the right iliac symphisis. Thinking it was necessary to turn the child, in order to effect delivery, I proceeded in quest of the inferior extremity. Carrying the finger along the back (*i. e.* from the *left* to the *right* side of the mother), I found that the bag last referred to was firmly attached to it. In feeling for the genital organs, I could not detect the slightest irregularity of surface to mark their presence in the usual position. The surface of the abdomen was occupied by a large, soft, and yielding fleshy mass. The right knee was easily found; but, on attempting to pull it round, I felt that the whole fœtus was firmly attached to some hard resisting body. Being afraid to use forcible extractive measures, I left the case entirely to nature. A strong pain brought the left knee into the vagina, which was quickly followed by the right one, and a few more expelled the child externally. The extrusion of the placenta was simultaneous with that of the child. Little or no hemorrhage followed, and the woman recovered perfectly in a few days. The child lived for a quarter of an hour after birth. The fœtus was about thirteen inches long. Its heads, arms, and inferior extremities were naturally formed, and proportionate to its length. The chest was small and contracted. The tumour on the back was situated over the lumbar and lower dorsal region. It contained about two ounces of serous fluid. The cyst was quite closed, and no communication existed between it and the vertebra.

The placenta was of great size, being fully twelve inches in diameter; its lobes were undistinguishable. From the centre of its inner (*i. e.* fœtal) surface, a number of large vessels issued, which, after being loosely collected together for about three quarters of an inch, entered a structure immediately to be described. On lifting up the placenta, the abdomen was found open, the anterior parietes being entirely absent. Occupying their position was a large solid tumour, not unlike the liver in appearance and texture, and connected to the longitudinal fissure (but more particularly to the adjacent part of the lesser lobe) of that organ by a band of peritoneum (?) of about half an inch in length, which enclosed a number of vessels. These vessels proceeded from about the centre of the posterior surface of the tumour. The vessels from the placenta entered its inferior margin, and were soon lost in its structure. The liver, bowels, &c., lay beneath it. The diaphragm was imperfect on the right side, and the lung was

in immediate contact with the liver. The kidneys were in their normal situation, and the ureters proceeded to the bladder as usual. On the anterior surface of this viscus (the bladder, which ascended for fully one and a half inches above the pubis, and was quite exposed) was a bluish spongy body resembling the penis in structure, and on either side were two small whitish glandular bodies like the testicles, from which issued a congeries of tubes or vessels, which joined the two crura of the penis below. There was no appearance of external genital organs in their usual place. The arms were perfect.

I was not allowed a dissection of the fœtus.

MEETING V.—*April* 9, 1851.—Dr SIMPSON in the Chair.

ON THE OBSTETRIC PRACTICE OF THE VIENNA LYING-IN HOSPITAL. BY DR ARNETH.

Dr Arneth read the following communication to the Society:—

In laying before this Society the following communication, my only object is to excite discussion on any of the more interesting topics in midwifery which I shall have occasion to notice. And I shall, in commencing, apologise for those imperfections in it arising from my comparative ignorance of the English language.

I have now been connected for three years with the Lying-in Hospital in Vienna, as assistant to one of the Professors of Midwifery in that institution, in whose wards more than 10,000 women have been confined during that time. In the other midwifery clinique, in the same hospital, the number of labours during the same period was still greater; and, through the kindness of my colleagues, I had an opportunity of observing all the important and interesting cases which occurred in their department.

In a woman affected with *abnormal vertical inclination of the pelvis*, there occurred a *central laceration of the perineum* during labour, and through which the child was born. The fourchette anteriorly, and the anus posteriorly, were uninjured.

In the cases of two women, there occurred *prolapsus* of the womb at the full term of pregnancy,—in the one complete, in the other incomplete. In both cases reposition of the mass was effected, and labour soon supervened.

There were forty cases of face presentation, and in these thirty-five children were born alive; but of these last, four died in the first days after birth. Two of the mothers died. In no case was instrumental interference resorted to; and for fifty years such has been the practice in this hospital.

Twice (once after a very easy forceps case, and once after a natural confinement) a small part of the os uteri, covered with exudation, was expelled with the discharges. In the second case alluded to, the woman was examined on the ninth day after confinement, and there was found a sharp edge in the os uteri, indicative of the spot where the portion had separated. Both mothers did well.

A patient, in whom the portio vaginalis had been amputated two years previously, became pregnant. The os uteri did not open spontaneously at all when labour came on. Incisions were made in different directions, and shortly afterwards delivery took place. The mother sank ten hours after the birth. Post-mortem examination showed old adhesions of the uterine peritoneum.

Incision of the cervix uteri was required only in a single other case, one of carcinoma of the cervix. In this case the mother did well.

Two cases of carcinoma uteri occurred. Living children were born, and the mothers made very good recoveries.

Cæsarean section was never performed, except in cases where we believed the narrowness of the brim of the pelvis to be so great, as to render it impossible to drag the perforated child's head through it with safety to the mother. So strictly has this rule been kept, that out of somewhat upwards of 200,000 confinements

that have occurred in the Lying-in Hospital of Vienna, between the years 1789 and 1851, the Cæsarean section has only five times been resorted to. All the five mothers died. The children survived the operation, and of some of them there were accounts some years afterwards. Professor Dubois has informed me that he had performed the operation twelve times without having had any successful case; nor is he aware of any practitioner in Paris having been more fortunate. Professor Stoltz, of Strasburg, lately informed me that he had operated four times out of six with good success,—nay, in one case, he was even fortunate enough to see a mother twice surviving this dreadful operation. Everywhere, on the Continent, it is an established rule to perform Cæsarean section in every case of a mother dying pregnant in the seventh month, or later. The essay of Bonchut, which has lately been crowned with the prize of the French Academy, proves that with the last pulsation of her heart the mother dies, and that, consequently, the operation of Cæsarean section may be performed immediately afterwards. In four cases, children were saved by the operation in the Vienna Lying-in Hospital. Two of the mothers died of phthisis, one of eclampsia, one of gangrenous inflammation of the cellular tissue of the neck.

It is a curious fact, that whilst, in all cases of midwifery, we are bound most carefully to save the lives both of mother and child, and when that becomes impossible, to do everything in our power for the benefit of the one life we hope to save,—that in cases of mortal wounds inflicted upon the mother, or of her being in articulo mortis *for a considerable time* (where the mother is unconscious, and where there is not the least hope to see her restored), accoucheurs too often delay the operation till unfortunately the child in utero is dead. We are of opinion, that in such cases, and more particularly if we ascertain that the child's pulse is becoming slower and weaker, the Cæsarean operation ought immediately to be performed.

A very frequent cause of the child's death during labour is prolapsus of the funis. We are of opinion that, under certain conditions, the following mode of management has been, generally speaking, much neglected: we mean the artificial replacement of the funis. When the head is presenting, and still moveable on the brim, and the os fully dilated, the best plan is to push the funis back, and lay it down in the hollow of the neck of the child. The registers of the Vienna Lying-in Hospital contain forty-three cases of this reposition of the funis. In these, thirty-eight children were born living; and it is only fair to state, that in three out of the five cases of failure, there was scarcely any pulsation in the cord at the time when it was pushed back; and in one other case where the operation had been performed, the labour proved so slow that the forceps were applied for inertia uteri. No hospital report, as far as I know, affords better results of the treatment of a large number of cases of prolapsus of the funis.

The whole prolapsed loop of the funis is taken in the hand, and, pushing the child's head a little back, we lodge it in the hollow formed by the child's neck. It is not necessary to say, that the funis is in this way protected on one side by our hand, and on the other by the hollow of the neck, in which it is allowed to lie. There is no reason to fear the occurrence of any injury from compression of the child's head. This operation does no injury to the mother; and, by exciting the uterus to fresh contractions, it sometimes renders the confinement much shorter.

In the cases which have come under my care, the funis, after being replaced, has prolapsed again in only one instance. It was again replaced with complete success. I know, however, the particulars of one case of frontal presentation, where the funis so tightly embraced the forehead of the child, that reposition was found impossible.

Sometimes, when it is impossible to feel any pulsation in the prolapsed funis, the child may be still alive. Four such cases came under my notice, where the funis had been pulseless for so long as half-an-hour, and the children were born living. As there is rarely time, in such cases, to make use of auscultation, my opinion is, that in all cases fit for reposition it should be performed, unless the

funis be very cold and discoloured, or other decided symptoms of the child's death be present.

In the 6608 cases that were under my care, operative interference was resorted to 95 times,—that is, nearly once in every 70 cases; 11 of the 95 mothers died,—that is to say, 1 out of every 9.

62 Children out of the 95 were born living, and 49 survived the ninth day after their birth,—that is to say, the day when they used to be dismissed from the institution; so that we lost sight of them after that time.

Turning was resorted to 44 times, or once in every 150 cases; the forceps were used 45 times, or once in every 146 cases; the child's head was perforated 4 times, or once in every 1652 cases. Premature labour was induced twice.

In the post-mortem examinations of some women who died of spontaneous rupture of the uterus, we are at once struck with the immense increase of the size of the fundus uteri, whilst there does not appear to be a similar state of the cervix. It is highly probable that, in such cases, the rupture is brought on by the immense force exercised by this hypertrophied part of the fundus, and which the cervix is unable to resist. The direction of the laceration laterally speaks for this view. The occurrence of rupture is much favoured if the texture of the mucous membrane has been softened by a previous inflammation. In one case of rupture, the peritoneum alone was affected. The laceration was situated over the fundus of the uterus, and was several inches in length.

One case occurred, in which a woman complained so much of violent burning pains in the abdomen, and was so affected with dyspnœa, that the use of the forceps was deemed necessary. The operation was easily performed. The peculiar characters of the dyspnœa, the complete arrestment of the action of the diaphragm, and the similarity of this symptom to what had been witnessed in a previous case of rupture of the uterus, led us at once to diagnose this as also a case of rupture. For some days after delivery these symptoms almost disappeared, and the pulse improved in character, only a minor degree of asthma persisted. On the seventh day after delivery she commenced to sink, and died suddenly. The uterus was found ruptured at the cervix, but the peritoneum was entire.

There were thirteen cases of eclampsia; four of the mothers died, eleven children were born living. Only one breech presentation occurred; it was in a twin case. All the other children presented with the head. In six cases we resorted to the forceps; the other cases were terminated without artificial interference. We cannot agree with the opinion of Ramsbotham, that the disease occurs most frequently in hot seasons, as there was only one case in May, two in April, four in February, one in December, two in November, two in October, and one towards the end of September. Most of the children (ten) were males; the mothers were, generally speaking, young females, pregnant for the first time. In the post-mortem examinations no morbid symptoms were discovered in the brain. Our treatment consisted in blood-letting, cold water dressings on the head, and the use of tartar emetic. It is worth notice, that very generally after the coma that follows the convulsions, and lasts occasionally for many days, there is to be observed a state of forgetfulness, being sometimes the only remainder of this dreadful disease. I repeatedly saw women who had not the least desire of denying their being pregnant, who could not recollect at all the particulars of their confinement—nay, even of their having been pregnant. This circumstance is most interesting also in a medico-legal point of view.

In the following case chloroform proved very useful. A recently confined woman, who, as she told us, had never suffered from spasms of any kind, complained, a few days after a natural labour, of fever and pains in the abdomen. These symptoms, however, subsided in a short time. A few days afterwards she suddenly began to howl like a dog, to contract her face in a most ridiculous manner, and to be exceedingly restless. As often as we put some chloroform under her nose, she regained her consciousness, and, when asked, complained of no pains at all; but the aforesaid symptoms reappeared immediately when the

chloroform was removed. These symptoms lasted for three hours, when we put her, by means of chloroform, into profound sleep; at once all the symptoms yielded, but re-appeared in a few minutes after she awoke. I applied chloroform for a second time, and allowed her to remain under its influence for about one hour and a half. After that time she remained entirely free from the symptoms of threatened mania.

From the few cases where chloroform was used in our midwifery practice, and these were all cases of long-lasting and exhausting operations, it would appear that this medicine has a particularly soothing influence on the vascular system; for in all these cases the heart's pulsations were, after delivery, scarcely quicker than in a healthy person; and I may here observe, that in the Vienna hospital for the last two years, nearly all surgical cases have been anæsthetised when operated upon.

In cases of hemorrhage after delivery, if there are no clots or portions of the after-birth in the womb, contractions are, if possible, excited first of all by frictions. In the very rare cases where the frictions have no effect, we resort to cold water dressings externally; and if these prove inefficient, we inject cold water into the uterus. This treatment has been so successful, that out of more than 20,000 cases that occurred in the clinique since 1833, there are only two cases on record of women dying of post-partum hemorrhage; that is to say, one of a woman where assistance was only resorted to when she was already moribund, and the second a female taken with small-pox, where every means proved inefficient.

In the period extending from 15th October 1847 to the end of October 1848, only one out of 128 mothers died. The mortality increased in November, and still more in December, when one mother died out of every fifteen delivered.

These unfavourable results were undoubtedly caused by the political events of that period. It is not necessary to state how depressed in mind the most of the women confided to our care must have been at a time when their friends, fathers, and brethren, fought either in the ranks of the rioters or in those of the soldiery. But, moreover, one of the wells that supplied some of the yards of the large hospital with water was destroyed by the troops. The consequence was, that that cleanliness could less effectually be looked for, which we may boast is maintained at other times in our hospitals. The supplies of straw, coverlets, and so on, were also intercepted.

Towards the end of October, when the stores were almost exhausted, and no fresh provisions arrived, many women were confined, and the consequent disease became more fatal in December.

The first cases of puerperal fever that came under our care, showed in their beginning no other symptoms than sores near the fourchette that extended widely into the vagina, and over the perineum. They appeared about the sixth day after confinement, were uneven, of a greyish colour, and covered with a corroding matter; they bled very seldom. Sometimes they healed by granulation, but this went on very slowly, the patients meantime becoming extremely emaciated. Other cases proved fatal, the post-mortem examinations showing inflammation of the mucous membrane of the uterus and of the peritoneum; and, more generally still, of phlebitis.

In the subsequent course of this epidemic of puerperal fever, peritonitis, at other times peritonitis combined with pleuritis, were the more general symptoms. In the course of three days I had the sad opportunity of seeing two cases of phlebitis, ending in the destruction of the eyeball by metastatic purulent deposits. In the first case, only two hours, in the second, eight hours, elapsed between the first symptoms of disease appearing in the eye, and the lamentable catastrophe. It was highly interesting to observe how entirely puerperal fever ceased in our wards after cholera made its appearance. During the month of May and the first days of June, eleven mothers died of puerperal fever; on the 10th of June we saw the first patient under cholera, and had occasionally cases of it till the

11th of August. During this whole period we had only very few patients complaining of the slighter puerperal symptoms, in no case ending fatally. Almost immediately after the disappearance of cholera, the cases of puerperal fever became somewhat more frequent, and decidedly more dangerous.

Prof. Kiwisch, late of Wurzburg, now of Prague, has recommended a new method of inducing premature labour, which, being decidedly the least troublesome and the least dangerous to the mother, has now been repeatedly resorted to, both by Prof. Kiwisch and the leading men of the Vienna midwifery department. This means consists in the use of a douche of tepid water falling from a height of about ten feet, the stream being allowed to impinge upon the os uteri, and thus to open it in a short time. This plan is peculiarly adapted for cases of contraction of the pelvis, where you have sufficient time to use any advisable method to bring on premature labour. It is much less serviceable in cases of urgency, as it is quite impossible to state beforehand how many applications of the douche will be necessary for that purpose. The average hitherto has been about ten applications; we used to apply them twice a-day for a quarter of an hour each day.

Dr Moir made a statement as to the mode of *inducing premature labour*, which had been first adopted, he believed, by the late Dr Hamilton, and which Dr Moir and his father had almost invariably practised, with very considerable success. Dr H., conceiving that many children were in these cases lost by the pressure on the head consequent on the premature rupture of the membranes, then the usual method of inducing premature labour, tried the plan of gently opening and dilating the os uteri with the finger, and if the pelvis was very deep, cautiously using some rounded blunt instrument, such as a large-sized male bougie, with the view of separating the membranes from the surface of the uterus for two or three inches up from the os uteri all round; this being repeated every second or third day, according to circumstances. The consequence of this procedure was, that in eight or ten days at the most the os uteri became considerably opened and relaxed, the membranes protruding somewhat; so that when labour came on, everything proceeded very much as in a favourable natural labour. If, however, labour did not come on in eight or ten days, he took a male catheter, from which the point had been cut off, and introduced it for some four or five inches along the posterior surface of the uterus, between it and the membranes; then turned the point inwards towards the membranes, and then pushed forward to the extent of the eighth or tenth of an inch the point of a silver wire which had been previously lying within the catheter to its very point. With this he penetrated the membranes, and allowed two or three table-spoonfuls to escape along the catheter, and then withdrew it. The pressure of the uterus on the puncture prevented further escape of the water; and in every case but one, where Dr M. had to repeat this puncture, labour came on within thirty-six hours—the membranes at the os uteri remaining entire, and assisting to dilate the os uteri. Dr H. had thus operated between forty and fifty times for deficiency of space in pelvis, and all the children except four had been born alive.

Dr M. and his father had performed it above twenty times, and though not with so much success to the infant as Dr H., yet all the parents had recovered perfectly, and in almost every case when the child was lost, it arose from its being still too large for the aperture through which it had to pass; and even then, these cases had this advantage, that the mother suffered less than if she had been allowed to go on to the full time, and recovered rapidly.

Dr Simpson observed, in relation to the induction of premature labour, that for many years past he had followed another plan, viz., dilating the os uteri, and perhaps, he might add, separating the membranes also, by the introduction of sponge tents. He thought it a more certain, more efficient, and more safe mode, than any of the others that were used. He had now employed it in numerous cases, without ever finding it once to fail; always with perfect success in regard to the mother, and in a large proportion of cases with safety as regarded the child. It had the advantage, like Dr Hamilton's plan, of not rupturing the

membranes; and thus allowed the child to be protected by the presence of the liquor amnii during the process of labour. It also had other advantages. It was, Dr S. believed, more easy of employment than almost any other method. When adopted, and when the os uteri was dilated by a succession of sponge tents, the first stage of labour (that is, the dilatation of the os uteri) was really in many cases to a considerable extent accomplished before labour itself began; and consequently the child was saved from the pressure, &c., incidental to the earlier and sometimes protracted part of the first stage. It was, as a general rule, speedier in its operation than any other plan proposed. Dr Arneth had seen with Dr Simpson a case in which Dr S. employed it. In this instance labour pains came on within two hours after the first tent was introduced. In the same patient, in her last confinement, when premature labour was also brought on, the whole process of delivery was over in twelve hours after the induction of premature labour was commenced by the introduction of the first sponge. Latterly, in using sponge tents for this purpose, Dr Simpson had always begun by introducing a pretty large tent, one at least as thick as the little finger; and (sometimes without withdrawing the first), he has introduced a larger and larger size every six or eight hours. Usually, he requests tepid water to be thrown up into the vagina every hour or so after the first tent has been introduced. This of course is with the view of supplying a sufficient quantity of fluid to expand the compressed sponge. Dr Simpson had tried in one case lately, the plan mentioned by Dr Arneth, of injecting tepid water. It was in an instance where the patient had repeatedly found the child to die a short time after quickening, and retained it for six or eight weeks subsequently. During her last pregnancy, the same occurrence took place with the same symptoms. A few weeks having elapsed she threw up tepid water, at Dr Simpson's request, twice a-day, with a view of bringing off the dead fœtus. After nine douches, applied night and morning with a common syringe, expulsive pains came on, and a dead and shrivelled fœtus and placenta were expelled. As to the mode in which the injection of tepid water acted in inducing premature labour, Dr Simpson had some doubts whether Kiwisch's explanation of it, that it led to relaxation of the soft parts by the imbibition of the water, was a true explanation. He had a belief that when a douche of tepid water was strongly thrown into the vagina, it did not escape always readily out of the vaginal orifice, but accumulated in that canal, and by its accumulation and distension opened up the cervix uteri, and ultimately even separated the membranes. If it acted by mere imbibition, then Dr Simpson imagined that it might be of use in midwifery in a way not thought of by Kiwisch. If we had any means of relaxing the soft structures of the mother for two or three days before delivery, we would be enabled to curtail the duration of labour, and consequently to diminish its mortality. We might in this way, for example, make first labours as speedy and hence as safe as future ones, which at present they by no means are. In Great Britain, if all the children born for one year were girls and no boys, Dr Simpson had elsewhere attempted to show, that six or seven thousand infantile and maternal lives would be preserved. And if we could make the birth of boys as expeditious as that of girls, a great number of lives would be saved annually. The question recurs, would douching the vagina for two or three days before delivery, produce such a relaxation of the maternal passages as to facilitate labour to the requisite extent?

After a variety of other remarks, *Dr Malcolm* proposed a vote of thanks to Dr Arneth for his very excellent and valuable communication, which was seconded by *Dr Charles Bell*, and heartily accorded by the Society.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

A MEETING of several gentlemen, interested in the progress of physiological science, was held on the 4th of June 1851, when it was resolved that a society should be instituted, having for its object the investigation of the structure and functions of all organised beings in their healthy and diseased conditions. The following gentlemen were appointed office-bearers:—*President*, Professor Bennett; *Vice-Presidents*, Mr Zaglas and Mr John Scott Sanderson; *Secretaries*, Dr W. T. Gairdner and Mr John Barlow.

MEETING I.—*June* 7, 1851.—Professor BENNETT, President, in the Chair.

INTRODUCTORY ADDRESS BY THE PRESIDENT.

In the infancy of natural science, its cultivators were few in number, and each could without difficulty comprehend all that was known regarding it. As facts multiplied, the principle of division of labour came into play; with this disadvantage, however, that circumscribed research led to circumscribed generalisation, and we had as many sciences as there were divisions in natural history. From this state of things we are now emerging; for within the last twelve years it has been demonstrated that the laws regulating the various parts of the organic world, if not identical, are closely allied, and dependent on each other. It has resulted, that out of the numerous divisions into which natural science has been split, and out of the multitudinous details which have been discovered, a system of unity and simplicity is now evolved, capable once again of being embraced by a single mind. For, instead of there being a science of Botany, a science of Zoology, a science of Physiology, and a science of Pathology, there is in truth but one science, the laws of which embrace the facts of all these. Thus the growth of a plant and of an animal is brought about by like histological changes; physiological and pathological products are fundamentally the same; the chemical composition and structural elements of the two realms of organic nature are closely allied, and the dependence of one on the other has been exactly determined; the mode of reproduction, and the homologies observable throughout the vegetable and animal kingdoms, exhibit in their types remarkable points of unity; and in the functions performed by all, we begin clearly to perceive the same physical, the same chemical, and the same vital phenomena. In short, the present aspect of physiology points out to us that all the natural sciences must be intimately connected in order to form a consistent whole,—not like a chain, the component parts of which are united by a superior and an inferior link in the series, but rather like a piece of woven cloth, every thread of which is brought into necessary connection, and is intertwined with its fellows.

Such, gentlemen, is the idea which has led to the formation of this Society; and there is every reason to suppose that, if we can bring together the anatomist, the botanist, the physiologist, the pathologist, and the chemist, science will receive a more powerful impulse from their united than from their individual exertions. The time and place are favourable for making the attempt; for at no period in the history of this school has a larger number of original investigators existed among its youthful members. The field for observation in this city is most abundant; and it has long been felt that numerous important facts have been lost, and several laborious researches rendered unproductive, from the want of a society of easy access, and of aim sufficiently comprehensive to take cognizance of them. Here, then, we propose to assemble such of the students as distinguish themselves in investigation, the teachers of the Medical School, and the professors of the Faculty, to occupy ourselves with the confirmation of demonstrative facts, and a rigid criticism of the theories derived from them.

The constitution of our Society is of the most simple description. It is unattended with expense. All who are actual labourers in the field of science may readily qualify themselves for admission by forwarding to us an original observation, which henceforth shall be the only recommendation demanded. In thi way we hope to be enabled to carry out our object, united by mutual esteem and respect. That object is the investigation, by every possible means, of the structure and functions of organised beings,—in one word, the advancement of physiology, in its widest sense. The results of our meetings will be published, under the conviction, that so long as the Society adheres to the letter and spirit of its constitution, its labours must contribute to the sum of human knowledge.

I now invite all who can, and are willing to co-operate with us, to join our young institution, satisfied, from the position and known talents of the gentlemen I have the honour to address, that as its object is worthy of their exertions, so it will be prosecuted with uninterrupted diligence and untiring zeal.

AN UNUSUAL FORM OF TUMOUR.

1. *Dr Bennett* exhibited the half of a fibrous tumour, which, when entire, was somewhat larger than a billiard-ball. It was found loose in the cavity of the peritoneum by the late Dr John Reid, who has described it in a paper inserted in the "Edinburgh Medical and Surgical Journal," No. 128. He observes, "With the exception of a few depressions, it was perfectly round and smooth. On making a section of the tumour, it was found to consist of a number of concentric laminæ, with a calcareous deposit in the centre. It was very elastic; and the fresh section was exactly similar to the fibro-cartilage placed between the bodies of the vertebræ,—with this difference, that the centre of the intervertebral substance is soft and pulpy, while the centre of the tumour was occupied by the calcareous deposit."

On making a thin section of the so-called calcareous deposit, it was ascertained by Dr Bennett to present a remarkable organic structure. This was exhibited to the Society under the microscope, and presented a cellular structure, closely analogous to what was observable in the pith of phanerogamous plants. The cells varied in size, from the one-thousandth to the one-five-hundredth of an inch in diameter, round in the centre, more compressed at the edges, and presenting a broad rim which was loaded with mineral matter. Here and there, dense partitions, resembling in structure compressed woody fibre, passed through the mass of cells. It was clear, from this description, that the hard nucleus of the tumour was not composed of a plum or apricot stone, to which at first it bore a considerable resemblance. Could it be the branch of a tree, which had been swallowed, ulcerated through the intestines, and formed the nucleus of the tumour, which, at first united, had afterwards become detached from the peritoneum? Mr Quekett, to whom Dr Bennett had shown the preparation, considered the texture to be cartilaginous; but he was by no means satisfied of the correctness of this opinion.

2. *Dr Van der Byl* shewed specimens of intestine, prepared after the manner of Professor Brücke of Vienna, so as to show the muscular structures within the mucous coat. The preparations were made by boiling in acetic acid, and making thin sections of the dried intestine. (See "Monthly Journal of Medical Science," for April.)

3. *Dr W. T. Gairdner* exhibited a specimen of a deposit in urine, corresponding in characters with those described by Dr Golding Bird as urate of soda, and by others as urate of ammonia. It consisted of spherical bodies up to about $\frac{1}{50}$th of a line in diameter, coated with projecting spicula, or rather curved blunt processes resembling stalactites, of considerable size and firmness, and of rather deep amber colour. It was probably a combination of ammoniacal and earthy urates, as it proved, on analysis, to have no fixed alkaline bases, but to contain lime and magnesia in appreciable quantity.

4. *Dr W. T. Gairdner* exhibited a very convenient pocket microscope, formed of a simple Coddington lens, and sufficient in power for the examination of urinary deposits, and even blood or pus.

5. *Mr Barlow* showed a hollow spherical body, consisting of a shell of calcareous matter, imbedded in a fibrous tissue, and about the size of an ostrich egg, which was expelled from the uterus of a mare, along with the after-birth. It contained another smaller sphere, of calcareous matter with projecting spicula, about the size of a hazel-nut. Some discussion took place as to whether it could be considered a blighted ovum; but it was generally agreed that no distinct opinion could be formed of its nature.

Mr Drummond was appointed to report on the structure and chemical composition of the calcareous or bony matter of the mass.

6. *Mr Cobbold* showed a drawing representing ossification within the Fallopian tubes, occurring in a married woman who had never borne children, and in whom were found (post-mortem) extensive adhesions throughout the entire abdominal cavity. From the rarity of the position, it afforded an interesting example of the conversion of lymph into osseous matter. The individual died of an affection of the lungs.

Dr W. T. Gairdner thought the appearances in this case corresponded with those to be expected in old tubercular disease of the uterine organs and peritoneum. It was unfortunate that no note had been preserved of the kind of disease of the lung.

7. *Mr Zaglas* made a long verbal communication on the mechanical relations of the spine and sacrum to the pelvis, which will be noticed in a future report.

Dr W. T. Gairdner and *Mr Barlow* were appointed to report on Mr Zaglas's views. (An account of these will be published in the report of a future meeting.)

MEETING II.—*June* 14, 1851.—Professor BENNETT, President, in the Chair.

REPORTS.

ANALYSIS OF A CALCAREOUS STRUCTURE FROM A MARE. BY JAMES DRUMMOND.

1. *Mr Drummond* communicated the following report on the structure described by Mr Barlow at last meeting:—

When analysed qualitatively, the tumour showed by Mr Barlow was found to contain carbonate of lime, phosphate of lime, a small quantity of phosphate of magnesia, and some soluble saline matter, chiefly chloride of sodium. I also tested it for fluorine, but could not detect any; which, however, may have been owing to the small quantity of the substance on which I was obliged to operate. I found in it a small quantity of sulphate of lime; but this, in all probability, did not exist as such in the structure originally, but was produced, during incineration, by the oxidation of the sulphur contained in the organic matter.

A quantitative analysis showed these substances to exist in the following proportions in 100 grains of the substance, dried at 212° till it ceased to lose weight:—

Organic matter, ...	42·08
Carbonate of lime, ...	11·73
Phosphates of lime and magnesia, . .	45·30
Soluble saline matter, ...	·89
	100·00

Comparing this with the analysis of ordinary bone, we find that it differs from

it only in containing a somewhat larger amount of organic matter and carbonate of lime, and a smaller amount of phosphates.

With respect to minute structure, a thin section presents almost the same appearance as ordinary bone, exhibiting haversian canals, lacunæ, and canaliculi, just as ordinary bone does; the only difference being, that the haversian canals are somewhat larger, and their lacunæ less regularly arranged, than in normal bone. It is somewhat curious that, if the lacunæ in bone arise from the nuclei of cartilage cells, we should here find regular lacunæ existing where it is probable there were no cartilage cells,—the organic tissue, when freed of its calcareous matter, presenting the appearance merely of a fibrous membrane.

ORIGINAL COMMUNICATIONS.

1. *Dr Bennett* showed specimens of a fungus or parasitic conferva, which he had found upon the dead larvæ of the common garden butterfly, which presented an umbelliferous, instead of the usual dichotomous, division of its thalli, and terminating in numerous mycelia, containing sporules.

2. *Mr Barlow* exhibited a specimen of tubercular disease affecting the epididymis of a boar. The testicle was enlarged, but not altered in structure.

LEUCOCYTHEMIA.

3. *Dr W. T. Gairdner* showed the spleen, lymphatic glands, and blood, of a man, aged about sixty, who had died in the Infirmary, in a highly adynamic state, with traces of purpura and obscure fever. He had no distinct local affection, except ulceration of the gums, probably caused by mercury before his admission, and the remains of what appeared to have been two chancres on the corona glandis, with a muco-purulent discharge from the urethra. The spleen was much enlarged, and in parts softened; the lymphatic and mesenteric glands also enlarged; and the blood presented great excess in the numbers of its white corpuscles. The case was, therefore, in every respect analogous in its pathology to those described by Dr Bennett, and others, under the names of "Leucocythemia," "White Blood," &c. Demonstrations of the diseased tissues, and of the blood, were shown.

Mr Sanderson was appointed to examine the structures in this case, and report.

4. *Dr W. T. Gairdner* showed the uterus, and a portion of intestinal canal, in a case of which he had not heard any details. The former was affected with fibrous tumours in various stages; the latter showed the glands in the neighbourhood affected with a deposit, which very much resembled cancer. Dr G. hoped to obtain possession of the details at a future period; but the case seemed interesting in reference to the alleged incompatibility of the fibrous tumour of the uterus with the cancerous diathesis.

VARIETIES.

Treatment of Stricture by External Incision.—We are happy to see that Mr Syme's operation has been adopted by Mr Guthrie, who, in a lecture lately published, thus relates a case in the patient's words:—"In April 1851, I applied to Mr Guthrie, having come from India for this purpose. He passed a small 5 with difficulty, and stated that I had three strictures, one half-an-inch from the orifice, which felt like a ring; the second, immediately in front of the scrotum, which felt hard, like a cord, for the space of an inch; and a third, at the curve, which he called a natural obstruction, but against which, for several years, my bougie always stopped. Mr Guthrie divided my second stricture from behind forward, so as to admit a No. 8 with ease, and which removed at once all my suf-

ferings. The first stricture was divided a few days afterwards, and a No. 11 went into the bladder without difficulty, leaving me in a perfect state of bliss. I can now pass a No. 13, and think myself cured."

STATE OF MEDICINE IN EGYPT.—On a former occasion, some general remarks were transmitted in regard to the Egyptian school of medicine, the glory of which was exhibited during the influential days of Clot Bey, the Mussulmanised French surgeon, whose name and reputation as an expert operator, and on account of his daring experiments in the plague, are matters of professional history. With the death of his patron, that wise and shamefully slandered old Mehemet Ali, the sun of his greatness immediately set. However, besides a reputation, he got what every adventurer in the service of the viceroy hopes for, a splendid fortune—and with it made tracks for Belle France, where he is resting upon his oars, and quaffing such bottles of champaigne as he never drank in the palace of Schonbra, although his master indulged him with the possession of everything else. He is accused now of squandering the Pasha's money, in unnecessarily ordering the fabrication of all sorts of surgical instruments, without reference to cost, and that were of no utility beyond constituting a museum. Say, however, what they may of him—it being fine sport to kick a dead lion—Clot Bey was a great man in his day. He made the medical school at Cairo, and during his administration it had some character. Latterly it has had none. The French influence has died out—the Italian ought to be kicked out. A more beggarly, sycophantic set of unprincipled toadies never existed. At El Arish, the ancient Rhinocolura—where political offenders, after having their noses cut off, were sent for exile—the spot, within the half-mud fortress, still remaining, in which King Baldwin, of Crusade memory, died—the last town of Egypt, on the borders of Palestine—is a quarantine station on the top of a sand swell, which is confided to the charge of one of these cunning renegadoes. He honestly confessed to me that he was not a physician, and further, that he knew nothing about the practice of medicine. He further said he should like my advice in a difficult case within the tabooed enclosure! He also wished to know whether I had any medicines with me, as he had none. He was health officer, and had the full control of the establishment. Afterwards, a dirty, bare-legged fellow, in the wake of a caravan in which I was travelling, accidentally finding out that I was a hakeem, asked assistance on account of a bronchial inflammation. He had consulted the grand hakeem at El Arish, who furnished a powder that was to act like magic; but instead of affording relief, augmented the evil about the soft palate. I found it was wood ashes, which he was directed to have blown down the throat, in large pinches, several times a-day; and a lad was actually seen blowing it down with a hollow stick.—*Letter from Dr J. V. C. Smith*, in *Boston Medical and Surgical Journal.*

USE OF TOBACCO AND INDIAN HEMP IN EGYPT.—I cannot deny myself the pleasure of adverting to Dr Mussey's widely-promulgated anathemas against the use of tobacco, the habitual use of which is as much objected to by myself as by that staunch apostle of temperance in eating, drinking, and smoking. But with all his zeal, his philanthropy, his bold arguments and cogent reasoning on this subject, in Egypt he would find stumbling-blocks in the way of his conclusions that would be worse to manage than a hogshead of the best Kentucky in the market. Men, from childhood, smoke incessantly in Egypt. They smoke everywhere and under all circumstances. There is no cessation—not an hour when a cloud of curling smoke is not ascending. It is the first and prominent civility to hand a pipe, and smoke you must, or suffer under the imputation of being no gentleman; and were that good man of Cincinnati sitting where this sentence is written, he would find himself smoke, like every one else here. People live long enough, in all conscience, notwithstanding this everlasting smoking; for they outlive their usefulness—outlive everything but animal wants—live, some of them, till everybody wishes they were dead! I have not been an inattentive observer of the smoking mania in Germany, and other parts of continental Europe; on the contrary, a strict inquiry into the moral and constitutional

effects of the habit, very judiciously called a vice, was instituted as I travelled from kingdom to kingdom where it prevailed; and I have arrived at the gratifying conclusion, that if persons wish to smoke they may, and I shall not waste my breath in warring against the habit. Another kind of smoking is practised in Egypt, and probably in Syria, unknown to us in America—viz., that of Indian hemp. Cigars are charged with it, and there are apartments where individuals may go, and draw in, through a long pipe-stem, a kind of smoke that exalts a dirty barefooted rascal into an imaginary prince. In a few minutes he sees the gates of a Mohammedan paradise, gazes wildly towards the sky, and laughs till all consciousness passes away, and he falls into a lethargy of considerable duration. I suspect it is hemp, and not opium, as generally supposed, with which cigars are drugged, and made the instruments, in the hands of designing men, in London and other great cities on the Continent, for the perpetration of many dreadful crimes.—*Ibid.*

Poisoning by Nicotine.—An extraordinary trial, which took place in Belgium, and in which the count and countess Bocarmé were accused of poisoning the countess' brother, by the forcible administration of nicotine, has been recently brought to a conclusion. The count, who was found guilty, has been condemned to death; the lady may consider herself fortunate in having escaped. The most interesting circumstances connected with this horrible affair are, that the count seems to have prepared the poison with his own hands; and that although he probably selected nicotine as a substance which could not be chemically detected in the body of his victim, M. Stas, a Belgian chemist, obtained distinct evidence of its presence on applying suitable tests. M. Orfila has since communicated the results of some experiments on the detection of nicotine, to the French Académie de Médecine, and although the process which he recommends differs slightly from that which was adopted by M. Stas, he arrives at the same conclusion, viz., that the poison can be detected unequivocally if present even in the quantity of a few drops in the stomach and bowels, and that it may even be found in the liver and other organs after its absorption into the system.

PUBLICATIONS RECEIVED.

A Translation of the New London Pharmacopœia, including also the New Dublin and Edinburgh Pharmacopœias, with a Full Account of the Chemical and Medicinal Properties of their Contents. By J. Birkbeck Nevins, M.D., &c. London: Longmans. 1851.

The Human Body, and its Connection with Man. By James John Garth Wilkinson, M.R.C.S., Eng. London: Chapman & Hall. 1851.

On Diseases of the Mucous Membrane of the Throat. By William R. Wagstaff, M.A., M.D., &c. London: Churchill. 1851.

Familiar Letters on Chemistry. By Justus von Liebig. Third Edition, revised and much enlarged. London: Taylor, Walton, and Maberly. 1851.

Report of the Pennsylvania Hospital for the Insane, for the Year 1851. By T. S. Kirkbride, M.D.

On Articulate Sounds, and on the Causes and Cure of Impediments of Speech. By John Bishop, F.R.S., &c. London: Highley. 1851.

Observations on the Diseases of the Rectum. By T. B. Curling, F.R.S., &c. London: Highley. 1851.

Clinical Observations on Diseases of the Genito-Urinary Organs. By Henry James Johnson. London: Highley. 1851.

The Teeth, and their Preservation in Infancy and Manhood, to Old Age. By Alfred Canton, M.R.C.S.L., &c. London: Bailliere. 1851.

Practical Remarks on some Exhausting Diseases. By Sir James Eyre, M.D. Second Edition. London: Churchill. 1851.

An Inquiry on the Subject of Vaccination. By Benjamin Ridge, M.D., &c. London: Churchill. 1851.

Hydropathic Statistics. By Walter Johnson, M.D., London. London: Simpkin and Marshall. 1851.

Nice et son Climat. Par Edwin Lee, M.R.C.S.L. Paris et Londres: Bailliere. 1851.

Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. London: Churchill. 1851. Fasciculus 9.

Pathology of the Human Eye. By John Dalrymple, F.R.S., F.R.C.S. London: Churchill. 1851. Fasciculus 7.

New York Register of Medicine and Pharmacy. Vol. II. Nos. 4 and 5.

EXCHANGE LIST.

The following journals reach us regularly :—

British and Foreign Medico-Chirurgical Review.
Dublin Quarterly Journal of Medical Science.
American Journal of Medical Science.
New York Journal of Medicine.
Psychological Journal.
Annales Médico-Psychologiques.
Präger Vierteljahrschrift.
Henké's Zeitschrift für die Staatsarzneikunde.
Journal für Kinderkrankheiten von Behrend und Hildebrand.
Zeitschrift für Rationelle Medicin, von Henle und Pfeufer.
Bulletin Général de Thérapeutique.
Revue Médico-Chirurgicale de Paris.
Journal de Médecine et de Chirurgie Pratique.
Boston Medical and Surgical Journal.
Philadelphia Medical Examiner.
Pharmaceutical Journal.
Veterinarian.
London Journal of Medicine.
Provincial Medical and Surgical Journal.
Chemical Gazette.
Dublin Medical Press.
Medical Times.
Medical Gazette.
Caspar's Wochenschrift.
Gazette Médicale de Paris.
Gazette des Hôpitaux.
L'Union Médicale.
Schmidt's Jahrbücher.

The Zeitschrift der K. K. Gesellschaft der Aertzte zu Wien now reaches us regularly. The Nos. for *November* and *December* 1850 are still missing, and we shall feel obliged to the editor if he will direct them to be transmitted by the next parcel.

The following reach us very irregularly, or at long intervals :—

Bibliothek for Læger.
Hygiea.

The British American Journal of Medical Science is *occasionally* charged with a postage equal to triple or quadruple the commercial value of the Journal ; and hence, although regularly dispatched, cannot always be received.

The Upper Canada Journal of Medical, Surgical, and Physical Science for April 1851 has been received, and shall be added to our Exchange List when arrangements are made for sending it to us free of charge.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Leucocythemia, or White Cell Blood.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Theory of Physic and of Clinical Medicine in the University of Edinburgh. (*Continued from Vol. XII., p.* 326).

IN the January and April numbers of the Journal I gave an account of all the cases of leucocythemia which had been published, or communicated to me. I also referred to such cases recorded in Medicine, as might reasonably be supposed to have presented this pathological state, although the blood had not been examined with a microscope. Lastly, I alluded to those cases in which considerable hypertrophy of the spleen and liver had occurred, and where the blood had been carefully investigated, and proved to contain no excess of colourless corpuscles. Since then, Case XVII has died, and I am enabled, by the kindness of Dr Heslop of Birmingham, to give a more complete history of the individual, as well as an account of the post-mortem examination. Professor Vogel has also published a remarkably well observed case, in the last number of Virchow's Archives, and other instances of the disease have fallen under my notice, and been communicated to me. Before proceeding to the second part of this memoir, therefore, it will be necessary to state the parts embraced in these various observations.

COMPLETION OF CASE XVII.—(Taken from the Note-book of Dr James Johnstone, at the General Hospital, Birmingham, by T. P. Heslop, M.D., Resident Medical Officer.)

John Duffy, ætat 42, spare, red hair; about 5 feet 7 inches in height; 10 stone and a-half in weight; married; a farmer's labourer; born in Ireland. Admitted into the hospital, under the care of Dr Johnstone, on the 24th of March 1851. Eight months ago, July 1st, 1850, this man first applied for relief at the hospital. He then complained of uneasiness and swelling in his left side; and his general appearance at that period did not materially differ from his present condition. The account he gave was,—that two years previously, during the summer, he first noticed a swelling in the left side, and that his urine became very thick. Eight years before, he had been engaged regularly at harvest work in Lincolnshire, where ague was common. The following year, in March, he was attacked with ague, and regularly shivered every third day, the

paroxysm usually occurring in the evening. He was ill for a fortnight with the attack, which was the last time he suffered from the disease, though, for the five preceding years, in which he had gone to the same district, he uniformly had an attack of the disease a few months after his return. He observes, that since the last attack, the colour never fully returned to his cheeks, but he was able to do his work nearly up to the time of his admission, though latterly with great difficulty, from weakness and shortness of breath. The whole left hypochondrium was dull on percussion, and a considerable portion of the left half of the abdomen was equally so, being occupied by a firm resistant substance, which passed superiorly and posteriorly behind the left lower ribs into the gastro-splenic region, while it displaced the intestines to the right side, and inferiorly. His blood was examined microscopically, and found to contain a great number of pale cells, with granular contents, averaging about twice the size of the yellow corpuscles, though some of them were of larger measurement. —(See further description of these bodies in the April number of this Journal.) After remaining in the hospital for a few weeks, he was discharged, but after a short time was re-admitted, when his urine was noticed to be albuminous.

He had measles when a child, but never had scarlatina, variola, or typhus.

He has been almost constantly either an in or out-patient of the hospital since July of last year. On his appearing to-day (March 24), he was so evidently getting worse, that he was again admitted. His face is pale and worn ; skin dry and wrinkled ; ankles œdematous. Complains of a singing and noise in the head, unattended by severe pain. Sight unimpaired ; but he is so deaf that he is scarcely able to hear the loudest talking. He states that the deafness has only come on during the last fortnight.

The abdomen is more prominent than ever; the hypogastrium giving the appearance of the bladder when in the last degree of distension ; the superficial epigastric veins slightly more evident than natural. No distension of mammary veins. A tumour, with well-marked outline, is felt occupying the whole abdominal regions, with the exception of the right hypochondrium, umbilical and iliac regions. When the patient stands, the tumour seems to rest upon the pubes. To the touch it is uniformly firm and resistant, its inner border, near the umbilicus, being distinctly and deeply fissured. No friction or fluctuation observable. It does not extend further back than the anterior superior spine of the ilium. The whole of the abdomen, exclusive of the regions already mentioned, is absolutely dull on percussion, the smartest stroke not eliminating even the gastric tympany, excepting immediately below the limit of the præcordial clearness. The superior limit of the dulness is about three inches below the angle of the scapula. Heart sounds normal. No murmurs in the neck. He is quite free from epistaxis or diarrhœa. The urine is turbid, depositing on standing a large quantity of lithate of ammonia ; acid ; spec. gravity, 1020 ; moderately albuminous. The microscope reveals nothing but a few epithelium scales and amorphous lithates. The blood has a slate-brown tint on its surface, as when about to show the buffy coat. Large, pale, granular cells in great profusion are still observed in it. Ordered a saline purgative three times a-day. On the 31st diarrhœa came on, for which he was ordered an opiate. On the 3d of April he complained greatly of the noise in his head, for which he was blistered. Continuing to get worse, he insisted upon leaving the hospital on the 10th of April, and died seven days afterwards.

Information of his death having arrived at the hospital on the 19th of April, I went, accompanied by my friends, Mr Elin, the house-surgeon, and Mr Sharman, to his residence, and requested an examination, which was readily granted. The attendant, an intelligent woman, informed us, that two or three days before death Duffy had complained of severe pain about the right hypochondrium, and had frequently mentioned, with surprise, the great diminution of his belly, having made her feel the left side to convince her of the fact. On examining the body externally, about forty hours after death, the weather

being rather warm, I was greatly struck with the *softness* of the whole abdomen; in fact, on the front surface I could not define, either by palpation or percussion, a distinct tumour, so that I could not avoid expressing a suspicion that the spleen had ruptured, discharging its contents into the peritoneal cavity.

Sectio Cadaveris, April 19.—The feet and ankles œdematous. On an incision being made through the parietes, the intestines alone came into view, with the exception of a small portion of the edge of the spleen, on the extreme left of the cavity. No fluid effusion or adhesions were noticed. On thrusting the hand into the posterior part of the left lumbar region, the spleen, enormously enlarged, was brought into view. It was easily dislodged from its situation, having contracted no adhesions to the surrounding organs. Its vertical measurement was 16½ inches; the transverse, above, 7¾ inches; below, 9½ inches. Its depth, 2¾ inches; and circumference, 37¼. It weighed 7 lb. and 13 ounces. On the spleen being taken out of the cavity, and the intestines being displaced, the liver was observed to be hypertrophied to nearly three times its natural size. Its structure was so perfectly broken down as to be nearly diffluent, so that it was found impossible to carry it away. In spite of its immense enlargement, it was entirely concealed by the ribs and upper portion of the intestines. The kidneys were enlarged, very soft, and of an uniform reddish colour; it being difficult to see the line of separation of the lobular and cortical portions. The right kidney (the only one weighed) was 6 oz. in weight. The heart dilated, its walls friable, and of a dark red hue. The ventricles moderately distended with imperfectly coagulated blood, of the colour of dark clay, and which, but for its situation, would have scarcely been imagined to be blood. The aortic valves were slightly atheromatous at their margin. The other organs were not examined.

Upon making a section subsequently of the spleen, no other change was noticed in its structure than that of being of a somewhat lighter tinge than natural. I observed, on a microscopic examination, a large number of cells, which differed in no respect from those seen in the blood during life.

Remarks.—The account now given of this case differs somewhat from that contained in the former note regarding it. Thus, instead of taking intermittent fever eighteen years before coming under observation, it seems that only seven years previously the last attack occurred; and that, for five years anterior to that, he had suffered from ague every autumn. In this manner the intermittent fever is made to bear more directly on the causes producing enlargement of the spleen.

Case XXIX.—*Leucocythemia detected during Life and after Death; Hypertrophy of the Spleen, Liver, and Mesenteric Glands.* [1]

Christian Kratz, æt. 34, a sailor, entered the hospital, 8th July, 1850. Two years ago there appeared a glandular swelling under the right angle of the jaw, which, on the application of lotions, disappeared in eight days. He then, for the first time, observed a slight enlargement of the abdomen, which gradually increased in size. Some time afterwards, the swelling under the jaw again appeared, suppurated, and was opened, giving exit to a considerable quantity of pus, after which it healed up. The enlargement of the abdomen progressed, accompanied by pain, increased from time to time. Cough, expectoration, and difficulty of breathing came on. The appetite diminished. After eating, pain in the abdomen and dyspnœa were excited; the stools were sometimes normal, at other times there was diarrhœa. For some months past,

[1] Condensed from Virchow's Archives. Band 3. Seite 570.

the abdominal swelling has increased with rapidity. He has never had intermittent fever or syphilis.

On admission, the patient is emaciated, but the countenance, though not healthy, cannot be said to be anemic. Pulse 84, full and hard. There is considerable dyspnœa; cough, with muco-purulent expectoration. On percussion, slight dulness under the right clavicle, and, on auscultation, tubular breathing and increased vocal resonance in this situation. Posteriorly and inferiorly over both lungs, fine and coarse moist râles are distinctly heard. The tongue is coated; appetite bad; considerable pain in the epigastrium, extending into the right hypochondrium; increased after meals, and on pressure. Since yesterday, strong pain in the right iliac region, also increased on pressure. The abdomen is protruded, and its superficial veins greatly developed. In the left side may be felt a large resisting swelling—evidently the spleen—which, coming from the left hypochondrium, extends inferiorly to the sacrum (darmbein), its anterior margin reaching forwards to the umbilicus. Pressure upon it produces pain. The liver is also greatly enlarged, extending from the seventh intercostal space above to the sacrum (darmbein) inferiorly, its left lobe coming in contact with the spleen. There is diarrhœa; and he has a few hæmorrhoidal swellings round the anus. The urine is normal in quantity, with a copious sediment of uric acid. The inguinal glands on the right side are slightly enlarged.

The blood presented its usual appearance when drawn from the arm, but in four hours presented a colourless layer superiorly, like cream. In twenty-four hours, it had separated into two portions, the upper half milk-white, the under dark red. The former contained corpuscles of the size, appearance, and reaction of pus corpuscles (from the minute description given, they were evidently the same as those previously described). The colourless cells appeared one-third or one-half as numerous as the coloured ones. A portion of the blood was allowed to coagulate, which it did in the usual manner. The upper part of the clot, however, presented a whitish layer, resembling white granulations. These consisted of aggregations of colourless cells. The serum was copious and clear, without a trace of milky opalescence.

The blood was analysed in Liebig's Laboratory by Dr Strecker, and yielded

Fibrine, . . .	4·46
Solids of Serum, . .	82·35
Globules, . . .	97·39
Total Solids, . . .	184·20
Water, . . .	815·80
	1000· 0

Up to the 19th of July, the symptoms underwent no change. The pulse then became more rapid (110), the thirst increased, the appetite remaining bad. Œdema of the feet and scrotum appeared, and a miliary eruption on the breast and abdomen. The dyspnœa increased, the urine had a strong acid reaction, although the uric acid sediment was diminished. The occasional diarrhœa continued. On the 25th of July, the pain in the splenic region increased, and from this period hectic fever was present, with profuse sweating and diarrhœa; and, notwithstanding the use of various remedies, judiciously administered, he died August 4th.

Sectio Cadaveris, twenty-six hours after death.—The body was greatly emaciated, and the increased size of the spleen was visible under the abdominal walls.

Head.—Fluid blood in the longitudinal sinus. The corpora striata, thalami, and corpora quadrigemina very soft. Brain otherwise healthy. No serum in the ventricles.

Chest.—The lungs were slightly infiltrated with serum at their posterior and inferior portions. No trace of tubercle. The pericardium contained

much bloody serum. The venæ coronariæ unusually distended with blood. The cavities of the heart, as well as the aorta and pulmonary artery, were filled with blood of the consistence of syrup, containing greyish-white soft coagula. The empty heart was soft, valves normal. At several points of the trabeculæ small masses of coagulated fibrine were attached. The thyroid gland was normal.

Abdomen.—The mesenteric glands consisted of white masses the size of a bean, containing, as determined by the microscope, numerous fat globules and granules. The mesenteric veins were distended to the size of the finger, with blood of the consistence of syrup, containing soft white flocculi. The liver was enlarged, and weighed five pounds thirteen ounces. On section, the parenchyma presented a homogeneous greyish red colour. Its cells were normal. The spleen occupied the entire left half of the abdominal cavity. It was twelve inches long, seven inches broad, and four and a-half inches thick; and weighed five pounds three ounces. The surface is smooth, and of its usual colour, only at the upper end covered with white fibrous exudations, and united to the peritoneum. On section, the parenchyma presented the usual appearance, and its structure, when microscopally examined, was also normal. The splenic vein was distended with the same blood as was found in other vessels. The kidneys were healthy. The suprarenal glands consisted of masses of cheesy consistence, owing to fatty degeneration.

The blood was carefully examined in various parts of the body, especially in the cavities of the heart, portal vein, the ascending vena cava, and iliac vein. The fluid blood from the heart, in addition to the colourless cells formerly noticed, contained nucleated fusiform cells, similar to those found by Virchow in the blood. The coagula were soft, partly white, and partly dark red; the former containing, under the microscope, amorphous granular masses, with several colourless blood corpuscles. The ashes of this blood were determined by Dr Strecker to contain, in 100 parts, 3·42 of oxide of iron.

Remarks.—This is a remarkably well observed case, the general symptoms, structural functions, and chemical composition of the blood being similar to what we have previously seen to exist. A new fact, however, was elicited by analysing the ashes of the blood after death, and determining that the amount of iron was decreased—a result to be anticipated from the comparative diminution in coloured corpuscles.

Case XXX.—*Leucocythemia detected after Death; Hypertrophy of Spleen, Liver, and Lymphatic Glands.*—(Communicated by Dr W. T. Gairdner.)

T. L., an Irish labourer, æt. about 60, was admitted into the Royal Infirmary on the 22d May 1851, complaining of exhaustion and suffering, arising from ulceration of the gums, which altogether prevented mastication, and greatly disturbed his articulation. The teeth were almost all decayed, and many of them extremely loose; the gums in the lower jaw almost destroyed by ulceration of the mucous membrane, which had a phagedenic character, and extended at some points to the inner surface of the lips; slight cough; no ulceration of throat; no trace of scorbutic affection in the limbs; a very few small petechiæ on the shoulders and neck, which were considered flea bites. He was of exceedingly dull intellect, and could not be brought to give an accurate history of himself; but his replies rendered it probable that he might have taken mercury before admission, at the same time, that no certain information could be gained either as to this fact, or the origin, nature, and duration of the disease, for which he had previously been treated.

During his illness, attention was chiefly directed to the local affection, and to the extreme state of exhaustion, which required from the first the adminis-

tration of stimulants. He had little appetite, and seemed to labour under an obscure remittent fever of typhoid type, but without marked nervous or other local symptoms. He complained occasionally of pain in the abdomen, but not distinctly localised,—the bowels were rather confined,—the stools natural,—the tongue natural,—the urine sufficiently abundant, but not particularly examined. Quinine, sulphuric acid, stimulants, and regulated diet had little effect ; he died, rather sooner than was anticipated, and without change in symptoms, on the 8th June.

Sectio Cadaveris, June 12th 1851.—Body somewhat emaciated,—presents some marks of commencing decomposition. The external lymphatic glands present considerable enlargement, especially on right side of body. The parotid and submaxillary glands of right side are distinctly enlarged, Those of the opposite side almost normal.

Chest.—The surfaces of the left pleura are slightly adherent at the apex of the lung, the diaphragm is free anteriorly, but at the lower and back part, the lung is firmly and extensively adherent, and cannot be removed without injury to the tissue of the organ. Similar adhesions exist in the opposite lung, but they are less extensive. Both costal pleuræ present a slightly mottled appearance, from the existence of scattered petechiæ, and both contain from two to three ounces of sero-sanguinolent fluid. On the left side, there exists, partly by the adhesions at the base of the lung, a transverse septum extending from the convex surface of the diaphragm to the opposite sixth and seventh ribs, and completely separating the pleura into two cavities, the lower capable of containing a pint of fluid, and in relation with the convex surface of the spleen. The pericardium contains about half-an-ounce of sero-sanguineous fluid. The heart is of normal size. Weight, 10½ ounces. The valves, ventricles, and lining membranes perfectly normal. The tissue of the heart is soft and pale. The right cavities of heart contain little blood. In the right side, there are several soft colourless coagula. The bronchi contain little fluid, and their mucous membrane is normal. The bronchial glands are in general of small size and dense. Several of them on section, present an appearance of dark pigmentary deposit. In the centre of one there is a mass of atheromatous matter. The right lung is universally crepitant, slightly impaired at posterior part of lower lobe, which is augmented in density. At the apex there is a small, but tolerably well-marked cicatrix. The left lung is normal, with the exception of slight impairment of crepitation at the lower and posterior part.

Abdomen.—Immediately under the peritoneum there are several extravasations of blood, especially in the neighbourhood of the spleen, where a patch existed five inches square. The alimentary canal (with the exception of the stomach), presents few abnormal appearances. The solitary and Peyer's glands are well marked but not injected, and several of them present a few petechial points. The mesenteric glands are generally enlarged, but none of them exceed greatly the size of a French bean. The liver is large, pale in colour, and free of abnormal appearance, with the exception of some petechiæ on the serous surface of lesser lobe. Weight, 80 ounces. The spleen, which, in consequence of firm adhesion to the diaphragm, was detained high in the abdomen, was much enlarged, weighing 34 ounces. On section, some portions were found of the normal colour and consistence, while others were deeper purple than natural ; and abruptly divided from these, there were many parts of soft friable consistence, and a lighter colour, mingled distinctly with an opaque yellowish tint. These portions, on microscopic examination, were found to abound very much in the corpuscles proper to the spleen, being as usual of very various size and appearance ; but mingled with them were many more or less resembling the white corpuscles of the blood. The Malpighian vesicles were nowhere visible, and the trabeculæ not more than usually distinct. The lymphatic glands of the posterior cervical, axillary, submaxillary, and inguinal regions, present more or less enlargement, nowhere very considerable. The largest are about the size of a hazel-nut. On section they are rather friable, abounding in whit-

ish juice, and present abnormal vascularity at some points, but no distinct deposit. On microscopic examination, the white juice abounds in corpuscles, many of them nucleated, often reaching $\frac{1}{100}$th or $\frac{1}{120}$th of a million in diameter, but of no characteristic type. The mesenteric glands present a similar condition, but none of them exceed the size of a wild cherry. The microscopic appearances are similar.

Blood.—The blood had, in all the vessels examined, and in the heart, a less deep-red colour than usual, and also a more muddy or greyish tint than natural, very similar to that observed in the mixture of pus with the blood. On microscopic examination, its white corpuscles were found in very large quantity, amounting in the fluid parts of the blood to nearly two-thirds the number of the red. Neither the red nor the white presented any altered characters. The nuclei of the latter were perhaps rather more apparent than usual; and their diameter was mostly double that of the red, which were on an average, rather smaller than is commonly observed.

Remarks.—This man presented febrile symptoms; but had no diarrhœa or hemorrhagic tendency, unless the sponginess of the gums, and the petechiæ in the mucous membrane and abdominal walls, be considered evidences of it. A careful examination of the structure of the spleen by Mr Sanderson proved it to consist of simple hypertrophy, principally dependent on an excess of its cell elements.

I have been favoured by Dr Wallace, of Greenock, with the particulars of two cases of enlarged spleen, which occurred in the Royal Infirmary of Glasgow, under the care of Dr Easton. In one the blood contained no excess of colourless corpuscles, and resembled Case XXVIII. The other was a case of leucocythemia, and is as follows:—

Case XXXI.—*Leucocythemia detected after death; Enlargement of the Spleen and Liver; Hemorrhagic Apoplexy.*—(Communicated by Dr James Wallace, of Greenock.)

David Hosie, mason, æt. 17, admitted 9th March 1850. Six months ago, in consequence, as he supposes, of having strained himself a week previously when lifting a heavy weight, the patient became affected with acute pain in the left hypochondrium, which was so severe as to prevent him from resting on the right side. At the same time, also, a hard swelling began to appear below the false ribs of the left side. It was painful on pressure, and increased rapidly in size, but has for the last five months remained stationary. The pain was relieved by cupping, and the patient continued afterwards without any particular uneasiness till six weeks ago, when he became affected with looseness of bowels, the stools consisting of blood and slime, and being accompanied with tormina and tenesmus. These symptoms continued for fourteen days, and reappeared after an interval of a week,—the dejecta, however, being free from blood.

On examination there is observed great fulness in the left side of the abdomen, caused by the presence of a tumour which extends from the hypochondrium to more than midway between the umbilicus and Poupart's ligament, its anterior margin being well defined, and about half an inch to the left of, and parallel with, the linea alba. It can be moved slightly upwards and downwards, and forwards and backwards, has an even surface, and is without pain even on pressure. Slight fluctuation is felt in the lower part of the abdomen, and there is some pain in the epigastric region when pressure is applied.

Stools frequent and loose, but quite feculent; tongue clean and moist; appetite impaired; skin of a natural heat; pulse 84, soft; diathesis strumous.—℞. *Hydriod. Potass.*, ℈ij; *Tinct. Iodin.*, ʒij. *M. Ft. liniment. tumor. sæpe applic.*

11th.—℞. *Bromid. Potass., gr. xlviij.; Syrup.*, ʒj.; *Aq.* ʒv. *M. Sum.* ʒss. *ter die. Contin. frictio.*

15th. The patient continued as last reported till this morning at nine o'clock, when he was found hemiplegic on the right side, and unable to articulate, but without any impairment of the power of deglutition. His pupils were natural, but contracted sluggishly on exposure to light. He appeared conscious when roused, and cried when spoken to. He pushed out his tongue when desired, and it was observed to be turned towards the right side.

About half an hour before the above changes were noticed, the patient was sitting up and took his breakfast heartily; the paralytic seizure having come on without any premonition, so that he was affected for some time before the nurse, or any of the surrounding patients, perceived anything unusual.

He continued in the above state till noon, when he expired, after having had two convulsions.

Sectio Cadaveris.—March 17th.—The upper half of the left hemisphere of the brain was completely disorganised, and occupied by a large mass of blood, imperfectly coagulated and mixed round the edges with pieces of softened cerebral substance. The rest of the brain, as well as the membranes, appeared healthy, and no change in the vessels supplying the organ could be observed.

The thoracic viscera were healthy.

The abdominal cavity contained about 20 oz. of serum. The spleen was enormously enlarged, and found to occupy a space corresponding to the lines indicated in the first report, its weight being 5 lb. 10 oz. avoirdupois. The liver was likewise enlarged, and weighed 6 lb. 9 oz. Both kidneys were natural.

The blood throughout the body was apparently not coagulated. As the sternum was being disarticulated, the right *vena innominata* happened to be punctured, and the blood which escaped appeared to have a mixture of a cream-coloured fluid, which was now floating on the top of it. This led to a farther investigation, and it was found, on opening the principal veins, that the blood communicated a greasy sensation to the fingers, that it had a dirty purple colour, and contained the above-mentioned ingredient, which was evidently most abundant in the splenic vein. Some of the blood effused into the brain was collected and set aside, as also some from the *vena cava* inferior. In both specimens, after repose, the creamy fluid separated from the blood corpuscles, between which and the serum it lay floating. In the specimen from the *vena cava* the creamy deposit was twice the bulk of that of the blood corpuscles, but the reverse was the case in the other. On subjecting a drop of the fluid to the microscope it was found to consist of well-formed pus globules, a preponderance of which, over the coloured corpuscles, was presented by a drop of blood from the spleen itself.

Remarks by Dr Wallace.—In this case there existed hypertrophy of the spleen and liver, complicated with diarrhœa, and terminating in hæmorrhagic apoplexy. In the last respect it differs from all the cases hitherto recorded, but manifests, at the same time, the tendency to sanguinous effusion, which appears in such cases to be not uncommon. It will be observed that no notice is taken of the state of the lymphatic glands, either during life or after death, although it is mentioned that the patient was of a strumous habit. But this apparent omission is easily explained, for in drawing up my reports I made it, for the sake of brevity, an almost invariable rule to record

few of the appearances which might be regarded as natural. Independently of that, however, my recollection of the circumstances of the case is so vivid that I can with confidence assert, that the glands were not enlarged, and, besides, that the boy had never been out of Glasgow, and at no time had any fever of the intermittent class. The *post mortem* examination was conducted in the presence of Dr J. A. Easton and Professor Wm. Thomson, the latter of whom thought it probable that the blood would exhibit an appearance similar to that witnessed in Dr Craigie's case, at the inspection of which, if I mistake not, he had been present. I accordingly subjected it to a microscopical examination, and found the conjecture to be correct, the blood being actually loaded with what I regarded as pus corpuscles,—an observation confirmed by Professor Allen Thomson and his demonstrator, Dr Aitken, to whom a specimen had been sent for that purpose.

In a letter, just received from Dr Sandwith, of Hull, I learn that the boy Tinlay (Case II.) died at Hull, on the 22d. The particulars of the post-mortem examination will be given on a future occasion. Another case of leucocythemia has also just fallen under the notice of Dr Gairdner; and anxious, before entering into any generalisations, to extend as much as possible the number of facts regarding this mysterious disease, I consider it better to postpone a general summary until some future occasion.

(*To be continued.*)

ARTICLE II.—*An Account of Scarlet Fever, as it Appeared in George Watson's Hospital, Edinburgh, during the Spring of* 1851. By BENJAMIN BELL, Esq., F.R.C.S.

(*Read before the Edinburgh Medico-Chirurgical Society, 9th July* 1851.)

GEORGE WATSON'S HOSPITAL is a charitable institution, intended principally for the maintenance and education of the sons and grandsons of members of the Merchant Company. The building, which is commodious and well ventilated, is surrounded by several acres of pleasure ground, and stands upon the southern slope of the Old Town, overlooking the Meadows, and commanding an uninterrupted view of the country beyond. The internal arrangements of the house are also favourable to the health of the inmates—every proposal likely to serve that important end being heartily welcomed by the governors. It may be added, that no death had occurred for a period of two years preceding the epidemic of which I am about to give an account, although the household commonly includes more than a hundred individuals, four-fifths of whom are under fifteen years of age.

This marked freedom from serious disease is partly owing to the circumstances already mentioned; but it may also be referred, in some measure, to the following causes:—1. The stringent rule laid down by the founder of the hospital as to applicants for admission, being "sound and wholesome in their bodies and minds;" 2. To the prompt attention which can be given in this and similar institutions to the earliest symptoms of disease; and, lastly, to the invaluable remedy which we now possess for scrofulous cachexia—I mean the cod-liver oil,—a remedy which our excellent professor of physiology had the great merit, several years ago, of reintroducing and pressing upon the notice of an almost incredulous profession.

When the recent epidemic commenced, the inmates, including the gate-keeper and his family, who were in constant communication with the house, were as follows:—Eighty-two boys betwixt the years of seven and fifteen, three masters, the matron, eight female servants, two male servants, their wives and three children; in all, a hundred and one individuals. Of these, it was ascertained, after careful enquiry, that twenty-eight boys and three adults had previously passed through the disease; so that there were seventy persons remaining who might be regarded as more or less liable to become affected. The cases which eventually did occur, were twenty-five, all of them hospital boys; and of these, one was believed to have had the disease previously, in early childhood. The first case was reported on the 12th of February; the last on the 7th of May.

Measures adopted for preventing the disease from spreading.—There are two sick-rooms under ordinary circumstances; a large one containing seven or eight beds,—a smaller one containing two. At the commencement of the late epidemic, no additional accommodation was needed, the smaller apartment being reserved for new cases until the features of the disease were fully developed, after which, the patients were transferred to the larger sick-room. As the cases multiplied, space was found for a few additional beds in both apartments; but even these proved insufficient, and it became necessary, at last, to transform one of the dormitories into a temporary sick-room—an arrangement which completely met the emergency. All reasonable care was taken to prevent communication, both direct and indirect, betwixt the boys who were well and the attendants on the sick; and, among other precautions, it may be mentioned, that whenever a new case appeared, the bedding, &c., was always transferred to the sick-room along with the boy himself. Cleanliness was attended to in every particular; ventilation was maintained by a good fire in each of the rooms, and a cautious management of the doors and windows; while the ordinary disinfecting agents were not neglected.

The cases, as already mentioned, were spread over a period of three months, and I see no reason to believe, that fresh importations of the contagion were brought from without the walls of the hos-

pital, because, although the boys were not hindered, during the whole epidemic, from visiting their friends as usual, once a week, strict inquiry was always made as to the several families being free from disease. Besides this, the general alarm which prevailed, very naturally, among all parties, rendered any such exposure to contagion highly improbable. We may infer, therefore, that, in spite of all the care which was taken, the disease continued to spread during these three months, chiefly, if not wholly, by communications of some kind, either direct or indirect, within the building itself.

Internal administration of Belladonna.—Conceiving that no means for arresting the disease ought to be neglected, and that a favourable opportunity now offered itself for testing the alleged prophylactic virtue of belladonna, I determined to give it a full and fair trial. Accordingly, on the 21st of February, upon the appearance of a second case of scarlet fever, the fifth part of a grain of the extract was given, morning and evening, to each of the boys. The dose was found, in a few days, to be too large, from the dilated state of the pupil and impaired vision which it occasioned in several instances. It was accordingly diminished, and then administered, without interruption, to all the boys who continued well, until the 7th day of June,—a full month after the last case of scarlet fever had occurred. It is important to remark, that the second case already referred to had been in the sick-room, separated from the rest of the boys, for more than a week before the symptoms of scarlet fever appeared, and that no additional case occurred until the 21st of March, an entire month after the belladonna had been regularly administered. There was thus ample time for the manifestation of its virtue as a prophylactic; but the subsequent occurrence of so many cases seems to throw considerable dubiety over the existence of any such power. No experience of a merely negative character can be regarded as of much weight, when contrasted with this positive experience now detailed. It is by no means unusual to meet with only two or three cases of scarlet fever, in a large assemblage of children, without the belladonna having been used at all; and, therefore, we are not called upon to give it the credit of securing a similar exemption in cases where it has been administered; but, surely, the occurrence of twenty-three cases out of fifty-four boys, who might be legitimately reckoned liable to the disease, is an overwhelming evidence on the opposite side.

Possibly, the administration of the drug may be useful in some cases, by inspiring confidence, and so indirectly fortifying the system against contagion; but we cannot divest ourselves of the impression, that the continued use of a narcotic for weeks together, even in small doses, must be prejudicial to health, and that thus, while failing to defend the individual against infection, it may render him less able to cope with the disease when it really comes. Certainly, a large proportion of the boys who took the belladonna, seemed to

have more or less furring of the tongue, impairment of appetite, and other evidences of slight indisposition.

General history of the epidemic.—Most of the cases may be described as specimens of *scarlatina anginosa*, all the primary symptoms being characteristic and well marked. In two or three instances, the affection of the throat was so slight, that it would have escaped notice altogether, if the patients' testimony had been deemed sufficient; but ocular inspection always discovered a redness and soreness of the fauces. We had, therefore, no case of the *scarlatina simplex*, understanding, by that term, the mere cutaneous efflorescence, unaccompanied by any affection of the throat. On the other hand, three of the cases were so strongly marked, and displayed so many symptoms of intense constitutional poisoning, that they almost came under the head of *scarlatina maligna*. One distressing case, which proved fatal on the fourth day, belonged, very decidedly, to that variety. We had no specimen of the *scarlatina faucium*, in which the sore throat is unaccompanied by any redness on the skin; although, in two of the boys, the efflorescence was so very limited as to be hardly perceptible. I have remarked that, in such instances, traces of the rash may generally be found at the elbows and knees, although absent on other parts of the surface.

Several of the boys suffered a good deal after the eruption began to fade, and even earlier in the disease, from pain and stiffness in some of their joints; and three, in particular, may be said to have laboured under acute rheumatism, with well-marked affection of the endocardium, requiring special and active treatment. In one of these cases, the primary symptoms of scarlet fever were severe; and I shall therefore give the history somewhat in detail, with the view of illustrating, by a particular example, the general plan of treatment which was followed during the epidemic.

CASE.—March 27 1851.—T. T., a stout healthy-looking boy of thirteen, felt his throat rather painful yesterday afternoon, but made no complaint. He awoke this morning at five o'clock, sick and disposed to vomit, and has been retching several times since. He is pale; the pulse frequent; fauces red; tongue thickly furred; breath offensive.—8 P.M. The fauces swollen, with a white spot on the left tonsil. To have a warm bath immediately.

28th. A scarlet rash is appearing on the upper part of the body; on the face it is rather livid. His eyes are suffused; his pulse rapid; the fauces very red, and tonsils much enlarged. A warm bath.

29th. The rash is coming out in a satisfactory manner. Pulse 110, and soft. Urine not affected by heat or nitric acid. The solution of nitrate of silver was applied to the throat.—8 P.M. Much moaning and delirium all day; and he complains of pain and stiffness in some of the larger joints. The rash is now pretty distinct on the lower extremities. He greatly enjoys tepid sponging of the surface. The bowels not having been moved since his illness, he is to have a teaspoonful of castor oil at bed time.

30th. The bowels have been freely moved, and the delirium has ceased. He feels better, and has taken some farinaceous food. The countenance is still flushed and rather livid, but the legs and feet are covered with a bright efflorescence. Pulse 115. Urine intensely acid, not affected by heat. The caustic

solution is applied to the fauces; the tepid sponging to be persevered with.—8 P.M. More comfortable; countenance improved; eruption vivid; he delights in the sponging.

31st. Has had a good night. Pulse 110. Urine 1025, acid, not affected by heat or nitric acid.

April 1. Much swelling of throat; the uvula and left tonsil have an ash-coloured surface. Pulse 105. To have a bath.—8 P.M. On coming out of the bath this forenoon, some purple blotches were observed on the skin, but they are no longer visible.

2d. Going on well; the throat still greatly swollen and sore; the caustic solution applied. Urine 1025, acid, depositing lithates, not affected by heat or nitric acid.

4th. Throat easier. Urine acid, not affected by heat, but effervescing on the addition of nitric acid.—8 P.M. Suffering much from pain in the shoulder joints.

5th. Almost every joint so painful that he is quite unable to move himself in bed. Urine barely acid, 1020, not affected by heat or nitric acid. To have a teaspoonful of lemon-juice in water every two hours.

6th. Still suffering from pain in the joints. Pulse 100. Throat nearly well. To continue the lemon-juice; to have a teaspoonful of paregoric at bed time, and castor oil in the morning.

7th. Less pain, but still unable to turn himself in bed.—8 P.M. Some irregularity of pulse, which beats 120 in the minute; increased action of the heart, and a bruit accompanying its first sound. Four leeches to the cardiac region; and a pill of calomel and Dover's powder every three hours alternately with the lemon-juice.

8th. Much relieved by the leeches. Pulse 100, soft but irregular. There is still some roughness accompanying the first sound of the heart. He complains of pain in his right knee, his neck and both shoulders. The pills and the lemon-juice to be taken every four hours alternately.

9th. Pulse less frequent, but intermitting once in fifteen beats. Joints easier. Bowels moved five times. Urine 1025, muddy from lithates, cleared by heat; no trace of albumen.

10th. Pulse 80, quite regular. Still complaining a little of pain in the right hip and knee.—8 P.M. A roughness with the first sound over apex of the heart; and a peculiarly delicate sawing sound over the aortic valves. Still he feels in all respects better. The pills to be continued.

11th. Much the same.

12th. The delicate sawing sound gone, and in its room a bruit de soufflet. Pulse calm. Some tenderness of gums. The pills to be discontinued; the lemon-juice once in six hours. He is now able to get out of bed and back again, without assistance.

15th. Is taking two grains of blue-pill night and morning. Urine, 1016, acid, not affected by heat or nitric acid.

22d. Seems to be well. Urine 1016, acid, no traces of albumen.

29th. Went home to his friends.

Remarks on preceding Case.—Use of Purgatives.—The irritability of stomach was so great at the outset of this case, that it seemed unnecessary to trouble him with the ordinary diaphoretic mixture of acetate of ammonia and sweet spirit of nitre, which was administered to most of the other boys. The warm bath, however, and tepid sponging of the surface were assiduously employed, and the eruption at last made its appearance in a satisfactory manner. Before this latter object is gained, I am not in general very anxious about having the bowels moved; but when once the rash is fully out, I believe that the use of a gentle laxative should not be neglected;

and the advantage of attending to this point was very apparent, not merely in the case now related, but in several others during the late epidemic. There is not unfrequently a decided sluggishness of the bowels in this disease; and, therefore, when we consider that the cutaneous function is necessarily performed in an imperfect manner; that in all probability a materies morbi of some kind requires to be eliminated; that the kidneys are prone to become disordered at a subsequent stage, and that the large intestines are important organs of excretion, the practice of giving mild purgatives in scarlet fever seems to have much to recommend it. One or two drachms of castor oil usually answered the purpose in view; but more than once, at an advanced period of the complaint, the pulv. jalap. compositus was used in preference. I am disposed to think that the late Dr Hamilton was the author of a real improvement, when he advocated the moderate use of laxatives in scarlet fever, as well as in several other diseases where they had been previously condemned, although in accordance with the common tendency to run into extremes, the practice has been sometimes doubtless carried to a hurtful extreme, in the hands of other practitioners less sagacious and less skilful than himself. But of recent years there has been a reaction in the opposite direction; and it is not improbable that an exaggerated dread of irritating the mucous membrane, may lead us to neglect entirely a class of medicines which are capable of performing important service.

Rheumatic Symptoms.—I prescribed the lemon-juice when the rheumatic symptoms became strongly marked, from having lately found it of great use in several cases of acute rheumatism, when given according to the plan recommended by Dr G. Owen Rees. Certainly the symptoms have yielded, in my experience, more rapidly than under any other treatment with which I am acquainted; while the remedy has the advantage of being an agreeable one to most patients, and may be steadily continued, even should cardiac symptoms supervene, without interfering with more active measures. An opiate given at night and a mild aperient every second morning will be found, I believe, almost the only adjuvants required for conducting most cases of rheumatic fever to a safe and satisfactory termination. Instead of entering upon the treatment of such cases, as formerly, with much doubt and anxiety, I believe that we are now in possession of a remedy which may be used with confidence; although it is quite possible that further experience may show that this favourable opinion has been too hastily formed.

Character of the Efflorescence.—The rash was slow in coming out, and not very abundant in this and several other cases—a circumstance which may be generally regarded as unfavourable, rendering the patient more liable both to the rheumatic affection and to the no less formidable sequela of albuminous urine. One boy, in whom the rash was barely perceptible, had a remarkable tendency to profuse perspiration, which continued with little intermission for two or three

days. The affection of his throat was at the same time severe, and very characteristic; and during convalescence he suffered from a smart attack of pneumonia on the left side of his chest. In some of the boys the efflorescence was intense, and the subsequent desquamation very abundant. In one of them, while this process was at its height, I found the urine loaded with epithelial cells, but not rendered opalescent by either nitric acid or the application of heat. In one or two others, at the same period, the urine was loaded with crystals of the phosphates and strongly alkaline, effervescing and becoming beautifully clear on the addition of nitric acid. In four of the severer cases, the rash had a livid hue instead of the bright scarlet usually observed. This was invariably accompanied by much prostration of strength. The poor boy already mentioned, who died upon the fourth day, had this lividity in a marked degree, at least on the face, hands, and feet. He had, at the same time, much irritability of stomach, an anxious expression of countenance, suffusion of the conjunctiva, a smooth dry tongue, a rapid and feeble pulse, twitching of the tendons, and incessant restlessness. The throat was severely affected, but this symptom, irrespective of the other symptoms, would not have been considered very formidable. The vital powers gave way rapidly, and his demise was sudden, almost unexpected. The other three cases, in which the eruption was livid, presented to some extent a similar train of symptoms, and gave rise to no small anxiety. With them I had recourse to the internal use of the chlorate of potash, having often seen it extremely beneficial in certain affections which appear to be characterized, among other circumstances, by defective oxidation of the blood, such as cancrum oris,[1] sponginess of the gums, and some forms of purpura. Without wishing to express a decided opinion, when the data are so very limited, I am disposed to think that the salt in question was of use in these cases of scarlet fever. After employing it, upon the grounds already mentioned, I was not a little gratified to find, that Dr Watson of London has been lately in the habit of prescribing it, under similar circumstances, and that he considers the practice decidedly beneficial. Having made these cursory remarks upon the principal features of the late epidemic in the primary stages of the disease, and alluded to some details of treatment, I may now be permitted to advert very shortly to the instances of acute albuminuria.

State of Urine.—I made a point of examining the urine frequently in all the cases of scarlet fever, with one or two exceptions. This was not done every day, but the observations were numerous; and no instance occurred of albumen being found in the urine, independently of dropsical symptoms more or less strongly marked. The

[1] The profession are certainly much indebted to Dr Hunt for his interesting paper upon the use of chlorate of potash in that very formidable affection. I am not acquainted with any remedy which better deserves to be called a specific.—*V. Medico-Chirurgical Trans.*, v. xxvi.

cases in which albuminuria co-existed with dropsy were five in number; and in all of them the two phenomena were detected simultaneously. In three, the occurrence took place on the nineteenth day of the disease; in one, upon the twenty-third; in one, upon the twenty-seventh. But in none of these five cases at an earlier date, and in none of the remaining twenty, were traces of albumen perceived, although the observations were made very carefully, with both heat and nitric acid, and amounted to one hundred and forty in number. In this point, my experience differs from that of several recent observers, among whom may be mentioned Drs J. W. Begbie and Patrick Newbigging, who aver that albumen, in small amount, will be found to exist in every case of scarlet fever; but these gentlemen seem to admit that the quantity detected, when dropsical symptoms do not exist, has been comparatively insignificant, and unaccompanied by any marked diminution either in the amount or the specific gravity of the urine. We all agree that when dropsical symptoms *do* supervene, the albumen is generally abundant and the urine scanty; and my own experience is, that the specific gravity must usually be regarded as low, when allowance is made for the smallness of the discharge. For example, in one of the five cases already referred to, the specific gravity of the urine on one occasion did not exceed 1012, while no more than six ounces were voided in the course of twenty-four hours.

A common idea prevails, that the *sequela* we are considering is met with most frequently in cases which have been mild in the primary stages. This may have arisen, in some measure, from the circumstance that such mild cases are less carefully watched, and so exposed to unfavourable influences at a period of the convalescence when the kidneys are peculiarly prone to become congested. I infer that this tendency to renal congestion does exist, both from previous observation, and from what was witnessed during the late epidemic. Every care was taken to prevent exposure. None of the patients were allowed to leave their beds until nearly four weeks had elapsed from the commencement of their illness; the functions of the skin were encouraged by the regular use of the warm bath, brought into the sick-room; laxative medicines were given in the cautious manner already mentioned, and the diet was carefully regulated; and yet a fifth of the whole number of scarlet fever cases were affected with acute albuminuria, and all of them, with a single exception, had been severely ill during the primary stage. The exceptional case, although mild up to the nineteenth day, proved speedily fatal after the supervention of dropsical symptoms; and there was every reason to believe, from examination after death, that previous and chronic disease of the kidney had rendered the individual more liable even than usual to the renal disorder. I believe, therefore, that the common opinion, that mild cases are peculiarly prone to albuminuria, is probably erroneous, although we may perceive how it came to be formed.

As the *sequela* under consideration is one of great interest, and still imperfectly understood in some of its bearings, I shall make no apology for the following short account of the cases in the order of their occurrence.

Case.—J. K., æt. 11, was brought to the sick-room in a febrile state on the 12th of February, the same day that the first case of scarlet fever occurred. His illness seemed to be connected with inflammation of the ear, and passed off in a few days. On the 20th, however, he awoke during the night with a painful choking sensation in his throat; and in the morning the fauces were found to be red and swollen, and the uvula very œdematous. He had a frequent pulse and hot skin. On the following day an obscure rash appeared, which gradually passed into the characteristic efflorescence of scarlet fever. A few leeches to the throat, the solution of nitrate of silver, tepid sponging of the surface, a diaphoretic mixture, and one or two small doses of castor oil, were the remedies employed; and in four or five days the fever had abated. He continued free from complaint until March 11th, the nineteenth day since the commencement of the fever, when a puffy state of the cheeks and eyelids made its appearance. Upon examination, the urine was found to be scanty, of a dirty olive colour, and somewhat muddy. It became whey-like on the application of heat, and had a specific gravity of 1012. Ten grain doses of the acetate of potash were prescribed every four hours. He took the medicine once or twice, but it made him sick, and so did the antimonial solution. Next day the pulse was 52, and irregular, the skin hot, the tongue furred, with frequent calls to void his urine, which was now reduced to five ounces in the twenty-four hours, and highly albuminous. He was bled from the arm to the extent of four or five ounces, and warm fomentations were applied to the loins.

13th. The urine still very scanty, not exceeding three ounces in the twenty-four hours, albuminous, and containing fibrinous casts of the tubuli uriniferi when examined under the microscope. The stomach continued all this time very irritable, little or no food being retained. Although he complained of no uneasiness in the region of the kidney, I thought it right to take a small quantity of blood by cupping; and on again visiting him, after the interval of a few hours, he was perspiring very profusely, and had retained some boiled rice and milk.

14th. The perspiration had continued all night, and the vomiting had ceased. Urine six ounces during the last twenty-four hours, 1012, and still albuminous. A drachm of the aqua acetatis ammoniæ to be given in water every four hours.

15th. Urine increased to sixteen ounces, but still albuminous.

17th. Urine steadily increasing in quantity, 1008, and less albuminous. When allowed to stand, depositing lithic acid crystals, of a pale colour, in considerable abundance; but the casts of the tubuli uriniferi were no longer seen. He now took his food with relish, and slept soundly during the night.

18th. Urine increased to thirty ounces, 1010, of a dirty hue, and distinctly albuminous; pulse 60; some œdema of the palpebrae still perceptible. To have three drops of Tinct. Lyttae three times a-day.

19th. No change. To have a warm bath, and to continue the Tinct. Lyttae.

21st. No traces of albumen, but a copious deposit of lithic acid crystals.

23d. The warm bath, followed by friction of the surface, is employed every second day. The urine continues much the same.

30th. His countenance is pale, with some fulness of the palpebrae. Urine 1010, very abundant, of a reddish tint and neutral reaction, although he has been taking for several days five minims of the diluted nitric acid before his meals.

April 1. Urine still of a red hue, like water in which flesh has been washed. Imperfect blood-globules are seen under the microscope, and a slight opalescence is observed on the addition of nitric acid. To discontinue the nitric acid, and to take, instead of it, five drops of the Tinct. Muriat. Ferri in a glass of water three times a-day.

3d. Urine much more natural in colour, acid, 1016; opalescent on the application of heat, but rendered perfectly clear by nitric acid.

7th. Urine abundant, of natural colour, 1010, and free from all trace of albumen.

He had no return of the complaint, and is now (July 4th) strong and robust, with the bloom of health on his cheeks.

CASE.—J. F., æt. 11, was taken ill on the 29th of March, and had a smart attack of the disease. The eruption was abundant, and the sore throat very characteristic. He suffered much from earache, and a troublesome irritating cough, both of which symptoms were manifestly relieved by the application of leeches. The warm bath and one or two mild laxatives were employed as usual. The urine was repeatedly examined, but nothing abnormal was observed until April 25th, the twenty-seventh day, when it had a dingy colour, deposited a dark sediment, and was rendered opalescent by heat and nitric acid. Simultaneously with this state of the urine, he had puffiness of the palpebræ, a furred tongue, and full febrile pulse. An aperient was administered, with the effect of bringing away a large quantity of dark coloured and offensive matter. Two days afterwards, the bowels being in a better state, and the feverish symptoms gone, he began to take the tincture of the muriate of iron three times a-day. On the 1st of May, the urine was nearly of its natural colour, 1020, acid, and free from albumen.

He continued the iron for a week or two, and soon recovered his former health and strength.

CASE.—W. C., æt. 12, was taken ill with scarlet fever on the 2d of April, and had the primary symptoms in a severe form. The eruption came and went; he had a rapid pulse, a dry tongue, and frequent vomiting. The last symptom was extremely obstinate and distressing, but subsided on the rash coming out in a more satisfactory manner, towards the evening of the 5th. The urine continued natural until the 25th, the twenty-third day of the disease, when a puffiness of the palpebræ being observed for the first time, it was found to be small in quantity, 1013, of a dingy hue, acid, and rendered opalescent by heat and nitric acid. The pulse was 80, labouring and intermittent. A warm bath immediately; the diaphoretic mixture to be resumed.

26th. Skin moist; pulse 80, still intermitting. Urine more abundant, but of a dingy colour, and rendered cloudy by heat and nitric acid.

28th. Urine the same in appearance; depositing a dark coloured sediment, consisting of fibrinous casts, lithic acid crystals, and epithelial scales. The Tinct. Muriat. Ferri to be given three times a-day.

May 1st. Urine 1012, acid, exhibiting a slight cloud under heat, and nitric acid.

3d. The albuminous cloud in the urine hardly perceptible.

6th. Urine of natural colour, acid, and without trace of albumen.

He continued taking the tincture until the 20th, when he went to the country for change of air. He is now in perfect health.

CASE.—J. K., æt. 11, small for his years, pale, and of delicate appearance, was taken ill of scarlet fever on the 11th of April. The disease was well marked, but mild, and after the first few days seemed to require no other treatment than warm baths, confinement to bed, and a farinaceous diet. On the nineteenth day (April 29th) some œdema of the palpebræ was observed. He had a furred tongue and offensive breath, but the pulse was calm and the skin cool; the urine 1015, of a dirty green tint, acid, and rendered opalescent by heat and nitric acid.

30th. No change of symptoms until the evening, when he was seized with a sudden and severe pain in the cardiac region, and difficulty of taking a full inspiration. Two leeches removed the pain.

May 1st. A frequent, short, and dry cough; but no dulness remarked on either side of the chest. The eyelids still œdematous. Urine 1015, acid, and slightly clouded by heat and nitric acid. Pulse calm and soft.

3d. No change in the symptoms; but he is more than usually pale. To have five minims of Tinct. Muriat. Ferri three times a-day.

4th. Some shortness of breath came on during the night, and still continues. There is dulness on percussion on both sides of the chest, behind and at the lower part. Urine decidedly albuminous.

5th. The cough and shortness of breath seem to be less troublesome; the urine 1015, rendered opalescent by heat and nitric acid. He is cheerful, and amused by his companions who are convalescent in the neighbouring beds. A large sinapism to be applied to the lower and back part of the chest. The tincture to be continued. The nurse was in the act of applying the mustard when the little patient called out that he was unable to breathe; the short cough became extremely urgent for a few moments; he sat up, clinging to the neck of one of the attendants, and almost immediately fell back in her arms and expired.

Being anxious to have an accurate *post mortem* examination, I requested the valuable assistance of my friend, Dr W. T. Gairdner, who has kindly furnished the following

NOTES OF THE MORBID APPEARANCES.

The body pale; lips a little livid; some œdema of the face; the heart healthy. Each pleura contained at least six or seven ounces of serous fluid. The lungs compressed and carnified at the back part, presented anteriorly marked emphysema, with patches of collapsed tissue marked off by the interlobular septa. The bronchi contained a good deal of fluid; the mucous membrane not much congested. The peritoneal cavity contained no fluid. *Liver* normal. *Spleen* adherent to the diaphragm at various points; its capsule thickened on the outer surface; the organ enlarged (probably may have weighed seven ounces); its texture firmer than natural. *Kidneys* normal in size; the surface somewhat irregular and uneven, but its vascular distribution nearly normal. On section the pyramids were of normal appearance; the cortical substance remarkably thin, at many parts not more than one-sixth of an inch; its vascular striæ not well marked, and few Malpighian bodies visible; its colour, however, and consistence normal. On microscopic examination, the epithelium scarcely appreciably diseased,—but if anything, smaller and more irregular in form than natural; no fibrinous casts; no fatty granules; no thickening of the tubuli; but several pale rounded bodies were observed like withered cysts, which were, I think, very probably the shrunk capsules of the obliterated Malpighian bodies. They were of tolerably uniform size, and about half the natural diameter of these capsules.

Case.—J. M., æt. 9, a delicate-looking boy, was taken ill April 15th, and had the disease in a very severe form. He was in an anxious state for a good many days; but after escaping various dangers, seemed to be nearly well, when, on the nineteenth day of his illness (May 4th), he again became feverish, with a hot and dry skin, a thickly furred tongue, and a frequent pulse. Simultaneously with these symptoms, some puffiness of the eyelids was also observed; and the urine was high coloured, scanty, and extremely albuminous. He was ordered the mixture, with acetate of ammonia, and sweet spirit of nitre every two hours.

5th. Less heat of skin; pulse 90; face œdematous; urine scanty, and loaded with albumen. Five drops of tincture of digitalis to be given with every second dose of the diaphoretic.

6th. 10 A.M. Urine more abundant, 1016, albuminous. A warm bath.—8 P.M. The bowels moved without medicine. Skin hot; pulse 100; urine scanty, resembling coffee-grounds; under microscope, blood globules, casts of tubuli and agglomerated crystals of lithic acid. Four leeches to be applied to the region of the kidneys; the diaphoretic mixture and the digitalis to be continued.

7th. The leeches did their duty well. Pulse 80, and intermitting. Urine still scanty, and dark-coloured as yesterday; rendered quite opaque by heat. Epi-

thelial cells; distinct nucleoli; casts of tubuli and crystals of lithic acid. Cupping-glasses to be applied to the loins.—8 P.M. Has been more comfortable all day; no thirst; pulse 72, intermitting; urine much more healthy in appearance, 1012, acid, and less albuminous. A warm bath.

8th. Some appetite for food; urine 1016, acid; little trace of albumen. A warm bath; dry cupping of the loins; the digitalis to be withdrawn.

10th. A steady improvement; but the puffiness of the palpebræ still continues. Dry cupping; a warm bath; the diaphoretic mixture.

11th. A large flow of limpid urine; œdema nearly gone.

12th. To have five drops of the Tinct. Muriat. Ferri twice in the day.

13th. Urine pale, acid, 1013, no trace of albumen. A warm bath and dry cupping.

14th. Feverish and thirsty during the forenoon and towards evening, very drowsy, almost comatose. The skin hot and dry; pulse 130; tongue covered with a white fur; urine scanty, of a bloody tint, 1013, acid, and exhibiting an abundant dark flocculent cloud on the application of heat. Four leeches to the loins; the diaphoretic mixture every two hours. Feeling anxious as to the issue of the case, I requested Dr Begbie to visit him with me late in the evening. The leeches had bled well, but the symptoms continued much the same. We agreed to give five drops of tincture of digitalis immediately, and again during the night.

15th. Dr Begbie paid him another visit this morning. He had been decidedly relieved after a copious evacuation from the bowels; his skin was cool, and the pulse had come down about thirty beats. A warm bath; the digitalis with ten grains of acetate of potash, three times in the day.

16th. Intense thirst; no appetite; skin again hot; pulse 104. Urine scanty, dark-coloured, and albuminous. A warm bath; dry cupping of the loins; the acetate of potash to be withdrawn, and five drops of the tincture of muriate of iron to be given alternately with the digitalis.

17th. A good night; appetite returning; urine more natural in appearance; little trace of albumen. The medicines, dry cupping and warm bath to be continued.

20th. Pulse 78, intermitting; œdema gone; bowels moved by castor oil; urine of natural colour, 1013, acid, almost entirely free from albumen.

The digitalis once a-day; the muriate of iron thrice.

He continued to improve steadily from this date; the albumen entirely disappeared in a few days; and he has been quite well for some weeks.

The phenomena observed in these, and other cases of acute albuminuria following scarlet fever, seem to be very consistent with the pathological views[1] so ably propounded by Dr George Johnston. He suggests as very probable, that a morbid poison reaching the secreting cells of the tubuli uriniferi modifies them so much that they are thrown off entire, and may be detected in the urine; that in consequence of this, there is an imperfect elimination from the blood of the proper renal secretion, while, at the same time, the tubuli become filled and distended with epithelial cells, and the blood, thus imperfectly purified from excrementitious matter, stagnates in the intertubular capillary plexus; that then the Malpighian tufts and the arteries which supply them become gorged with blood,—and that all this pressure and distension of vessels leads to that escape of serum and blood with the urine which is so characteristic of the disorder. According to this view, the blood is supposed to come from the Malpighian capillaries, which lie within the dilated extremities

[1] Medico-Chirurgical Transactions, vol. xxxiii.

of the tubuli; its serum flows into the tubuli, mingles with the urine, and renders it albuminous, while the colouring matter and fibrine coagulate in the tubuli, and afterwards escape in the form of cylindrical moulds or casts, in which epithelial cells are commonly entangled. We do not know if Dr Johnston's rationale of acute albuminuria will pass the ordeal of a rigorous criticism in all its details; but it certainly contains the elements of important truth, and gives encouraging promise of future discoveries. One practical inference seems to be clear,—that to stimulate the kidneys with powerful diuretics in this disease, at lèast in the earlier stages, must be contrary to sound principles of pathology and therapeutics. The organ is already oppressed and labouring, and seems to be doing its utmost to eliminate a poison from the blood. Should we not endeavour to relieve it, by promoting as much as possible the functions of the skin, and also by increasing the solid excreta from the large intestines which Liebig calls the chimney of the organism? Local bleeding in the lumbar region is also clearly indicated; and the frequent application of cupping-glasses will probably be useful when no more blood can be spared.

The tincture of the muriate of iron was prescribed in four of the cases after the febrile symptoms had subsided. It seemed to be indicated by the anæmic state of the patients; and being a mild diuretic, I hoped that the kidney, having partly resumed its proper function, might be assisted by it in clearing the loaded tubuli and getting rid of the fibrinous casts. Right or wrong in theory, the practice appeared to be useful. In the last case, I was encouraged to prescribe the tincture during the persistence of acute febrile symptoms, partly by the recommendation of Dr Begbie, who had seen it useful in a case of hæmaturia, and partly by the interesting paper lately presented to this Society by Mr Hamilton Bell and his brother on the treatment of erysipelas. The benefit apparently derived from it was remarkable. May there not be a closer analogy than we suppose betwixt the two affections, at least in so far as regards the condition of the capillary vessels?

ARTICLE III.—*On the Natural History, Action, and Uses of Indian Hemp.* By ALEXANDER CHRISTISON, M.D., Edin., Annual President of the Medical Society of Edinburgh.—(*Continued from p.* 45.)

INDIAN hemp appears to possess a remarkable power of increasing the force of uterine contraction during labour, and as I have not observed any special remarks on the subject, the following instances of its employment may be interesting. It was given in a few cases in the Maternity Hospital of Edinburgh, and the following short remarks appear in the Case-book of November 1849:—

One woman, in her first confinement, had forty minims of the tincture of cannabis one hour before the birth of the child. The os uteri was then of the size of a shilling, the parts very tender, with indurations around the os uteri. The pains quickly became very strong, so much so as to burst the membranes, and project the liquor amnii to some distance, and soon the head was born. The uterus subsequently contracted well.

Another, in her first confinement, had one drachm of the tincture, when the os uteri was rigid, and of the size of a half-crown; from this the labour became very rapid.

Another, in her first confinement, had also one drachm of the tincture, when the os uteri was of the size of a half-crown. Labour advanced very rapidly, and the child was born in an hour and a-half. There were severe after-pains.

A fourth had ʒiij. of the tincture, in divided doses, which much accelerated and increased the pains. She had then chloroform for six hours.—[I have since been informed, that the severity of the pains was so great as to cause some alarm, and chloroform became necessary to produce insensibility.]

Shortly afterwards, having directed my particular attention to this action of hemp, I gave it in several cases in the Maternity Hospital, and observed whether it had any effect upon the duration of the pains or intervals. The following tables are examples of the way in which the observations were made, being similar to those of Dr Simpson in his experiments upon the effects of galvanism on the uterus; but I shall not give that of each case, as I am not inclined to attribute much importance to this part of the experiment. The observation was made in the following way:—The duration of several pains and intervals was observed, then the tincture of Mr Robertson's extract of hemp, of the strength of three grains to the drachm, was administered, and after a few minutes the duration of the pains and intervals was again observed. In the tables the time is shown in minutes and seconds.

CASE I.

BEFORE HEMP.		AFTER HEMP.	
Pains.	Intervals.	Pains.	Intervals.
65″		8 drops given.	
		50″	4′ 45″
		1′ 45″	5′ 45″
		12 drops after half an hour.	
		1′ 45″	4′
		2′ 45″	4′
		1′	—

Case II.

BEFORE HEMP.		AFTER HEMP.	
Pains.	Intervals.	Pains.	Intervals.
50″	3′ 20″	17 drops given.	
57″	1′ 50″	1′ 15″	15″
57″	2′ 32″	20″	1′ 58″

Case I.—Was a natural labour, and eighth pregnancy. The first stage was not completed till twenty-four hours after the woman was seized. Hemp was given four hours before its completion. After the first dose of eight drops, little effect was observed; but after the second of twelve drops, the duration of the pains was increased, and the interval shortened; and it was very obvious that the intensity of the pains, counting from the second pain after the hemp was given was increased; by the fourth or fifth pain the effect wore off, and hemp was not again given.

Case II.—This was a second pregnancy. Seventeen drops of hemp were given in the second stage. The second pain, after the hemp was taken, was lengthened, and the interval shortened; this was not the case with the third pain; but the intensity of the pains was much increased, and the woman was speedily delivered.

Case III.—First pregnancy. Hemp was given in the second stage of labour, and the chief fact observed was increased intensity of the pains; the duration of the pains was slightly increased, and the intervals decidedly shortened, after the second dose of hemp. Twenty drops were first given, and after twenty minutes thirty drops more. Twenty-four hours after, twelve drops were given, and after-pains were induced, which the woman said were "quite as bad as when she took in labour first."

Case IV.—First pregnancy. Twenty-five drops of the tincture were given at the completion of the first stage; after this, both the pains and the intervals were shortened, and the intensity of the pains increased. After one or two pains the effect wore off, and thirty drops more were given at the end of half an hour. The third pain after this became very intense; and pain succeeded pain without intermission for several hours. As there was deformity of the pelvis, chloroform was administered, and delivery accomplished by the forceps.

Case V.—First pregnancy. Hemp was given during the second stage. First thirty drops, and then thirty-five drops after half an

hour, and the patient was delivered during its action. The effect of the first dose was chiefly shortening of the interval at first, and prolongation of the pain; but the effect on the interval was more marked after the second dose. The pains were described by the patient as more intense, and by examination it was ascertained that the head of the fœtus was more forcibly propelled.

CASE VI.—First pregnancy. Thirty drops of hemp were given in the second stage, and the effect was very decided. Previous to the administration of the hemp there had been no progress for an hour, the patient was nervous and excited, and though she complained much of the pains, the contractions of the uterus were felt to be feeble, and the child's head did not move; but on the second pain after the cannabis the contractions became very strong, forcing down the head, and the child was expelled ten minutes after the hemp was given. At the same time there was no decided effect on the duration of the pains and intervals.

CASE VII.—Sixth pregnancy, with the first stage not completed. Thirty-two drops were given, and the action was well marked; the woman said, the second pain after it was the strongest she ever had. After an hour and a-half, forty drops were given, but the action was not attended to; and in an hour and a quarter fifty drops were given, but there was no action on the pains; they became irregular, and the intervals were very long; the case was then allowed to proceed naturally. There seemed to be a tolerance of the remedy; for though 120 drops had been taken, no physiological effect of any kind was induced.

In these cases, then, it does not appear that the duration of the pains or of the intervals was materially affected in all; but in Cases I., II., IV., prolongation of the pain and shortening of the interval were most obvious; while in Case V. a shortening of the interval corresponding to each dose of hemp was observed. Shortening of the interval was in general a more conspicuous phenomenon than prolongation of the pain. Upon the whole, however, I am not inclined to lay much weight upon these results. But there can be no doubt that the *intensity* of the pains was greatly augmented by the hemp, except in the last case, where, after the effects of the first dose passed off, no action followed the repetition of it. This case was an exception to all the others.

It is worthy of remark, that in none of these cases were the ordinary physiological effects produced; there was no excitement or intoxicating action, and there did not seem to be the least tendency to sleep in any of them.

In conclusion, I may state what appears to be the most obvious difference between the action of ergot of rye, and that of Indian hemp. First,—While the effect of ergot does not come on for some considerable time, that of hemp, if it is to appear, is observed within two or three minutes. Secondly,—The action of ergot is of a lasting character, that of hemp is confined to a few pains shortly after

its administration. Thirdly,—The action of hemp is more energetic, and perhaps more certainly induced, than that of ergot.

There appears little doubt, then, that Indian hemp may often prove of essential service in promoting uterine contraction in tedious labours.

More extended experience will show how far these effects may be depended on, and to what cases hemp is most applicable.

MODE OF ADMINISTRATION.

Indian hemp may be administered in several ways. The extract, in the form of pill, produces the most gradual effect, and the disagreeable taste of the solution is avoided; but its action in this form is very uncertain. The following emulsion has been recommended:—A scruple of the extract rubbed in a warm mortar with a drachm of olive oil, to which are added half an ounce of mucilage, and seven ounces and a half of distilled water (Bromfield). But the simplest method is to use the tincture, which should be dropped into a little water, and immediately swallowed. The water may be sweetened with sugar; or an aromatic, as compound tincture of cardamom, may be added. The usual strength of the tincture is three grains of the extract to a drachm of rectified spirit.

The extract may be given in doses of one to six grains; the tincture in doses of ten to thirty drops, for ordinary purposes. Less than thirty drops is of little service in promoting uterine contractions; and greatly larger doses, as much as one or two drachms repeatedly, must be used in the treatment of tetanus, in which disease there is very great tolerance of the remedy.

ARTICLE IV.—*Case of Hysteria; with Albumen, and subsequently Xanthic Oxide, in the Urine.* By DOUGLAS MACLAGAN, M.D., F.R.S.E.

THE nervous affections classified under the convenient and comprehensive name of hysteria, are notoriously so varied in their phenomena, that every practitioner meets with individual examples of the disease, differing in their course of symptoms from any thing which he can find recorded. Few medical men think it worth their while to publish isolated cases, as they are generally more curious than instructive, and seldom lead to any important deductions either in pathology or therapeutics. I should, on these grounds, have hesitated to record the following as a mere instance of hysteria, but for the interest which attaches to the morbid states of the urine which presented themselves in the course of it. This will, I hope, be accepted as my apology for the somewhat lengthy narrative of the case.

The daughter of an Indian officer, aged fourteen and a-half, of scrofulous aspect, marked by puffy cheeks, clear complexion, and thick upper lip, but without any existing strumous disease, lively, intelligent, and of very amiable disposition, growing rapidly, but preserving a moderate degree of stoutness, not yet menstruating, had been under my care for some years, but had never been the subject of medical treatment for anything more serious than occasional catarrhs, and some of the diseases of childhood. Towards the end of January 1850, I was requested to see her on account of deafness which had been observed some weeks before by the lady under whose charge she lived, but which was regarded as trifling, and was ascribed to accidental cold. The dulness of hearing, however, had recently increased so much and so rapidly as to prevent her from going to her classes, and during the preceding few days had terminated in complete deafness. She now stated that she could hear no sound whatever—that she was insensible to the ticking of a watch when placed upon the temporal bones, or between the teeth, and had no sensation of sound in the ears, when desired to blow with closed mouth and nostrils. She spoke readily and in a moderate tone, carrying on conversation partly by speaking on the fingers, and partly by watching the motions of the lips when words were spoken slowly and with much oral gesture. She stated that she had occasional, but not severe, pain in both ears. She had no affection of the throat, the tonsils and fauces generally being in no respect unnatural in appearance, unless a slight paleness of the mucous membrane could be regarded as such. She had, however, for several weeks been affected with that form of chronic coryza, which is a frequent and very troublesome complaint of young persons of strumous habit of body, and which, so far as my own experience goes, is far more frequent in females than males. There was, as usual in such cases, a sense of tumefaction and obstruction in the nostrils, though no marked swelling could be observed, and air could be blown freely through them. There was a constant flow of thin barely purulent discharge, which was occasionally perceptibly fœtid, and always complained of as such by the patient herself, and there was no excoriation or redness of the upper lip, such as occurs in common coryza. The ears were found each to contain a little wax, which was readily removed by the syringe. The right membrana tympani was now seen to be slightly redder than natural; the left was of perfectly healthy appearance. There was no redness or tenderness of the meatus, as she never appeared to wince under the introduction of the aural speculum. The bowels were rather torpid. The general health otherwise was good. She had no pains about the pelvis, loins, or thighs, nor any other indication that menstruation was coming on. Her appetite was good, and with the exception of the somewhat dull and stupid aspect which her deafness occasioned, she seemed to be in the enjoyment of her usual health.

Blisters were applied behind both ears, and on one occasion when

she complained of unusually acute pain in the right ear, leeches were applied over the mastoid process. The bowels were regulated by occasional doses of fluid extract of senna; and with a view to the treatment of the nasal discharge she took twice daily a pill containing a grain of citrate of quinine and iron, with two of chian turpentine.

About the second of February my patient happened to receive a visit from another young lady who had consulted an aurist for a similar train of disorders, viz., deafness and strumous coryza, and whom I have since then had under my care for a series of nervous affections, in some respects resembling those of the subject of the present narrative. Their complaints naturally became the subject of conversation. It was mentioned that the aurist had declared the nasal discharge in his patient to be connected with an exfoliation of bone; and as this excited great alarm in the minds of the relatives of my young patient, and as the deafness had in no respect improved, it was at my request agreed upon that Professor Syme should see her in consultation.

February 6.—Seen by Mr Syme, who agreed with me in scouting the idea of exfoliation, and in regarding the case as one of nervous deafness depending on some constitutional cause, and rather a subject for medical than surgical treatment. As the face appeared rather more puffy than usual, attention was directed towards the state of the kidneys, and the urine was examined.

Evening urine of February 6. Clear pale-greenish with a slight fleecy deposit. Dens. 1009, distinctly, but not abundantly coagulable by heat and nitric acid. The deposit under the microscope was found to consist solely of fine large nucleated epithelium, without any tubular casts or other morbid appearances. She was ordered to take twice daily a pill containing 1 grain of sulphate of iron and two of Barbadoes aloes, instead of the tonic and laxative which she had been using, and the ears were to be stimulated by the introduction of an ointment consisting of ʒiij of lard, ʒi of olive oil, and 12 minims of oil of cloves.

February 9.—The nervous character of the case was unequivocally demonstrated by the occurrence of a smart attack of hysteria, marked by an appearance of choking, a peculiar hawking sound in the throat, jerking and twitching of the extremities, and sudden alternations of delirious talking, singing, laughing, and crying. These symptoms, however, were at the time speedily subdued by a sinapism along the spine, hot fomentation to the feet, and a few doses of spiritus ammoniæ aromaticus.

February 17.—She has continued since the 9th to be occasionally troubled with the hysterical paroxysms, which, however, have been on the whole slight. The deafness remains complete. This evening she suddenly complained that the room was darkened, and that she could see no light, although the gas not far from her bed was burning brightly. I saw her in about two hours after this took

place. She spoke readily, generally in a confused manner, but occasionally quite coherently. All communications made to her were through the sense of touch. She recognized her relatives and myself by feeling our faces, and said that she knew the doctor by his whiskers. When at rest, she lay with her eyes shut, but without any forcible contraction of the orbiculares; the pupils were somewhat dilated, but contracted under the light, though rather sluggishly. She had much jerking of the extremities, had several times attempted to get out of bed, and was very uncontrollable. The tongue was clean; the pulse little quicker than natural, but rendered rapid when she attempted to get out of bed. The face was rather flushed, but the heat of the skin was not morbid. The head was shaved and cold applied. The feet were fomented, and a smart purge of scammony and calomel given.

I obtained some of the urine of this evening; it was of dens. 1010, not in the least coagulable.

19th.—Dr Christison saw her along with me. She was still very much in the same excitable state, but occasionally saw quite well, and also appeared to hear. She mistook her relatives and attendants for one another, but named Dr Christison whom she had known by sight. She took readily some soup which was ordered for her. As the scammony had not acted freely, it was repeated with good effect.

20th.—The jerking and other hysterical symptoms have diminished. She sees pretty well, but complains that her vision is confused. She states that she hears none. She was ordered moderate animal food, to keep the head cool, and to take a teaspoonful of ammoniated tincture of valerian twice daily.

26th.—Has gradually improved since date of last report. The mental confusion and blindness are quite gone, but she hears none. The chief peculiarity about her is a wrong pronunciation of certain words; thus she calls a horse a *harse*, a sailor a *sawler*, &c. Of this she appears totally unconscious, and she continued for some time afterwards to make these mistakes, and was occasionally rather nettled at her relatives for smiling at them, as she could not be made to comprehend what was the source of their amusement. She has had no dyspeptic symptoms, is not thinner or weaker, generally sits in bed reading or knitting, but has once or twice been up to have her bed made. The evening urine of this day was of dens. 1019, and more coagulable than on any former occasion. It presented a considerable white fleecy sediment, consisting chiefly of epithelium, with a few crystals of uric acid.

March 7.—Has complained of slight flatulent distension after meals for two days, but is otherwise improved; has good appetite; vision is perfect, but deafness still complete. An occasional nervous "chuckle" is the only hysteric symptom. The urine of this date is dens. 1016, not coagulable, and depositing nothing but epithelium.

13th.—Continues as before. The face is now less puffy; bowels

are easily regulated by the iron and aloes pill; and hysterical symptoms are kept in check by occasional doses of valerian. The urine of this day is pale, in good quantity, dens. 1019, not albuminous, but depositing a trifling amount of phosphates when heated.

17th.—I was called this evening hurriedly to see her, from her having suddenly lost the power of speaking. I found her quite clear in intellect, with perfect vision, but hearing none. She could move all her limbs perfectly, but signified that she could not speak. She had complained of cold feet the day previous, had used a warm bath to the lower half of the body in the evening, after which she had felt faint. She had slept well, but had a very dull heavy look all the morning. The attack of this evening is ascribed to the exhaustion from last night's bath.

I ordered her immediately a teaspoonful of spirit. ammoniæ aromaticus, to be repeated during the night; she was enjoined to refrain from laxatives for two days, and was next morning to have a small jug of cold water poured over the back, and to return immediately thereafter to bed for a short while, but to get up in the afternoon.

The urine of this date was dens. 1016, as copiously albuminous as on 26th ultimo.

18th.—Is better to-day; has had the cold douche, and liked it, having been accustomed to it before her illness, and having had a comfortable reaction after it. She speaks in a whisper, but otherwise is as she has been for some time.

April, May.—From date of last report, and during these two months, she continued in a nearly stationary condition. She was generally able to go out for short drives or walks when the weather was fine, but frequently manifested great torpor and indisposition to make any exertion. She had occasional fits of hysteria, and took food irregularly, but, on the whole, rather well. She continued the cold bath with much delight in it. Once or twice she appeared as if she heard a word or a sound, and on two occasions said that she had heard; but this did not continue. No other treatment was followed than giving occasional doses of valerian, and regulating the bowels. During these two months I examined the urine at least once a week. Both morning and evening urine were sent to me. Generally it was slightly albuminous, but frequently it was entirely devoid of this character. I never was able to observe any distinct relation between the presence or absence of albumen in the urine and the other symptoms. Sometimes when the urine was most coagulable she was least languid and hysterical, sometimes the reverse was the case. The density of the urine was generally good, ranging from 1015 to 1025.

Nothing particular occurred during all this time, and I now hoped for a gradual restoration to health, anxiously looking for some catamenial demonstration. On 16th June, however, I was unexpectedly called to see her, and found her in a state of complete insensibility. The face was slightly flushed and very puffy, but there was no œdema

of the extremities. The pupils were rather contracted, but quite sensible to light. Pulse 80, and of fair strength. There was no paralysis of any of the limbs, but she could not be roused to speak. The hair having grown a little, the head was again shaved, and cold applied; and as the bowels had been torpid for some days, one-sixth of a drop of croton oil in sugar was placed on the tongue every four hours till the bowels acted freely.

She was seen by Dr Christison during this attack, which lasted till the 23d (seven days). Her condition was very remarkable, and more nearly resembled catalepsy than any thing else. She generally lay motionless as a statue, but occasionally lifted up her hands together as if in an attitude of prayer, and held them up for some minutes at a time; sometimes she seemed to point with one finger to heaven. She generally appeared sensible as regarded the evacuations; but her wants in this respect were for the most part anticipated by a very attentive nurse, who watched any manifestations of restlessness which generally preceded an evacuation. She never spoke during the whole of these seven days; but she swallowed perfectly, and took small quantities of strong soup, some doses of valerian, and a little wine when the pulse was weak.

Dr Christison saw her with me on the fifth day of the attack; the urine of that date was 1028, of good acid reaction, and not coagulable. On the seventh day, the 23d June, this comatose state went off rather suddenly. She woke up as if from a deep sleep, spoke collectedly, but in a dull manner, and had a very stupid, vacant expression. She could not be made to hear any sounds, but her vision was quite good. By next day she was nearly in her usual state, took her food well, and never suffered from any sickness or vomiting except once or twice from the croton oil, which had been continued in single doses as a laxative.

On the 25th, menstruation occurred, without pain, and rather profusely. It lasted altogether five days. On the second day of the menstruation (27th) she again fell into her former comatose state, which continued two days, and again passed off suddenly as it had done on the 23d.

From this time she remained in a very listless state, and almost constantly in a supine position in bed. It was with the greatest difficulty that she could be induced to get into an easy-chair for an hour or two. Her objection to this was, that she said she could not support her head or body. She certainly, when placed upright, allowed her head to hang in a way which cannot be better expressed than by the word dangling. She allowed her chin to rest upon the sternum, or if she was inclined to one side, let it roll over towards the shoulder, more as if it had been attached to a short rope than to a vertebral column. She expressed a complete repugnance at the cold douche, and always complained of being the worse for it on two or three occasions, when, with great labour to her attendants, I got it tried. She continued in this state for nearly a month, but

the muscular power of the trunk and neck gradually improved, and she got so far better as to be able to sit up for some hours in the chair, and to work a little with her knitting-needle. She menstruated again on 19th July, the discharge being free, and lasting four days.

29th July.—She again became comatose as before, with this exception, that she could be roused, and opened her eyes, but she would hardly speak, and refused to take food, saying that every thing had a nasty taste. This semi-comatose state, however, was attended by a new phenomenon in the form of a most profuse spontaneous salivation. The saliva did not flow from her mouth in a stream, but frothed up from between her lips, exactly like a beer barrel in very active fermentation. There was no fœtor of the breath, no appearance of swelling of any of the salivary glands, and I need hardly point out the fact, that it was in no way due to mercury, as she had not for months used a particle of that metal. The profuseness of the salivation may be judged of by the fact, that she completely saturated three or four thick towels every day. This state of matters lasted for five days. It did not terminate suddenly as in the former attacks of insensibility, but there was a gradual cessation of salivation and return of consciousness, and by the sixth day she was *in statu quo*.

August.—No menses this month. She keeps better, and has had no comatose nor hysterical attacks. Bowels easily regulated by the iron and aloes pill, which is her only medicine. She sits up for several hours daily.

September 3d.—Menstruated again, is cheerful, and takes food well. She complained at this time of a pain in the left axilla, and there is a swollen gland there which is tender, but without redness. This appears to be connected with an irritable state of the left mamma, which is rather larger than the other, somewhat hard, and exceedingly tender. Fomentations with lint dipped in hot water, saturated with laudanum, and covered with oil-skin, and subsequent protection by a thick fold of cotton wool, gradually removed these complaints.

10th.—Continues to improve. She has grown considerably, and though in no respect emaciated, is a little thinner than before. The axillary swelling is nearly gone. She was ordered to take a dessert-spoonful of cod-liver oil twice daily.

October 1st.—She takes the cod-liver oil readily; has improved in every respect, and keeps free from all hysterical affection. From this time, with the exception of one symptom to be noticed presently, her condition was that of steady progressive improvement. The hearing, which till now had continued in abeyance, returned, not suddenly, but irregularly and gradually, so that it is not easy to assign any precise date to its recurrence. It was for a long time, however, very imperfect,—one proof of which was, her occasionally sitting down to the piano and making hideous discords, of which she seemed to be quite unconscious.

One circumstance only remains to be mentioned. About the middle of October she complained of some uneasy feeling in mictu-

rition, which, however, was not considered of any importance, and I consequently did not see her till it had lasted two days. Thinking that it probably depended upon mere nervous irritability, I ordered only a hot fomentation and a dose of sweet spirit of nitre, but desired the urine of that day to be preserved and sent to me, that I might see whether or not it contained any sabulous matter. The urine sent was about twenty ounces in quantity, pale coloured, density 1020, of acid reaction, and not albuminous; but there was a deposit in the bottle, the appearance of which was new to me. It consisted of large brownish-yellow thin translucent scales, with sharp angles, and it appeared to me so unlike any thing usually deposited from urine, that at first sight I thought that it consisted of films of the bottle glass, as it resembled very closely, except in being darker in colour, the thin scales which are occasionally seen in bottles in which alkaline fluids have long been kept. I soon found, however, that this was not its nature, for on trying a portion of it with nitric acid, it dissolved, forming a clear yellow solution. I therefore collected it on a filter, washed it with a little cold water, and on subsequently examining its properties, found that it was xanthic oxide, associated with a considerable proportion of earthy phosphates. I was in hopes that I should obtain enough of this interesting substance to enable me to examine its properties; but in this I was disappointed. The whole quantity which this sample of urine yielded, did not, when it was dried, amount to a couple of grains, and of this I consumed a considerable proportion in determining its nature; and although I desired all the urine which my patient passed during the next twenty-four hours to be kept for me, I found in it only one or two flakes of the deposit not worth collecting, and on the day following it had totally disappeared, and only a few crystals of uric acid replaced it. Dr Golding Bird seems to have met with a case where the same thing happened (op. cit., p. 169). From the date of this occurrence I have had no occasion to prescribe for my patient. She continued to improve in strength during the winter months; all the functions were performed regularly; in spring she resumed her attendance on her classes, and is at this moment in the enjoyment of perfect health.

In the remarks which I have to append to this case, I shall almost entirely restrict myself to those which apply to the state of the urinary organs. I do not consider that the other symptoms—nervous deafness, functional amaurosis, nervous aphonia, and phenomena resembling catalepsy—are devoid of interest; but little can be said of them in relation to the present case, than that they were, from the course of the symptoms, believed to be dependent upon the state of the uterine system; that it was hoped that, when menstruation was established, they would give way; and that the result verified this prognosis. In the earlier periods of the case, it undoubtedly occurred to me, that there might be tubercular disease of the brain; but this

never amounted to more than a suspicion, and the unequivocally hysterical character of the other symptoms soon led to a decided belief, that these various affections of the senses were not dependent upon organic cerebral disease.

One other question, also, was not excluded from consideration—viz., Were these affections of hearing and sight real or feigned? On this point also I perfectly made up my mind. Not merely were many experiments made, both by myself and by the relatives of the patient, to test her insensibility to sounds, but these affections of the senses were accompanied by other phenomena—*e. g.*, the comatose attacks, the salivation, &c.—so totally unfeignable, and so fully betokening a powerful functional derangement of the nervous system, as to render it quite undoubted that the other symptoms were equally real.

The spontaneous salivation is a curious but not unknown effect of uterine derangement. Some well-marked cases in which this occurred have been quoted by Dr Laycock (On Hysteria, p. 34); but it seems to have been observed exclusively in pregnant women. It is evidently analogous to, and perhaps here was vicarious of, the well-known copious flow of limpid urine so common in hysteria,—an occurrence which took place very seldom, and never to any great extent, in my patient.

The condition of the urine was throughout the whole of this case a matter of much interest to me. At first, I was led by it to form a rather unfavourable prognosis. The fact of the urine being in a marked degree albuminous and of low density (1009 and 1010, on the first two occasions on which it was examined), and this occurring in a strumous habit, justified such an opinion. The circumstance, however, of the urine being, at a subsequent examination, found to be improved in density, and not coagulable at all, gave a favourable aspect to this part of the case; and repeated examinations afterwards having shown that the urine was quite variable in this respect, being sometimes albuminous and sometimes not so, led me to the conclusion, that this symptom was here comparatively unimportant; at least, that it was of a purely functional character.

The term functional, as applied to the appearance of albumen in the urine, is used by writers in two senses. By some it seems to be used merely in contra-distinction to organic degeneration of the kidney, and to mean an inflammatory or irritated state of this organ. By others, again, the term functional is used in its strict sense, as expressing simple derangement of the secretory action of the organ, irrespective of inflammation or organic disease; and those who use the term in this sense generally do so with the intention of disputing the fact of its ever occurring. Dr Prout fully expresses the views of those who hold these opinions, in the following sentence:—

"Whether such a state of simple suspension of the function of the kidneys ever takes place alone, without involving other parts of the system, I am unable

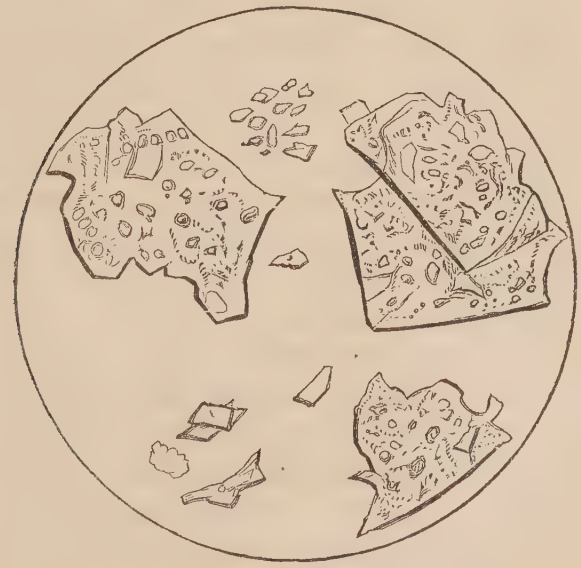
to decide; but my belief at present is, that it does not, and that in all such instances, as, for example, that of chylous urine just alluded to, not only a condition of the kidneys exists, to which, for want of a better name, I must apply the epithet *inflammatory*, but that other parts of the system, especially those connected with the assimilative functions, are likewise more or less involved in a similar condition; and that when this supposed inflammatory condition of these organs subsides, the kidneys resume more or less perfectly their natural disorganising function, and the albuminous matter, as a consequence, disappears from the urine. This peculiar condition of the system, and temporary suspension of the nephritic function, may be apparently produced by various causes, among which may be mentioned the effects of certain drugs, as mercury, cantharides, &c. A similar condition of the system seems also to be occasionally produced by, or rather to follow, attacks of fever; also by a state of pregnancy; by certain indigestible articles of food; by violent mental emotions, &c. Under the operation of all which, as well as of many other similar causes, the urine has been found to be temporarily albuminous. There is a point, however, of great importance connected with this inquiry to which I particularly wish to call the attention of the reader—viz., that the causes mentioned do not *invariably* produce serous urine in all individuals; the inference, therefore, must be, that in the persons liable to be so affected there exists a sort of latent predisposition (incipient degeneration?) to kidney disorders; otherwise every individual ought to be similarly affected by the operation of the same causes, which, as just stated, is not, according to my observation, the case. From what has been stated, then, the answer to the query above proposed will be,—that in some individuals the urine is liable to become albuminous, from certain derangements of the system in general, and of the kidney in particular, which cannot at present be otherwise defined than as *functional* derangement, though it is not improbable that such assumed functional derangements may partake of the character of incipient disease of the kidneys.—*On Stomach and Renal Diseases*, p. 120.

Now, in relation to the present case, I am not disposed to adopt either of these theories of the distinguished author whom I have quoted,—either that this was dependent upon a state meriting the title inflammatory, or that it partook of the character of incipient disease of the kidneys. I do not see in the history either of the renal or general symptoms any evidence of the existence of inflammation. There never was seen, in all the samples of urine which I examined, any fibrinous casts, or other decided products of inflammatory action. The epithelium, which was present, was never unusual in quantity, nor of a broken-up or injured appearance. It was exactly such as may be seen any day in urine, perfectly free from albumen. Nor in the general symptoms was there anything betokening inflammatory tendencies,—everything went rather the other way; and, certainly, neither in the uterus, nor in the organs of assimilation, was there anything to show that they were involved in inflammatory action. If we adopt Prout's other supposition, that this was a case of incipient kidney disease, or a proclivity thereto, we must, with the pathology, adopt a very comforting feeling, that sometimes, at all events, incipient kidney disease may be a comparatively unimportant matter as regards therapeutical indications. For assuredly, in this case, there was no special treatment applied to the kidneys at all. No cupping, leeching, diaphoretics, counter-irritation to the loins, or any of the means usually had recourse to in such cases. For my own part, I cannot hold

any other opinion than that the albuminuria here was functional in the strictest sense of the word; that it was dependent upon the innervation of the kidney being deranged in common with that of the whole of the rest of the system; that, in short, it was as purely functional as the excessive discharge of limpid urine so common in most cases of hysteria, or as the spontaneous salivation which actually occurred in this patient. I am disposed to believe that the albuminous urine of pregnant women, which has been explained upon the supposition of its being caused by pressure on the emulgent veins (v. Golding Bird, p. 327), is sometimes of a like functional character, though there can be no doubt, from the observations of Simpson, Christison, and others, that on the other hand, it frequently betokens mischief of the most serious character.

The xanthic oxide which afterwards showed itself in the urine did not occur till the patient had ceased to be hysterical, and perhaps had little connection with the general nervous symptoms; but what I have to say regarding it may as well be stated now.

The following are the characters which the deposit presented:— As seen floating about in the urine when agitated, it appeared as large brownish-yellow thin translucent scales with sharp angles. It was obviously of considerable density, as it sank rapidly to the bottom when allowed to rest. When collected in a filter, washed, and dried by gentle pressure and exposure to the air, it appeared as a small mass of deep honey yellow colour and waxy lustre, easily splitting up into thin translucent scales. Under the microscope, it presented the appearance represented in the accompanying woodcut.



The sharp angled irregular fragments of which it consisted were about $\frac{1}{500}$th of an inch in diameter. There was no trace of crystalline form; when triturated with water, by moving about the thin glass under which it was placed for microscopic examination, it

merely broke down into smaller angular fragments, and subsequently into minute amorphous particles.

The trifling quantity at my command did not permit of my making many experiments with it, but I was able to determine the following chemical characters. When heated in a tube in an oil bath it underwent no change, except a little darkening in colour, at a temperature below 480° Fah., when it began to evolve a peculiar feeble odour somewhat like that of burned horn; it did not fuse, and it gave off no condensible products though heated to 550°. When it was exposed to the flame of the spirit-lamp it gradually burned away, gave off ammonia, and left a perfectly white ash of the exact figure of the fragment used, which ash consisted of earthy phosphates.

It was insoluble in cold water, in alcohol, and ether, but dissolved readily both in water and in alcohol, when hydrochloric acid was added, forming a pale yellow fluid, which when evaporated in a watch-glass left a white residuum, without any crystalline aspect. Strong sulphuric acid dissolved it, but a portion of white matter remained undissolved. When water was added to the solution in sulphuric acid it did not cause any perceptible turbidity. Nitric acid dissolved it readily without effervescence, the solution when evaporated left a bright yellow residue totally devoid of any trace of crystalline appearance; and when ammonia was added the colour became an intense deep lemon yellow. There was not the least trace of pink in the evaporated nitric solution, nor was there the slightest indication of purple from the action of ammonia, so that its diagnosis from uric acid was complete, and it thus presented the characters of xanthic or uric oxide. In one respect only did I fail in developing the re-actions of the substance as given by its discoverer Dr Marcet. He states that if caustic potash be added to the yellow residue of the nitric solution "it instantly turns to a more or less intense red colour, and upon evaporation it assumes a brilliant crimson hue." I do not know whether this is a constant character of uric oxide or not, as I have never had an opportunity of working with an undoubted specimen of that substance; but I do not find this re-action, which would be very characteristic, noticed either by Simon, Golding Bird, or any of our recent writers on urinary deposits, as having been observed by them. The effect of caustic potash on the substance examined by me was merely to give it a dirty brown tint.

Notwithstanding this, I believe this substance to be nothing else than uric oxide deposited upon a basis of phosphates; and it is worthy of remark that in the calculus originally examined by Dr Marcet there was a considerable amount of ammoniaco-magnesian phosphate. I note this circumstance more particularly, because, from some observations to be mentioned presently, I am inclined to believe that this substance has a strong tendency to deposit itself along with phosphates.

At the time when I subjected this deposit to the above trials, I

happened to have under my care two well marked cases of hysteria, one in a strumous looking girl who had gone through an attack of pelvic cellulitis, the other in a married lady who had recently undergone great mental anxiety and depression, and whom I had formerly attended under symptoms of incipient phthisis. As both of these cases were a good deal analogous to that just detailed, I examined the urine in both with a view to trying whether this form of disease (hysteria in a strumous habit) had any connection with the existence of xanthic oxide in the urine. From both I obtained very distinct traces of a substance which with nitric acid gave a yellow residue rendered brighter in colour by ammonia. Subsequently, however, on extending my investigations to other cases of disease, and finally to healthy urine, I found that a small proportion of a substance having these characters was a constant and apparently normal constituent of the urine. It is well known that there exists in the urine a yellow colouring matter which has been described as *Uroxanthin* by Heller, but which had previously been designated by Dr Golding Bird *Purpurine*, from the very characteristic re-action, first pointed out by him, of hydrochloric acid upon it. Dr Prout had suggested that there were probably two yellow colouring matters in urine, and Berzelius had described one under the name of *Halophyle*, "from the remarkable obstinacy with which it adheres to the urinary salts." [Quoted by Golding Bird op. cit.] It was of importance to me to identify with, or distinguish from, these various substances, the matter found in the above narrated case and that which I had ascertained to exist normally in urine. It appears to me most probable that it was the observing traces of this substance which led Dr Prout to suppose that there were two yellow colouring matters in the urine; and he also hints at a possible connection between the yellow colouring matter and the uric oxide (op. cit., 562). I cannot find the passage quoted by Dr Bird from Berzelius in which he mentions the substance which he calls halophyle, and therefore cannot say whether it is to the ammoniacal or the earthy salts of the urine that he supposes it to adhere with such obstinacy; but if it is to the latter, it is most probable that his halophyle is identical with that which I have observed as a constant constituent of urine. It is evident that neither the deposit from my hysteric patient's urine, nor that which I have found in other cases, has anything to do with the purpurine of Golding Bird; for, on repeated trials with both, I found that neither of them, when boiled with hyrochloric acid, have the least tendency to assume a pink or purple colour; and the re-action of nitric acid on uroxanthin (Heller), the same substance as Golding Bird's purpurine, is to strike a violet colour, which is not the case with the substance now under consideration. (Simon's Chemistry, ii. 523.)

In the second edition of Dr Bird's valuable book on urinary deposits, I find the following sentence:—"Very recently two German chemists, MM. Strahl and Lieberkühn, have announced the exist-

ence of traces of uric oxide in human urine. But if the body they have described is to be regarded as a normal ingredient of urine, it, as Lehmann has suggested, is more likely to be identical with guanine,[1] on account of its solubility in hydrochloric acid."

I cannot understand this remark of Lehmann; for Dr Marcet states distinctly, that xanthic oxide is soluble in hydrochloric acid, and that this solution when evaporated gives a white residue. I see no reason, therefore, why the substance announced by Strahl and Lieberkühn should be regarded as anything else than Marcet's xanthic oxide. I have not seen their paper, which is quoted by Dr Bird as a separate publication, and not from any periodical; and I, therefore, do not know by what means they attained to this conclusion; but finding the results of my own experiments to correspond with theirs, I can have little doubt that we have been examining the same substance.

The method which I have followed for detecting this supposed xanthic oxide in urine is the following:—The urine, gently warmed, is mixed with about $\frac{1}{8}$th of its bulk of hydrochloric acid, allowed to rest for 24 hours or more, and filtered to separate uric acid. The fluid is then evaporated nearly to dryness, during which it becomes dark-coloured, generally more or less purple from the presence of purpurine. The residue, which is strongly acid, is then taken up with rectified spirit, and neutralised with ammonia. A fawn-coloured precipitate separates, which consists of the uric oxide, associated with phosphates, which, when washed with water, gives very readily the characteristic yellow re-action, when treated with nitric acid and ammonia.

Other occupations have prevented me from extending these experiments, but I am induced, by one trial which I have made, to think that, by acting on this precipitate with potash, and transmitting carbonic acid, the uric oxide will be obtained free from inorganic matter.

It may be demonstrated, also, without employing spirit, by adding hydrochloric acid to the urine, separating uric acid, neutralizing by carbonate of potash, filtering to separate some phosphates, and silica from the potash, which are deposited, and then heating the liquor to expel carbonic acid. A fawn-coloured deposit of phosphates is obtained, which gives very characteristically the yellow reaction with nitric acid and ammonia. But the advantage of using spirit is, that

---

[1] Guanine, discovered by Unger in Guano, supposed by him to be uric oxide, but subsequently stated by Einbrodt and Unger to be a distinct substance, and now designated by the name which it bears. According to him, xanthic oxide is $C_5\ H_4\ N_4\ O_2$, whilst guanine is $C_5\ H_5\ N_5\ O$. I find, however, that the re-actions of guanine with nitric acid and ammonia are identical with those ascribed to true xanthic oxide; their properties otherwise are marvellously similar, and I desiderate some further analyses and examinations of authentic xanthic oxide calculi before admitting that these two substances are distinct.

a quantity of inorganic salts is thus separated, which, by their presence, render the reaction with nitric acid less distinct; and as the purpurine remains entirely in solution in the alcohol, whether the fluid be acid or alkaline, this source of impurity in the precipitate is likewise more completely avoided. On one occasion, when I left the alcoholic fluid, filtered from the ammonia precipitate, to stand for several days, a gray slimy deposit had separated, which appeared to contain no inorganic matter at all, and to consist entirely of the substance which gives the yellow reaction with nitric acid and ammonia, and which I have supposed to be uric oxide.

From what I have observed in my experiments, I think myself warranted in drawing the following conclusions:—

1. That the deposit in the case above narrated was of uric oxide, associated with phosphates.

2. That urine contains two yellow (?) matters. One giving the purple reaction when heated with hydrochloric acid (purpurine, Golding Bird—uroxanthin, Heller); the other giving the yellow reaction with nitric acid, followed by ammonia.

3. That the latter has been hinted at by Prout as existing normally in urine; is probably the substance called halophyle by Berzelius; is the uric oxide found by Strahl and Lieberkühn, and is perhaps identical with guanine.

4. That this uric oxide has a strong affinity for the earthy salts of the urine, as the purpurine of Golding Bird has for the ammoniacal urinary salts; and it has, I suspect, generally in analyses of urine, been neglected as mere colouring matter adhering to the phosphates.

5. From observations which I have not thought it necessary to detail, it appears to be most abundant in clear pale urine, especially that from nervous subjects, not depositing uric acid or lithates, and not containing much purpurine.

I shall not attempt to speculate on the probable source of this substance until these observations have been subjected to a more rigid scrutiny. It appears to be most likely that some decided physiological relation will be established between this constituent of urine and the substance called hypoxanthin found by Scherer in the spleen, which he states to differ from uric oxide, as the latter does from uric acid, only by containing one equivalent less of oxygen.

66, Frederick Street, Edinburgh,
July 16, 1851.

ARTICLE V.—*Case of Hemorrhœa Petechialis.* By DR IMLACH, Liverpool.

JANE ———, aged 21, came from the Isle of Man about the 20th of Feb. 1851 to take a situation as nurserymaid at Edgehill. She was then in apparent good health. On the third March, hav-

ing a headach, she took an aperient pill, and some castor oil on the following morning; but she made no complaint of being ill, and was in good spirits as usual. On Thursday 6th March the tongue was foul, and she had some headach, which was better in the evening. On Friday (7th) she had some pain in the back, and she said that the catamenia had appeared that day in very moderate quantity, but about a week before the expected time. She was obliged to go to bed, as the pain in her back became severe in the course of the day, and in the evening she passed a good deal of blood, which was still regarded by her as menstrual.

On Saturday morning she was considerably worse. She passed blood apparently along with the urine. She spat a little blood, though only enough to tinge the sputa; petechiæ, small but numerous, were seen on her chest; and she soon became very prostrate. When seen at noon she was sinking fast, and she died soon after two o'clock, having continued to the last moment in the full possession of all her mental faculties.

When the body was removed from the bed, it was found that there must have been a considerable discharge of blood, but the quantity of it could not be estimated; and the only additional information communicated by her relatives was, that the girl had always enjoyed good health, but that she was the fifth member of of her family who had died of "ruptured blood-vessel," or hæmorrhage.

A *post-mortem* examination was made twenty-five hours after death. The chest and limbs anteriorly had numerous petechiæ on them; the back and dependent parts of the body were much discoloured. On opening the body, purpurous spots were seen everywhere, large ones on both lungs, and numerous smaller spots were found on the costal pleura of both sides, as well as in some old adhesions in the left side of the chest. There was more than half-a pint of reddish serum in each side of the thorax, and the lungs, when cut into, were seen to be somewhat congested, but not diseased. The heart was healthy, but there were a few small spots to be seen on its surface and on the reflected pericardium.

In the abdomen the peritoneum was everywhere spotted, especially in the mesentery and mesocolon. The stomach and small intestines looked red, and, on being opened, were found deeply congested in their mucous coats. The colour was most intense about the small curvature of the stomach and in the duodenum; and there was a considerable quantity of tough mucus adherent to the great sac of the stomach. The jejunum and ileum were also deep in colour, but not so intensely as the duodenum, except in occasional patches which were situated towards the mesenteric side of the gut. There was no fluid blood in the intestines. The large intestine was pale and shrunk in size.

On the surface of the liver, both at its anterior edge and particularly at the lobulus quadratus and the posterior parts, the dark

purple discoloration was most remarkable, but the colour did not penetrate the substance of the liver to any great depth.

The cellular sheath of the vena cava was full of exuded blood (?) and this extended along the right renal vein.

Both kidneys were much congested; their pelves and infundibula were quite full of coagulated blood, very black in colour. There was a little dark fluid, probably grumous blood, in the bladder.

The uterus contained some menstrual (?) fluid, and in the left ovary was a corpus luteum, evidently just formed. There was the usual small irregular projection on the peritoneal surface, and when this part of the ovary was cut into, a recent clot filled the ruptured cyst. The right ovary was stained, just as the liver was, with purpurous ecchymosis. The hymen was perfect.

The spleen was of a dark colour, and showed no unusual appearance. It was of its usual size. The inner surfaces of the aorta and of the vena cava were white and healthy.

The head and spine were not examined.

ARTICLE VI.—*Case of Aneurism of the Aorta, arising from the back part of the Arch simulating Laryngeal Disease, and fatal by Suffocation; with Remarks.* By W. T. GAIRDNER, M.D., F.R.C.P., Pathologist and Assistant-Physician to the Royal Infirmary of Edinburgh.

(*Read to the Medico-Chirurgical Society of Edinburgh, 18th June* 1851.)

THE following case of aneurism of the aorta is interesting, 1st, from the absence of physical signs, and the prominence of the symptoms of a laryngeal affection; 2d, from the free communication of the sac with a mucous canal, without causing serious hemorrhage; 3d, from the termination of the disease by suffocation, and the remedial measures suggested by this termination. On these grounds I beg to lay it before the Medico-Chirurgical Society, along with a preparation showing the parts involved in the disease.

Thomas O'Brien, æt. 46, a robust labourer, was admitted into the hospital at the hour of visit on the 30th May 1851. He complained of great dyspnœa, which, in the recumbent posture, was so extreme as to threaten suffocation. The breathing was sonorous, with distinct stridulous character on inspiration. The countenance anxious and flushed; no fever or pain complained of. Expectoration considerable; the chest was examined as well as his state permitted, and revealed only slight bronchitic râles, the harsh laryngeal breathing being heard over all the bronchi. The voice was evidently produced with effort, scarcely husky, but having a somewhat muffled character; there was no tenderness over the larynx; the epiglottis and throat were natural. Shortly after his removal to a ward, the

paroxysm subsided to some extent. I then learned that this was only an accidental exacerbation of a state which had existed for some months, and for which he had undergone active treatment. A blister was applied to the nape of the neck; and he was ordered ipecacuan wine ʒss every second hour.

On the 31st he had slept well; but the dyspnœa had returned in the morning; at visit he was better, but not able to lie down. The chest was examined with more care; no abnormal percussion at any part; the respiratory murmur every where abundantly audible, and natural; some coarse mucous râle in both backs, and a few dry bronchial râles elsewhere; the sounds over the region of the heart and of the great vessels strictly normal. The pulse was hurried, but natural in character.

He continued in much the same state till June 4th; paroxysms of extremely difficult breathing occurring three or four times a-day, and lasting generally from twenty to thirty minutes. He expectorated daily from six to eight ounces of frothy mucus, tinged of a distinct rusty colour, with streaks of purple. Repeated examination of the lungs, however, showed that they admitted air abundantly in every part, and were free from every physical sign of disease. The treatment was not altered; the operation of tracheotomy was proposed, and, though urged as a certain means of relief from the paroxysms, was objected to in the strongest terms by the patient, who said he would rather die than submit to it.

On the morning of the 4th June, between 7 and 8 A.M., he had an attack of laryngeal suffocation of peculiar intensity, accompanied by dull pain in the lower part of the chest. He was seen by the resident clerk, and again refused the operation; he seemed to obtain relief by being supported in the erect position, and walking up and down the ward, friction being also applied to the front of the chest. Another paroxysm, not so severe, occurred in the afternoon. At half-past 7 P.M. there was a return of the paroxysm. He was seen at 8 o'clock, when he was livid and exhausted; he expressed a desire to be bled, and again refused to permit tracheotomy. At half-past 8 the dyspnœa was intense, the lividity of lips very great; the face generally pale; the skin covered with cold sweat. The operation was performed by the resident surgical clerk in attendance, but his eyes became fixed before the tube was introduced. He continued after the operation to breathe slowly and at long intervals; the pulse continued perceptible for about ten minutes, but he did not rally, and died about a quarter of an hour after the tube was introduced. Artificial respiration was employed without effect. A small quantity of blood was lost during the operation, some of which entered the trachea, and was apparently expelled again with considerable force.

Post-mortem examination, 6th July.—The body unusually robust; post-mortem lividity considerable; rigor mortis well marked. No emaciation either of fat or muscle.

Pleuræ containing little fluid; adhesions at apex of right lung,

corresponding to a few encysted cretaceous concretions, little larger than a barley-corn. A little emphysema in the anterior parts of both lungs, and slight collapse of the tissue posteriorly, otherwise they were healthy. The greatest bronchi had the mucous membrane slightly congested, and contained a considerable quantity of tough mucus and muco-purulent matter rather deeply tinged with blood; but nowhere any distinct coagula.

The heart weighed 12½ oz.; its muscular tissue much congested. On the aortic valves, which were quite competent, and not at all deformed, there were one or two very minute granulations, and a few similar ones on the inner membrane of the vessel near its origin. The other valves perfectly normal.

The thoracic aorta had its inner membrane throughout uneven and thickened, but with little distinct abnormal deposit. The arch presented no general dilatation; it was, however, slightly dilated upwards at the root of the innominata; and this vessel, as well as the origin of the right subclavian, was uniformly large relatively to the vessels on the opposite side. The two carotids were of equal size; but both of them, as well as the left subclavian, were very slightly expanded at their origin.

At the back part of the arch, half an inch below and between the origin of the innominata and left carotid, was an oval opening, through which a hazel nut might be passed lengthways. Its edges were tolerably smooth and rounded; and it was three-quarters occluded by a mass of firm granular coagulum, which passed from this opening into the aneurismal sac beyond. This was of the size of a walnut, and was situated between the aorta and the trachea, being adherent to the perichondrium of some of the tracheal rings; the sac was nearly full of laminated, decolorized coagula, with a little fluid blood.

The left recurrent nerve, emerging from below the aorta, passed immediately to the left of the sac, and rather behind it, being bent over it, and at one point almost imbedded in the thickened cellular tissue which surrounded it; at this point there were also one or two indurated lymphatic glands around the nerve, dark from carbonaceous deposit. The pneumogastric nerve on both sides, and the recurrent on the right, had their normal relations, excepting that the subclavian artery, where it was surrounded by the right recurrent, was, as before mentioned, somewhat dilated.

The tongue rather brown, and dry in front. Its root, and the fauces natural.

The epiglottis normal in size and form; its mucous membrane faintly rose-coloured on the posterior aspect, and displaying a somewhat granular surface, from prominence of the mucous follicles, especially in the neighbourhood of the arytænoid cartilages. Ventricles of larynx and vocal cords natural.

The cricoid cartilage and three upper tracheal rings divided by a perpendicular incision in the middle line.

The mucous membrane in the larynx and upper fourth of the trachea nearly natural in colour and appearance. Below this the mucous membrane presented rose-coloured vascularity, deepening towards the bifurcation, on the left side, into purple. The mucous membrane slightly granular throughout this injected part from hypertrophy of the follicles.

About an inch and a quarter above the bifurcation on the left side there was a circular opening, admitting readily a crow-quill, and passing into the aneurismal sac before mentioned, which lay in contact with the outside of the costal cartilages.

Nearer the bifurcation there were three or four small points slightly elevated, and of an opaque yellowish colour, as if the mucous membrane were stretched over some abnormal deposit. The cartilages of the two tracheal rings immediately behind the opening were entirely separated from their perichondrium at the part opposite the aneurismal sac.

The abdominal viscera were congested as usual in asphyxiated persons, but had no other morbid appearance. The abdominal aorta was not so uneven internally as the thoracic, but presented more distinctly atheromatous opaque deposit in its inner membrane.

The first question which suggests itself in connection with this case is, *What was the cause of death?* On this point, I think, a consideration of the whole circumstances will leave no doubt that the patient died chiefly from laryngeal suffocation, induced by pressure of the sac on the recurrent nerve of the left side. The occurrence of suffocation from this cause is too well attested by numerous cases of aneurism and tumours of the chest now on record, to admit of reasonable doubt. The evidence adduced by Dr Hugh Ley upon this subject in his work on laryngismus stridulus, although certainly insufficient to establish his exclusive theory of that disease, is strongly confirmatory of the correctness of the views entertained nearly two centuries ago by Willis as to this source of death in some intra-thoracic tumours. The experiments of Legallois, and the far more elaborate and satisfactory ones of Dr John Reid, have demonstrated, in the most unquestionable manner, the production of laryngeal suffocation by various kinds of interference with the recurrent nerve on one or both sides of the neck. "From the experiments we have detailed," says Dr Reid, "it is apparent that severe dyspnœa, amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or the trunks of the pneumogastrics. For when both, or *even one recurrent nerve*, was irritated, the arytenoid cartilages were approximated, so as in some cases to shut completely the superior aperture of the glottis."[1] Section of the vagi, also, according to Dr J. Reid, produced "sudden and

[1] Physiological, Anatomical, and Pathological Researches, p. 120. See also pp. 167 and 272.

violent attacks of dyspnœa, which generally went off in the course of a very few minutes, when they did not terminate in suffocation;" leaving, however, the animals liable to renewed paroxysms on the occasion of a violent struggle, or any exertion tending to hurry the respiration. It is unnecessary to enter into the physiological details and principles connected with these curious results; it is sufficient for the present purpose to observe, that they fully explain the numerous cases recorded in pathological and practical works from the time of Bonetus, in which tumours involving these nerves (in the great majority of cases aneurismal) have been shown to produce death by sudden orthopnœa, often independently of any pressure directly on the air-tubes. Indeed it is worthy of remark, that spasmodic dyspnœa is a cause of death in a very considerable proportion of cases of aneurism of the aorta.[1] It is sufficient to refer, in illustration of this point, to the cases by Drs Graham and Alison, communicated to the Edinburgh Medico-Chirurgical Society in 1835,[2] in which aneurisms of the aorta were accompanied by marked laryngeal dyspnœa from this cause, in Dr Graham's case altogether simulating a primary laryngeal affection; to a similar case under the care of Dr Todd,[3] in which the recurrent nerve of the left side, and all the muscles to which it was distributed, had undergone atrophy from the pressure of the tumour; to the case of aneurism of the innominata, detailed by Mr Lawrence,[4] in which death took place from suffocation, tracheotomy being proposed but not performed; and to several examples of this form of dyspnœa detailed in a paper by Dr Henderson,[5] and in the work of Dr Ley before referred to.[6] I shall only say farther, that the violent paroxysms of dyspnœa experienced by my patient on many occasions before they were actually fatal, the highly stridulous respiration, the difficulty he evidently had in producing vocal sounds, and their altered character, pointed unquestionably to the glottis as the source of his danger; and the absence of any physical signs or morbid appearances indicating serious pressure of the aneurism on the trachea render it not probable that death can be ascribed to this cause. There remains only one other possible cause of suffocation, and to this I am willing to allow its due influence. The exhaustion consequent upon the numerous attacks of dyspnœa during the last days of life evidently told severely upon his strength; and the considerable and increasing quantity of mucus in

---

[1] It may even be said that this symptom is rarely absent in those aneurisms which spring from the back part of the arch of the aorta. See Dr Greene's collection of cases of this kind in the Dublin Quarterly Journal, No. 3, new series; and Mr Crisp's table of aneurisms, Treatise on the Blood-vessels, p, 235; for numerous instances bearing on this point.

[2] Edin. Med. and Surg. Journal, vol. 43, p. 292, *et seq.*

[3] Lancet, June 1841, p. 400.

[4] Medico-Chirurgical Transactions, vol. vi.

[5] Monthly Journal of Med. Science, 1841, p. 10.

[6] On Laryngismus Stridulus, &c., p. 453, *et seq.*

the bronchi must have been expectorated far less freely in the last hours of his existence. The accumulation of this mucus, which was found in the larger tubes after death, fully explains why the operation of tracheotomy performed *in articulo mortis*, was followed by so imperfect a result. It is worthy, however, of remark, that up to the last visit which I paid him, the evidence of obstruction in the bronchi continued to be not greater than in cases of very slight bronchitis, and fully warranted the idea, that the greater part of the mucus expectorated came from the upper part of the air-passages. This idea corresponded also with the morbid appearances in the dead body.

The muco-purulent matter found in the larger bronchi after death was considerably more tinged with blood than that expectorated at any period during his fatal illness. But it is very doubtful whether this increased hemorrhage was from the sac; as I am told that a good deal of blood was drawn into the trachea during the operation. At all events, it is clear that hemorrhage was not connected with the fatal event, nor did it ever form a serious complication,—never amounting to more than was sufficient to give a purplish, and often only a rusty tinge to the expectoration.

With this absence of material hemorrhage, it is important to ask, *how long the communication of the sac with the trachea had existed before death?* The lungs having presented no symptom of disease, and being found after death free of all serious lesion, it is difficult to suppose that even the small quantity of blood in the sputa had any other source than the aneurism; and yet we have evidence that, if this be so, the opening must have continued for months, yielding only these small quantities, as the patient distinctly stated that he had at no time coughed up clots of blood. When we consider the nearly complete occlusion of the sac by coagula, this phenomenon will appear less difficult to understand; at all events, it is far from rare in the histories of aneurisms opening on mucous surfaces, and especially into the air-passages, to find, even after one serious hemorrhage has given evidence of a rupture of the sac, that the hemorrhage reduces itself to a very trifling amount, and sometimes is altogether suspended. The case of Mr Liston at once suggests itself as an illustration of this fact. In the only record published of the fatal illness of this distinguished member of our profession, it appears that the first hemorrhage was followed by a period of exemption from symptoms, and that when these recurred, it was in the form of a cough attended with expectoration, which was "difficult, small in quantity, and of a rusty colour;" no further material hemorrhage occurring till his death, which was from orthopnœa.[1] In the remarkable case of abdominal aneurism which I read to the Society last year,[2] it is more difficult, owing to the situation of the first opening into the duo-

[1] Lancet, December 11th, 1847.

[2] Aneurism of the Superior Mesenteric artery opening into the duodenum, twenty-two months before death.—*Monthly Journal*, vol. x. 1850, p. 83.

denum, to judge of the amount of blood that may at different times have been ejected, and it is highly probable that some bleedings may have taken place unobserved; but it seems in every way probable that no considerable hemorrhage occurred during twenty-two months, from an aperture which had evacuated gallons of blood in a few days, and which was found very nearly, if not entirely, sealed up after death.

Could the aneurism have been discovered during life? On this point we have the following data;—no dull percussion, abnormal pulsation, or tremor at the upper sternum; no abnormal sound over the heart and great vessels in front or behind; normal and symmetrical percussion over the lungs in every part; no abnormal respiratory sound over the trachea in front or at the root of the lung behind; abundant and symmetrical respiratory murmur in both lungs and over every part of them, mixed with slight mucous râles behind, and a very little sonorous râle in front. These physical signs, in regard to which full and careful examination may be relied on, form the elements of a tolerably complete negative diagnosis of aneurism, the suspicion of which was certainly entertained at the second examination of the chest, but soon dismissed, the case being treated as one of laryngeal affection. Perhaps it is still doubtful if more than a bare guess could have been formed under the circumstances; but the event showed that the suspicion thus negatived by physical diagnosis was allowed too hastily to be driven from the mind by the apparently greater probability of an ulcerative lesion of the larynx and trachea; and during the short period he was under my care no time was given for considering the case in all possible points of view, especially as the indications of practice appeared sufficiently distinct. After a careful consideration of the diseased parts, however, I am led to think it probable that while no sure sign of aneurism of the aorta could have been made out, a very close and accurate examination at the root of the neck, and in the course of the arteries, *might possibly* have discovered the dilatation of the innominata and subclavian vessels. Circumstances which did not come to my knowledge till after the patient's death were also calculated to arouse suspicion. The long persistence even of a trifling amount of blood in the expectoration would certainly have justified the belief in something more than an ordinary ulceration in the larynx; and a pain which he is said to have suffered at one time at the upper part of the sternum, but which was not complained of during the last part of his illness, would have confirmed the diagnosis of some fault in the thoracic region. The absence of any marked tenderness on pressure over the larynx, and of swelling of the epiglottis, was calculated to attract, and did attract, attention from the first; but this negative circumstance was considered as outweighed by the rest of the evidence.

It is worth while to remark, although it is difficult to obtain any accurate data on the subject, that the combination of symptoms pre-

sented by this case may probably be expected not unfrequently to occur in chronic or acute ulceration of the laryngeal mucous membrane. Local pain and tenderness are by no means of constant occurrence in these cases; neither can alterations of the epiglottis and upper vocal cords be recognized in all cases, though some kind of local symptom will doubtless be accessible in the great majority. On the other hand, the presence of blood in the sputa, though of course a suspicious circumstance when long continued, is neither universally present in aneurism, nor always absent in laryngeal ulceration. I have lately seen a case almost precisely parallel to this one in every important feature, in which paroxysmal laryngeal dypsnœa, apparently very little under the influence of remedies, and accompanied for a considerable period by blood in the expectoration, is probably due to a primary laryngeal affection, of which the local symptoms have lately become more distinct, while physical signs of aneurism remain, after repeated and careful examination, undiscoverable. If this man remains under observation for a sufficient length of time, it is probable that a more secure diagnosis may be formed; but at first it would have been impossible to act on an assured conviction either of thoracic or laryngeal disease, while the state of the patient has been, and continues such, as may render a recourse to tracheotomy an extremely necessary expedient for his security, or even his rescue from impending death.

In reference to diagnosis, the practical conclusions which follow from the above remarks are no less evident than important. The mistake of an intra-thoracic tumour for a laryngeal affection is one of those accidents which has probably occurred in practice far more frequently than it has been accurately recorded; although a sufficient number of instances have been published to show that it may readily occur in the most careful hands, in the absence of stethoscopic examination. It cannot, therefore, be too strongly insisted on, that a physical examination of the chest should take place in all cases of supposed laryngeal disease. This is indeed an invariable rule with all careful practitioners, on many grounds; although it may be doubted whether the lungs and air-passages do not often too exclusively absorb attention in such examinations. But the present case, while it proves still more strongly that no amount of caution in the examination of the chest, and especially of the great vessels, is superfluous, also shows, I think, conclusively, that the absence of the physical signs of aneurism or tumour should not suffice to remove completely the suspicion that they may be concerned in the affection of the larynx. It is obvious that the part of the aorta most apt to be affected in these cases is the middle or transverse portion of the arch, and particularly its posterior or inferior surface, where it is most removed from the possibility of physical diagnosis. It is also evident that a very small tumour in these situations is enough to give rise to all the symptoms of laryngeal obstruction. Dr Todd correctly remarked, in his clinical lecture upon the case above alluded to as

having occurred under his care, that "most observers had attributed these symptoms (those of chronic laryngeal affection) to compression of the trachea and bronchi, and had overlooked the condition of the recurrent nerve." In the present case, as in that of Dr Todd, "there was the most ample evidence that the pressure upon that nerve occasioned the laryngeal distress."

With regard to the treatment of such cases, the present narrative seems also not devoid of instruction. Had an aneurism been discovered or strongly suspected in this case, it seems probable that general blood-letting, together with such remedies as would have contributed to control the heart's action, might have been pursued farther with advantage to the patient; whereas the chronic nature of the supposed laryngeal affection, and the active treatment to which he had already been subjected, were accepted as sufficient reasons for foregoing these remedies and trusting to blistering, ipecacuan, and the performance of tracheotomy. This operation was absolutely refused by the patient while he had sense and vigour, and the case accordingly adds one more to those in which the operation was performed too late to be of any service; but I think it is impossible not to admit that it would probably have prolonged life had it been performed at any period before the final agony, and that the patient's sufferings throughout his illness would have been greatly less severe had advantage been taken of one of the earliest threatening paroxysms of dyspnoea to place a tube in the trachea. In a clearly ascertained case of aortic aneurism, such a proceeding could of course only be proposed as a temporary relief from immediate and pressing danger; and even in this point of view it could only be prudently recommended after careful examination had ascertained the freedom of the lung and of the air-tubes from any considerable pressure; but under these circumstances, I should certainly not hesitate in offering to the patient the benefit, even though temporary, which this little operation is calculated to afford. Much less should I feel justified in withholding it, on the ground of the uncertainty of diagnosis, in cases like the present, where an obvious laryngeal spasm exists, the source of which cannot be discovered, but which is unconnected with any other ascertainable affection of the respiratory passages. For further remarks upon this subject, however, I beg to refer the reader to the report of the debate on this paper in the Medico-Chirurgical Society. —(*See the Medical News for this month.*)

ARTICLE VII.—*Treatment of Stricture by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh. (*Continued from p.* 21.)

CASE IN ILLUSTRATION OF THE IMMEDIATE EFFECTS.

R. L., aged forty-five, a railway porter, recommended to my care by Dr Flucker of Berwick-on-Tweed, entered the hospital on the 25th of January last, in a sad plight from the combined effects of stricture, fistula in perineo, and phymosis. The integuments of the penis, scrotum, and pubes were greatly swollen and indurated, so as to present a most formidable appearance of morbid derangement. The prepuce was not only contracted, but singularly elongated and contorted, so as to form a spiral appendage extending several inches beyond the glans. The scrotum and perineum constituted one mass of induration, with deep fissures and fistulous openings, through which the urine passed without being under control by the patient, who was thus kept constantly in a state of the greatest discomfort. The disease, it was stated, had commenced between five and six years ago.

On the 27th I cut away the redundant portion of prepuce, and united the edges of the skin and mucous lining by sutures.

On the 30th I made a free longitudinal incision through the raphe, and directed poultices to be applied.

On the 1st February, the swelling being greatly reduced, and more free access to the urethra having thus been obtained, I succeeded in passing No. 1 bougie through the stricture, which was anterior to the bulb.

On the 10th February I passed a grooved director into the bladder, and divided the stricture by external incision. There was not more than two teaspoonfuls of blood lost at the operation, and none at all afterwards. The catheter was withdrawn at the end of forty-eight hours.

The patient rapidly improved from day to day, and was dismissed in every respect perfectly well on the 11th March.

CASE IN ILLUSTRATION OF THE REMOTE EFFECTS.

Six years ago a gentleman, about forty, applied to me on account of stricture, for which he had repeatedly been under the care of Mr Liston, both here and in London. I succeeded in dilating the canal, and gave the patient bougies to pass occasionally in order to prevent relapse. Although able, from long experience, to do this very expertly, he found it impossible to keep the canal patent to the full extent, and every few months returned to get another stretch. As he resided on his estate, at the distance of about sixty miles from Edinburgh, these frequent journeys were not very convenient, and he also began to fear that the stricture was gaining ground, so as to

render his efforts inadequate even for palliation. In these circumstances he requested me to divide the seat of contraction, which was at the bulb, and I did so in the month of March 1847. Not the slightest bleeding, or other inconvenience, was experienced, and the water flowed entirely by the natural route. After this the patient still passed a bougie occasionally, but no longer felt any difficulty in doing so, and was also relieved from a variety of unpleasant feelings which had previously annoyed him, even when the urethra had been fully dilated.

About two years ago this gentleman suffered an attack of hemiplegia of the right side, which so impaired the use of his hand as to render him unable to pass the bougie, and then he ascertained the full extent of benefit derived from the operation, since the complaint, instead of becoming rapidly aggravated in this helpless state, has been found, upon examination, to show no troublesome disposition. I passed the full-sized bougie lately with perfect freedom, after a lapse of twelve months, during which no instrument had been introduced.

(*To be continued.*)

Part Second.

REVIEWS.

Abhandlung über Perkussion und Auscultation. Von Dr JOSEPH SKODA. Vierte Auflage. Wien, 1850, s. 324.

Treatise on Percussion and Auscultation. By Dr JOSEPH SKODA. Fourth Edition. Vienna, 1850. 8vo, pp. 324.

Die Erkenntniss der Krankheiten der Brustorgane aus Physikalischen Zeichen, oder Auscultation, Percussion, und Spirometrie, &c. Von Dr J. F. H. ALBERS. Bonn, 1851, s. 245.

The Diagnosis of the Diseases of the Thoracic Organs by means of physical signs, or Auscultation, Percussion, and Spirometry. By Dr J. F. H. ALBERS. Bonn, 1850. 12mo, pp. 245.

A Practical Treatise on the Diseases of the Lungs and Heart, including the Principles of Physical Diagnosis. By WALTER HAYLE WALSHE, M.D. London, 1851. Large 12mo, pp. 580.

OF the many works on auscultation and percussion with which modern medical literature abounds, there is none that can supersede the original treatise of Laennec. So sensible, indeed, have succeeding writers shown themselves of its merits, that, while scarce one of the physical signs of chest-disease is now admitted to have the full pathognomonic significance attributed to it by Laennec,

they seldom fail to record his doctrines, and usually make them the test for their own commentary. Many parts of the Traité d'Auscultation can never become antiquated; discoveries in physics and changes in pathological science may reform its inaccuracies or suggest new interpretations of some signs,—novelties in nomenclature may be introduced,—but the admirable descriptions given by Laennec of what he himself observed of the symptoms and morbid anatomy of certain diseases, adapt themselves to every revolution in medicine. To the practical physician an acquaintance with his works is indispensable.

Of those who, since the days of Laennec, have been improving the means of physical diagnosis of chest diseases, none has laboured more successfully than Professor Skoda. His highly original treatise made its appearance at Vienna in 1839, and soon earned for its author a European reputation. It evinced, it is true, great contempt for all other labourers in the same field—made sad havoc of the doctrines of M. Fournet in particular—exhibited no extreme reverence even for Laennec; and, in the preface to a second edition, expressed the author's disrespect for authorities, and his compassion for those who had read all that had been written on auscultation and percussion.

Ich lege keinen Werth auf Citate; ich gestehe ferner, dass ich denjenigen bedauere, der alles gelesen hat, was uber die Auscultation und Perkussion bereits geschrieben worden ist.—P. viii.

Yet, by the force of honest and patient labour at the bedside, by diligent use of the extraordinary facilities for the study of morbid anatomy afforded by the General Hospital of Vienna, by instituting and frequently repeating a series of simple experiments upon the dead body, and by applying a rigorous logic to most of his observations, Skoda has rendered great service to the cause of physical diagnosis. To prove the non-existence of *pathognomonic* signs seems to be one of the great objects of his treatise. In the outset of his work he arranges the auscultatory phenomena according to their *physical* nature and causes, not in accordance with their supposed pathological significance; and afterwards, in describing the signs of special diseases, shows that, as physical conditions, identical or allied, may be brought about by different pathological processes, so may the same physical sign be met with in connection with different morbid conditions. Were Skoda's work purged of some redundant chapters devoted to the criticism and condemnation of exploded doctrines, amended in a few places where the author, with all his caution, permits hypothesis to usurp the place of fact, and finally translated from rather obscure German into lucid English, it would be an acceptable addition to medical literature. The fourth edition does not materially differ from those which have preceded it.

The second work, whose title is prefixed to this article, bears the well-known name of Professor Albers, and is professedly founded upon some excellent lectures on physical diagnosis delivered by Dr

H. Davis, and published in the "Lancet" for 1850. As these lectures contained a very full analysis of the doctrines of Skoda, and other authorities of the German school, they have been judiciously used as a groundwork for the text-book prepared for the students at Bonn. Professor Albers has not, however, contented himself with a mere translation of the English text, but has introduced, particularly in the latter part of the work, which treats of the auscultation of the heart and arteries, considerable changes, suggested by his own opinions and observations.

The work of Dr Walshe is of a more comprehensive character than either of those above mentioned, including the symptoms, physical signs, and treatment of the different diseases of the chest, and presenting altogether the best guide to their clinical investigation which we have yet met with. Dr Walshe has, we think, acted wisely in not swelling the bulk of the volume, by introducing the details of general and special pathology,—subjects with which the student is supposed to have acquired a moderate acquaintance before he commences the study of disease at the bedside.

In the introduction to the section on percussion, the author advocates the use of the fingers instead of the pleximeter and hammer. We quite agree with him, that the physician should be able to dispense with these instruments in ordinary circumstances. The hammer will, however, be found useful in clinical teaching, especially where it is desired to make the sounds elicited from the chest audible to a numerous class. There are individuals, moreover, who, from awkwardness or some other cause, cannot acquire the proper mode of percussing with the fingers of the right hand, and find no difficulty in managing the caoutchouc hammer. But whether the fingers or the hammer be the plessors, we should be sorry to dispense altogether with the ivory pleximeter,—a modest little instrument, whose production in the sick-room hurts not the nerves of the patient, while its use saves the middle phalanx of the doctor's left index, and enables him,—especially when examining the abdomen or chest of a stout patient,—to proceed with a degree of dispatch and accuracy otherwise unattainable. Dr Walshe prefers the India rubber pleximeter of M. Louis to the plate of ivory recommended by M. Piorry.

In describing the sounds obtained by percussion, observers are too often contented with using the terms *clear* and *dull*. M. Piorry was the first to point out the importance of attending to the sensation of resistance experienced by the percussing fingers, and to the difference of sound on gentle and forcible percussion. Skoda, in describing a percussion sound, endeavours to give some idea, 1st, of its degree of fulness or sonoriety;[1] 2d, of its clearness; 3d,

[1] He employs the words voll, *full*, and leer, *empty*, to define those peculiar quantities of sound by which, as well as by the "pitch," information is conveyed to us regarding the *size of the body from which the sound proceeds.*

whether it be tympanitic; 4th, of its pitch; 5th, whether it have the metallic or cracked-metal character; 6th, of the degree of resistance experienced by the percussing fingers. This method is closely followed by Dr Davies and Professor Albers. In the work of Dr Walshe the percussion sounds are classified according to their *clearness*, *duration*, *quality*, and the degree of *resistance* of the parts percussed. The variations in quality are referred to three types, viz., the *wooden*, *hollow*, and *tympanitic*. The hollow type is subdivided into the *tubular*, *amphoric*, and *cracked-metal*.

If these divisions are not too numerous, some of them at least are of no great importance. Thus, the *full* and *empty*, *high* and *low-toned* of Skoda, the *wooden* and *tubular* of Dr Walshe, are, we believe, superfluous "nuances," which may without impropriety be disregarded. In appreciating the character of any given percussion-sound, the observer need hardly attempt more than to discover, 1st, whether it is produced by light (superficial) or firm (deep) percussion; 2d, whether it is, in the common sense of the terms, clear or dull; 3d, if clear, whether it has the tympanitic, metallic, or cracked-metal character; 4th, whether the subjacent parts resist the fingers which make the percussion stroke.

We may here allude to one very curious, and at first sight rather paradoxical observation connected with the subject of percussion. In certain cases in which a portion of the pulmonary tissue has become *condensed*, the percussion-sound at the points of the chest corresponding is *tympanitic*. There can be no doubt whatever of this fact. Dr Walshe, at page 318, writes, "In much rarer cases (of pneumonia), the percussion-sound may be purely *tympanitic* over the hepatized lung. I have found this at the anterior base on the right side." And again at page 64, he states, "further, as was first noticed by that accomplished observer, Dr Graves, the quality of the note over pneumonic consolidation, sometimes temporarily becomes tympanitic: of this I have now observed two positive examples, in one with, in the other without, plastic lymph on the pleural surface. The only plausible explanation seems to be, that air, the product of secretion, accumulates for a time (in my cases for a few days) in the pleural cavity."

We have ourselves met with instances of the same sort, but have been accustomed to account for them differently. It has been long ago shown by Skoda, that a portion of lung which has lost its elasticity, and yet contains a certain quantity of air, will often, when percussed, yield a tympanitic tone; and we believe that this is commonly the condition of the parts in cases such as those to which Dr Walshe here alludes. To account for this curious production of the tympanitic sound, Skoda offers a theory, which may be briefly stated as follows:—The sound which we hear when the lung is percussed consists in ordinary circumstances of two elements,—the vibrations communicated directly to the contained air by the shock, and vibrations into which the elastic parietes of the

air-cells are at the same time thrown. These two sets of vibrations being different in character interfere with each other, and cause the ordinary *non-tympanitic* lung-sound. But when the elasticity of the lung is lost, as in the course of hepatisation, the direct vibrations of the contained air are not interfered with, and consequently assume the *tympanitic* or pure character. The explanation here offered, however ingenious, is merely theoretical; but, while we deny its sufficiency, we must acknowledge the fact, which we have ourselves frequently verified at post-mortem examinations, that a lung hepatised or compressed *to a certain degree* will often, when percussed, yield a pure tympanitic tone. We are, consequently, inclined to doubt the occurrence of those temporary accumulations of air in the pleura, which Dr Walshe seems inclined to admit.

Analysis of Dr Walshe's chapters on auscultation we shall not at present attempt. The subject is an extensive one, and will probably occupy our attention on future occasions. We, however, cordially recommend this and the remaining divisions of the work to those who are educating their ears for the clinical study of chest diseases.

But let no one suppose that, by reading this or any other book, he can acquire a short and infallible mode of detecting lesions of the lungs or heart. There are many obstacles in the way of such knowledge. Practice is necessary before certain mechanical difficulties in the use of the pleximeter and stethoscope can be mastered,—a correct appreciation of the sounds of the healthy chest must next be attained,—observations in a large hospital, the counsel of teachers, and the study of some good work on auscultation, will illustrate the physical effects of disease; but the student has yet to learn how to make his diagnosis. The ease, accuracy, and legitimate confidence, with which he can detect a lesion will depend, not so much upon the quickness of his ear and delicacy of his hand, as upon the knowledge of disease and of morbid anatomy which he has acquired from experience and study, and upon the honesty with which he mentally appeals to this, while he associates with it in each case all the symptoms and all the signs presented to him, in order to deduce from the combination the truth which he seeks.

On the Prevention and Cure of Spinal Curvatures and Deformities of the Chest and Limbs. By Mrs Godfrey, Liverpool. London. Churchill. 1851.

The fair author of this work begins by stating, that her attention was first directed to the subject of it "by the circumstance, that patients who came to consult my husband for affections of the spine, &c., frequently requested me to be present during their

examination, or to examine them myself previously to their seeing him." The sufferings of these poor people, and the small amount of confidence they seemed to have placed in the skill of her spouse, excited "an anxious concern to discover, if possible, a remedy or relief for so painful a malady. At that time, few works had been written on the subject, nor with what I had seen could I altogether concur; and, human aid being unattainable, I was led to seek wisdom from God, in order that I might discover the cause of the deformity of those bodies he had so beautifully formed; determining, at the same time, to study the anatomy of the human frame, in order that I might ascertain the situation of the nerves and muscles. I was much delighted by the discovery, that every muscle terminated in a tendon, and each tendon was inserted in a bone,—as this discovery gave me a clear idea of the plan of treatment to be adopted, and was the lever of my hope."

This notable discovery in anatomy led, it does not very clearly appear how, to certain principles of pathology and practice still less comprehensible. Displacement of the ribs, or parts of the spine, appear to be the great sources of deformity, instead of derangement in the condition of the bodies of the vertebræ, as hitherto supposed, and are occasioned by circumstances not hitherto suspected:—

"A young lady has just been brought to me with an extensive lateral curve of the lumbar vertebræ, though she has never been allowed to put on stays, and though the utmost attention has also been paid to her position. She has always enjoyed good health, with bone and muscle proportionately strong Great care has also been paid to each part of her dress; and as her mamma had entertained a great horror of strings, she had carefully avoided them. Yet, on examining the dress throughout, I found one part fastened by a large button, which had evidently pressed on the right side of the fourth lumbar spinal joint, causing it to deviate from the line, and to be depressed. This was the starting-point of the curve.

"There was also brought to me a little girl, four years of age, suffering from an angular curve of five of the lumbar vertebræ, and projecting two inches above the column. The integuments surrounding this curve had greatly thickened, and the spinal ribs were drawn from their proper position, causing on each side a deep hollow over the hips. As the disease had advanced so far, notwithstanding the early age of the patient, it must evidently have commenced in infancy. I immediately inquired, if the nurse had fastened the band of the small flannel tightly round the waist? Her mamma replied, that the monthly nurse had always done so, for the purpose of strengthening her loins. But, if this lady had understood the structure of the body, she would have known that such treatment, instead of giving strength, could tend only to destroy the foundation of it, by pressing in the short ribs and joints of the spine."—Pp. 38, 39.

"When children are a little older, they are made to wear trousers fastened by a large button, which has been a frequent cause of curvature. If the button is pressed against the side of the bone, the curve will be lateral; if upon the top, it will be anterior, causing a projection above the bone, which is flattened."—P. 40.

"An interesting little girl came to me, looking full of blooming health. Her mamma had discovered a sinking of the ribs on one side of the chest, which could not be accounted for; all her dress having been made with the utmost care, to avoid unequal pressure. On examination, I discovered four of the ribs on the

left side of the sternum elevated, and four on the right inverted. Finding that her dress throughout was correctly made, and that her position had been carefully watched, I caused her to be dressed before me, when the frock on one side of the front appeared to be drawn down lower than the other. I then requested that the pocket might be searched, and, to the surprise and amusement of her mamma, it was found filled with pence, pebbles, and a heavy knife. The little creature most ingenuously told us she always carried as much in it." —P. 43.

The treatment established on this foundation is conducted on strictly homœopathic principles, since the deformity, being caused by pressure, is also to be remedied by pressure, through assiduous friction by "a skilful hand."

We should be glad to know what motive has induced the 'medical publisher,' whose name appears on the title-page of this book, to offer such a production to the profession and the public. Was it to convey practical information to the members of a learned profession?—was it slily to ridicule the "Practical Treatises" with which the press has of late been teeming?—or was it to prove the hopelessness of opposing such popular delusions as homœopathy, by showing the extent of absurdity to which honest John Bull allows himself to be gulled?

The New London Pharmacopœia, translated and arranged in a Tabular form, with the Edinburgh and Dublin Pharmacopœias, showing at one view the differences in the Formulæ of the three Colleges, together with the Tests given by each College for the purity of the several Preparations, with Practical Remarks. By PETER SQUIRE, M.R.I., &c. Churchill. London, 1851.

THE facility and increase of locomotion in this country render it more necessary than ever that even practitioners, and still more druggists, be acquainted with the pharmacopœias of all the divisions of the empire. Mr Squire, a pharmaceutic chemist of note in London, has undertaken to supply a commodious work for the purpose, in which the formulæ of the three Colleges are placed side by side in three conspicuous columns. In many works on pharmacy and materia medica the same object has been previously effected more or less. But this is the first which has been undertaken for no other purpose; and naturally of course its purpose has been accomplished well. Of the competency of the author to execute it no one can doubt, who is acquainted with his zeal and success in the improvement of pharmacy. The wonder is that a man of his position and character should be found willing to encounter so irksome a labour; for the "Practical Remarks" on the formulæ are so few and brief, that their absence would not have

materially detracted from the merits and practical value of the book.

Mr Squire is deeply impressed with the necessity of a National Pharmacopœia, or a conjunct pharmacopœia for the whole British empire. He apparently thinks it a sufficient argument, that "when patients are travelling from one part of the kingdom to another with such rapidity, a prescription written in London may be prepared in Edinburgh or London the following day, and with widely different results;" and that it may be seen by "any one who looks into the formulæ of our three Pharmacopœias, that the differences in strength are sufficient to justify serious apprehensions as to the consequences." But he does not rest his proposal, perhaps through dread of the charge of selfishness, upon a much more real and cogent reason,—the exceeding inconvenience to the dispensing chemist of being compelled often to walk by three different rules. As for the risk of error, at least of serious or dangerous error, it is greatly less than might be naturally imagined. Error is seldom committed where the risk of it is apparent to all; and he would be a very stupid or very lazy chemist, who, in Edinburgh or Dublin, would use the local Pharmacopœia for making up the prescription of a London physician, or conversely. It would have been well nevertheless that Mr Squire had stated his own experience on this head. Our own impression certainly is that errors are rare and immaterial. But no one can call in question the exceeding annoyance occasioned by three formulæ to the dispensing chemist; or that it is the duty of the Colleges to relieve him.

As little can any one doubt that this duty has not been thought a very binding one by the framers of recent editions of the Pharmacopœias. The Dublin College have not lost sight of it; the Edinburgh College had previously set them a good example; yet both might have carried their efforts at approximation farther without prejudice. By the London College of Physicians, the duty of approximating the Pharmacopœias seems to have been understood in a peculiar sense. It seems to have been assumed that the London Pharmacopœia is to be the centre, with which the others may close as they see fit. There is a glaring instance of this in the familiar Morphia Solution. In 1831 Dr Gregory, the present professor of chemistry in Edinburgh, discovered a cheap process for preparing muriate of morphia; and his medical friends, who made experiments with it in hospital and private practice, sensibly adopted for use a solution of the same strength with laudanum.[1] This preparation immediately came into universal use in Scotland. It was admitted into the Edinburgh Pharmacopœia in 1839. It was transferred into that of Dublin in 1850. But in 1851 the London

[1] Edin. Med. and Surg. Journal, 1831, xxxv. 331.

College, tardily admitting such a preparation for the first time, have doubled its strength. It is no excuse, even if it was an existing reason, that a preparation of that exact strength had come into use in London. The preparation came first into use here; it was first sanctioned by authority here; a good reason could be shown for the strength adopted, and was indeed palpable to every one; no essential reason could exist for altering it; another authority had adopted it. But the London College must have a preparation of its own; and this object is simply attained by doubling the strength of the solution. Mr Squire may well say, as his single practical remark on this solution, "It is much to be regretted that solutions of morphia differ so widely in strength." He might have added, that, if the question of amalgamation of the Pharmacopœias in this article has come to involve a question of honour,—the most difficult of all questions to settle,—the fault lies solely with the College of London. This was a sin of commission. As an example of omission,—an error, however, more easily rectified in future,—the Edinburgh colocynth and henbane pill may be referred to. At least thirty-five years ago, probably much more, the late Dr Hamilton, professor of midwifery in Edinburgh, made known to practitioners the invaluable property possessed by henbane of counteracting the irritant and griping tendency of many useful cathartics. A pill, known by his name, gradually came in consequence into universal use here and elsewhere; and a similar preparation, but of variable composition, has also been much employed in London. It 1839 it was with great propriety admitted into the Edinburgh Pharmacopœia, and its exact composition fixed after careful inquiry. No man can doubt that this was a valuable addition. The College adopted what every one used; for there is no pill superior, probably none equal to it, for ordinary purposes, as an efficacious yet mild laxative. This pill has not been adopted, however, by the London College, although probably every physician, both in London and throughout England, prescribes something like it.

While Mr Squire's work shows satisfactorily all the important differences between the three pharmacopœias, it is not less useful in pointing out the miserable petty peculiarities of each in points of no real moment whatever, but which must be a source of perpetual annoyance and vexation to the dispenser, who aims at a conscientious discharge of his duty to the medical profession. It is to be hoped that the book will convey many a useful lesson on such matters to the future improvers of pharmacopœias.

For indeed it is quite plain to any one acquainted with the history of the pharmacopœias and Royal Colleges during the last twenty years, that a conjunct or National Pharmacopœia, the darling object of Mr Squire's wishes, is now, and must for some time be, a mere chimera and impossibility. Until the Colleges shall spontaneously approximate the existing pharmacopœias to one

another, amalgamation will never be nearer than at present. How much may be thus effected will be apparent to any one who casts his eye over Mr Squire's triple column, and who assumes the existence of a mutual spirit of accommodation. The Edinburgh College will probably, in the nature of things, be first called on to imbibe this spirit, and, it is to be hoped, will not be deterred from so doing by the opposite example of the College of London.

Something more, however, than a spirit of accommodation is necessary, before even a friendly rivalry in the work of approximation can end in a conjunct Pharmacopœia. Some look to one side of the question so entirely, that they would even bring down the power of the legislature upon the Colleges, and thrust upon them a National Pharmacopœia. This would be a fatal blunder. Such zealots would begin at the wrong end. The practice of pharmacy in the three countries must approximate, before it can become identical: it cannot be violently made identical in theory, in order practically to approximate. The habits of practitioners in Scotland and England, in the practice of prescribing, must become less dissimilar, before one pharmacopœia will conveniently serve for both. The simpler system of physicians in Scotland requires one set of formulæ; the more complex method of English physicians another; and the former at least could not work easily with the latter's tools. For example, the Edinburgh Pharmacopœia adopts a concentrated Compound Iodine Solution, partly because it was that of the discoverer of the virtues of iodine, but chiefly because, being capable of administration in the dose of five or ten drops in mere water as a vehicle, it suits well with the habits of Scotch physicians and Scotch patients, when a remedy must be given often, for a long time, and while the patient may be moving from place to place. English physicians are not so mindful of these circumstances, because their more elaborate prescriptions, the offspring of the apothecaries' drugging system, renders their extempore remedies more cumbrous and less locomotive. There is no other ostensible reason why the London College should have adhered, in the recent edition of the Pharmacopœia, to their bulky form of the compound iodine solution; which must be given in thirty times the dose of the Edinburgh solution in order to attain the same effect.

There is much to be considered, therefore, and much to be reformed, in medical practice, before a National Pharmacopœia will be within reach. But, in the meantime, a great deal may be done towards so useful an object by the removal of unnecessary and often senseless differences; and Mr Squire's Tabular Form will greatly facilitate this reformation by pointing out clearly where such differences exist.

Portraits of Diseases of the Skin. By ERASMUS WILSON, F.R.S. Fasciculus 9.

THE present fasciculus contains representations of what Mr Wilson calls Lichen Annulatus Solitarius, several specimens of which, occurring in different individuals, he has caused to be drawn on one person. For the sake of economy in illustration we do not object to this procedure, although it renders the "portrait" as unlike anything in nature as can well be conceived. The next plate, if it be a faithful copy, must be considered as representing a very remarkable case of roseola in the leg. The third plate represents a so-called syphilitic lichen of the abdomen, although to us its syphilitic origin is very doubtful. The fourth plate gives us a very good example of psoriasis guttata. This work retains its character for careful finish and beauty of execution.

Hand-Book of Natural Philosophy and Astronomy. By DIONYSIUS LARDNER, D.C.L., &c. First Course, including Mechanics, Hydrostatics, Hydraulics, Pneumatics, Sound and Optics. London: Taylor, Walton, and Maberly, 1851. 8vo, pp. 820.

THE object of this work is to supply to those who have not the leisure to pursue the mathematical investigations of physical science, such a compendium of the facts in its chief branches, and such illustrations of the methods pursued in their acquisition, as may be usefully applied by the artisan, the man of letters, or the student of medicine. The treatise seems based upon the materials of several excellent volumes, which originally constituted the scientific part of Lardner's Cabinet Cyclopædia, and in preparing which the author had the advantage of the co-operation of many of the most eminent natural philosophers of this country, *e. g.*, Herschel, Kater, and Brewster. In the work now before us, condensation has been practised without sacrifice of perspicuity, and pains have been taken to illustrate fairly the present state of science. The volume contains 420 woodcuts, is exceedingly moderate in price, and alike creditable to its author and publishers.

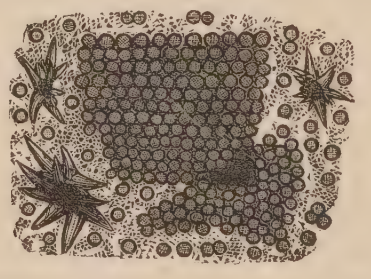
Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

COLCHICUM IN THE DELIRIUM AND COMA OF SCARLATINA.

A boy, æt. 14, entered the clinical ward on the third day after experiencing distinct rigors. There was restless delirium, and constant moving of the head from side to side on the pillow. He was apparently conscious when spoken to, but could not answer questions—the tongue was protruded with difficulty, dry, and of bright red colour, studded with florid elevations—deglutition was much impeded—bowels open—pulse 130, weak—urine voided with difficulty, and diminished in quantity—sp. gr. 1025—not acted on by heat and nitric acid—skin hot and dry, covered with the bright red scarlatinal eruption—*Ordered salines and slight diuretics.* He continued in the same condition, the angina coma and alternating delirium, however, being more pronounced until the sixth day. During this period all the urine passed was carefully examined. The amount was diminished (17 oz. per day), but it was free from deposit, and unaffected by heat or nitric acid. ℞ *Sp. Æther. Nit.* ʒiij; *Pot. Acet.* ʒij; *Tr. Colchici* ʒss. *Aquæ,* ʒiij. *Ft. Mist. A teaspoonful to be taken every four hours.* On the following day all coma and delirium had disappeared. He answers questions when put to him—skin cool—eruption faded — pulse 96, weak—passed 30 oz. of urine, which is turbid, with small flakes of a membranous character floating in it. On the 8th day the quantity of urine excreted was 50 oz., and it was still more loaded with sediments. On examining the urine with a microscope, it was seen to contain—1st, membranous flakes,



Urate of ammonia, in a granular membranous form, and in rounded masses, with spicula.

composed of aggregated rounded particles, apparently agglutinated together, and strongly resembling some forms of vegetable tissue—2d, rounded and irregular masses with spicula—3d, amorphous molecular masses. The whole of these elements on being analysed by Mr Drummond, were found to consist of urate of ammonia. Next day the urine was only slightly turbid, and on the following one, was perfectly clear. From this time the boy gradually recovered.

Commentary.—This was a very severe case of scarlatina. The angina was intense, occasionally rendering deglutition impossible. There was delirium on the third day, alternating at night with coma, which was often profound. The worst result was apprehended. It recurred to me that the head symptoms in this as in several cases of typhus, might probably depend not so much upon inflammation of the brain, as is generally supposed, as upon absorption of, and poisoning by, urea, an idea that appeared to me supported by the diminished

quantity of the renal excretion, as well as its freedom from all deposit. Remembering the alleged virtues of colchicum in increasing the elimination of this excretion, I ordered it, in combination with diuretics, and the result was remarkable. For on the next day, not only had the fever diminished, but the urine was increased in amount and loaded with urates to an extent and in a form I had never previously seen. It may be argued that the fever had terminated by a natural crisis on the seventh day; but I cannot help thinking that in this case nature was assisted by the colchicum and diuretics. At all events, this medicine seems to me worthy of more extensive trial in scarlatina accompanied by diminution of urine and head symptoms.

BLEEDING IN PNEUMONIA.

I have on a former occasion pointed out the rule which, it appears to me, should guide you with regard to bleeding in pneumonia. If you are called to a case at a very early period before exudation is poured out, and before dulness as its physical sign is characterised, but when notwithstanding there have been rigors, embarrassment of respiration, more or less pain in the side, and commencing crepitation, then bleeding will often cut the disease short. This state of matters is rarely seen in public hospitals. When on the other hand, there is perfect dulness over the lung, increased vocal resonance, and rusty sputum, then exudation blocks up the air-cells, and can only be got rid of by that exudation being transformed into pus, and excreted by the natural passages. In such a case, bleeding checks the vital powers necessary for these transformations, and as a general rule, if the disease be not fatal, will delay the recovery. I believe this to be the cause of so much mortality from pneumonia in hospitals where bleeding is largely practised, for in general, individuals affected do not enter until the third or fourth day, when the lung is already hepatized. Three cases which you have witnessed, appear to me to support the correctness of these principles :—

Case I.—John Conolly, æt. 50, re-admitted June 16th, for mitral disease of the heart. At the visit on Monday June 30th, he was in his usual state of health. On the day previous, he was allowed to go out and see his friends and stay all night. He acknowledges that he drank considerably. About two o'clock P.M. of the 30th, he felt a sharp pain in the right side, in a circumscribed spot, an inch below the nipple, increased on taking a full inspiration. In the evening, the clerk detected a double friction murmur in the part complained of, and the pulse was 106, full and strong. ℞ *Pulv. Opii. gr. iij.; Antim. Tart. gr. vj; Pulv. Arom.* ℈*j. M. fiant. Pulv. vj. One to be taken every second hour.*—*July* 1, at the visit. Pain in side continues, troublesome dry cough occurs at intervals, friction murmur continues as heard last night. Pulse 111, full and strong. ℞ *Antim. Tart. gr. vj.; Pulv. Opii gr. ij.; Sacchari Albi* ℈*j. M. Div. in Pulv. vj. One to be taken every hour. Applicent. Hirudines viij. regioni dolenti. July* 2.—Has taken four of the powders—leeches bled freely—on examining the chest, it continues everywhere resonant on percussion. Double friction murmur still heard below the right nipple. Above the nipple it is only heard with expiration. There is also distinct though exceedingly fine crepitation at the lower margin of the right lung posteriorly, and slight increase of the vocal resonance. Respiratory murmurs of left lung puerile and tubular. *Venesectio ad* ℥*xv. et habeat Pulv. Opii gr. j.* He felt no faintness from the bleeding, but the pulse which was 110, full and strong, fell to 86, and became soft. The blood drawn coagulated quickly, presented a strong buffy coat, and became cupped. *In the evening* there was profuse diaphoresis, the pain in the chest had abated, and the breathing was much easier. *To continue the Antimonial powders. July* 3.—Pain in the side has nearly disappeared, he can inspire deeply without difficulty, friction murmur scarcely appreciable to the ear, and no crepitation can be heard. *July* 4.—Countenance pale and sallow, with a depressed expression. Complains of weakness, but no pain, no friction or crepitating murmurs in chest, cough still troublesome.

An expectorant mixture was ordered in the evening by the clerk. From this time he became gradually well, the heart affection remained unaltered, and he was discharged July 14.

CASE II.—James Burns, æt. 24, admitted into the clinical ward, May 20, for syphilitic nodes. He had no cough, nor expectoration; and on examination of the chest, both at the time of admission, and subsequently, his respiration was perfectly healthy. On the 19th of June he commenced coughing, and on the 22d, crepitation was detected by the clerk under the right clavicle, with increased vocal resonance, and slight dulness on percussion. *Two leeches were applied.* On the 23d, he was hot and feverish; pulse 110; cough severe, with dyspnœa; expectoration slightly tinged with blood. Still crepitation under right clavicle, but also dulness over the lower third of left chest, posteriorly, with crepitation, and increased vocal resonance. *℥xij. of blood were drawn by venesection.* ℞ *Vin. Antim. ℥ss. Sol. Mur. Morph. ℥iij. Aquæ ℥v. M. One ounce to be taken every three hours.* On the 24th, my attention was directed to him for the first time, and I found limited dulness under the right clavicle, and complete dulness over two-thirds of left lung posteriorly. There was also distinct crepitation, and increased vocal resonance, in both regions, which were dull; pulse 108, of good strength; sputum tinged with blood; bowels constipated; œdema of the feet. *The treatment now consisted of antimonials, salines, and diuretics.* On the 27th, although the crepitation and dulness had extended, some hope was entertained of recovery, from sudden copious diaphoresis, and the loaded state of the urine, which contained abundant amorphous sediment of urates, mingled with fibrinous casts of the tubes. The pulse, however, became weaker; and notwithstanding the liberal administration of wine and stimulants, he sank on the 28th. An examination of the body could not be procured.

Commentary.—In both these cases pneumonia came on in the ward, the individuals having been admitted sometime previously, the one for chronic cardiac disease; the other for tertiary syphilis. In the first case, after drinking, and probable exposure to cold, during a visit to his friends, pleurisy came on, followed on the second day by pneumonia, as determined by fine crepitation, and slight increase of vocal resonance. Notwithstanding the existence of mitral disease, he was bled to ℥xv., followed by a full dose of opium, and his recovery was rapid. In the second case, a man of syphilitic constitution, the symptoms were more latent, and he was bled on the fourth day, when dulness was established over two-thirds of one lung, and a portion of another, and he died on the ninth day. There must always remain a doubt as to whether, in this last case, phthisis did not exist, as evidenced by the dulness, fine crepitation, and vocal resonance under one clavicle, cough and sweating,—a point I had no opportunity of determining, as I had only just then taken charge of the wards. But such a suspicion, conjoined with the undoubted syphilitic cachexy, were rather opposed to bloodletting, notwithstanding the quality of the pulse, and the threatened advance of the disease. No doubt it is a difficult matter to establish any general principle as to bleeding in pneumonia; but I think the two cases now noticed point out the benefit of that practice at one stage of the disease, and its inutility, if not danger, at another.

CASE III.—Edward Lanon, a labourer, æt. 17, admitted into the clinical ward July 18, 1851. He is of strong, robust constitution; but on the 14th, was suddenly seized when in bed with a sharp, stabbing pain in the right side; increased ou deep inspiration. This was soon followed by rigors, and the usual symptoms of fever. On the following day he was bled to 16 ounces by a medical practitioner, and took opening medicine. The pain continuing next day, he was also cupped. On admission, the cough, though not frequent is very harrassing, accompanied by slight pains in the left side; no expectoration. On percussion there is dulness over left side of lung anteriorly, and over two-thirds of right lung posteriorly and inferiorly. On auscultation, there is crepitation and increased vocal resonance in both seats of the dulness, and double friction

murmurs over right lung anteriorly. On the 19th, crepitation also heard posteriorly on the left side; pulse 106, strong. On the 20th, the friction murmurs had diminished. Sputum also now abundant, tenacious, and of rusty colour. 21st, crepitation on left side diminished; pulse 90, strong. Urine, which up to this time was perfectly clear, now loaded with a copious sediment of amorphous urates. A blister to be applied to the right side. 22d, crepitation now loud, and coarse on both sides; no friction; sputum abundant; urine much clearer than yesterday. 23d, slight crepitation. 24th, no crepitation; fully convalescent.

Commentary.—This was a case of double pneumonia, occurring in a strong, healthy lad, who had been very judiciously bled largely by the practitioner who saw him on the second day, not with the effect of cutting it short, but probably of diminishing its intensity and extent. On admission, notwithstanding the strength of the pulse, we refused to bleed, for the reasons previously stated; but favoured excretion of the morbid products by antimonials and diuretics. The result was copious elimination of urate of ammonia by the kidneys on the 7th day; diaphoresis and recovery on the 10th day.

OCCASIONAL DIFFICULTIES IN THE DIAGNOSIS OF PNEUMONIA.

The following cases illustrate the difficulties which occasionally present themselves in the diagnosis of pneumonia, notwithstanding all the aid furnished to us by a careful study of functional symptoms, combined with physical signs:—

Case I.—*Acute Nephritis; Hydrothorax; Collapse of the Lung; Bronchitis.*—A man, æt. 45, a waiter, entered the clinical ward labouring under bronchitis, Bright's disease, and supposed pneumonia. Six days before admission, there had been distinct rigors, fever, and vomiting; the kidney disease was indicated by diminished amount of urine, of darkish brown colour, spec. grav. 1014, highly coagulable on the addition of heat and nitric acid, with general anasarca. The pneumonia was imagined to exist on account of limited dulness of the lower margin of right lung, extending two inches above hepatic dulness. Inspiratory murmurs over this region, anteriorly harsh and tubular, but posteriorly and inferiorly accompanied by distinct crepitation. Sputum very viscid, and rusty coloured, with small clots of blood, size of a pea. On the following day the dulness on percussion and crepitation had extended upwards to the middle of scapula. Sputum strongly pneumonic. He died on the 6th of July comatose. *On examination after death,* the body was generally anasarcous. Both pleural cavities contained a considerable amount of serum, most abundant on right side. The kidneys were of a dark mahogany colour, covered with hemorrhagic spots. These, on microscopic examination, were found to depend on hemorrhage into the tubes, while the corpora Malpighiana were ascertained to be very generally loaded with recent exudation. The lung on the right side was collapsed inferiorly, sunk readily in water; but on microscopic examination, was found to be perfectly healthy in texture. There was intense bronchitis, and the tubes were loaded with mucus.

Case II.—*Organic Cardiac Disease; Bright's Disease; Vesicular Pneumonia.*—A man, æt. 44, entered the clinical ward labouring under cardiac disease, Bright's kidneys, and pneumonia—evidently in a dying state. The functional symptoms and physical signs indicative of the pneumonia were as follows:—Troublesome cough, dyspnœa, with pain in right side of chest, copious sputum, which was gelatinous, purulent, and of rusty colour. On percussion there was comparative dulness over the inferior third of right lung, both anteriorly and posteriorly. On auscultation, loud crepitation was heard over inferior portion of right lung, with inspiration, and increased vocal resonance. On the opposite side, puerile respiration, but no dulness or crepitation. These symptoms increased in intensity during the next two days, while the physical signs became more marked. He died on the 11th of July. *On examination*

after death, extensive cardiac and renal disease was discovered as anticipated. The left lung was healthy. The inferior lobe of right lung was of a dark mahogany colour, crepitant throughout, and floated in water. On section, however, it yielded on pressure a fluid like prune juice, and there were visible here and there yellow deposits, scattered through the parenchyma at considerable distances from each other, varying in size from a small millet seed to that of a pea. A thin slice of the lung with Valentine's knife, examined microscopically, showed that the yellow masses, though to the naked eye they exactly resembled tubercle, were really composed of finely molecular exudation, filling up a group of air vesicles. Similar exudation could also be seen filling up individual air vesicles, and the smaller bronchial tubes, which, on breaking up fragments of the pulmonary tissue with water, floated loose, constituting casts of their cavities. Those removed from the smaller bronchi, exactly resembled the exudation casts of the tubuli uriniferi, so commonly found in the urine. The pulmonary parenchyma was also loaded with compound granular cells, brownish masses, bright red crystals (of hematine ?), and groups of blood globules undergoing disintegration.

CASE III.—*Anemia; Bronchitis*; *Fatty Heart, Liver, and Kidney; Curvature of Spine; Chronic Pneumonia.*—A woman, æt. 23, entered the clinical ward 8th July, 1851, with general anemia, palpitations and anemic murmur of the heart, bronchitis, enlarged liver and Bright's disease. On percussing the chest, marked dulness at the apex of right lung, loud gurgling râle, and pealing resonance of the voice in the same situation; moist and dry râles were also heard over other parts of chest posteriorly, but no dulness on percussion; copious purulent sputum and night sweats. She died on the 13th, and in addition to fatty heart, liver, and kidneys, it was found that the apex of right lung was consolidated, of grayish colour, sinking rapidly in water; covering it were the pleuræ, strongly united by chronic adhesion, On microscopic examination, the air cells and parenchyma of the affected lung were infiltrated with granular exudation, mingled with numerous compound granular cells. There was also general bronchitis.

Commentary.—In the first case, pneumonia was diagnosed, but there was found hydrothorax, with collapse of the lung. In the second case, pneumonia was also diagnosed, but the lung appeared to the naked sight only congested with a few tubercles, and was determined to be really pneumonic by the microscope alone. In the third case, phthisis was diagnosed, but there was chronic pneumonia confined to the apex of the right lung, and chronic bronchitis. It may be useful to analyse the facts of these cases, so as to derive as much instruction as possible from the theoretical errors which resulted from them.

In Case I. there was dulness on percussion over the right back, inferiorly and posteriorly, increased vocal resonance, and fine crepitation. These physical signs, though generally indicative of a pneumonia, are not necessarily so. They simply show increased condensation, with pressure on the air vesicles, and, observe, this was found to be the case, for there was hydrothorax, and condensation of the lung from external pressure. Similar signs are often produced by a thoracic aneurism. The physical signs themselves would not have misled us, but there were associated with them rigors and fever six days previously, and rusty sputum mixed with blood. Thus the febrile attack corresponded with the commencing period of the supposed pneumonia. Then the man was a waiter and a habitual tippler, and we were called upon to decide whether the acute symptoms were connected with the lung or with the kidney. Now it is rare to see a case of acute nephritis producing general anasarca, and running its course so rapidly, and in consequence we considered the renal disease to have been chronic—in short, an ordinary case of Bright's disease, with supervening pneumonia. It turned out, however, to be an acute attack of nephritis, accompanied by rigors, fever, vomiting, &c., followed by rapid anasarca and death by coma. The rusty sputum was also calculated to mis-

lead ; for although the air tubes were filled with tenacious purulent mucus, there was no appearance after death of bloody extravasation into the parenchyma of the lung. It must, therefore, have been altogether bronchitic. Such a singular case of acute nephritis, so complicated, must be considered of extreme rarity, and in no way diminishes the value of the general conclusions to be derived from the ordinary functional symptoms and physical signs of pneumonia.

In case II. we should have considered ourselves to have been in error, had it not been for the microscope. In addition to undoubted cardiac and renal complication, there were all the functional symptoms and physical signs of pneumonia. Owing to the friends taking away the body at an unusual hour, the examination was performed by the house-clerk in my absence. He informed me next day that there was no pneumonia ; but fortunately he had preserved a portion of the lung, and on first examining it I believed he was correct. It was of a dark mahogany colour, crepitant throughout, and floated in water. On section it yielded a copious dark red, frothy fluid, and there existed what had every appearance of yellow miliary tubercle thinly scattered. In short, the lung was in the condition of what is called "infarction," loaded with blood, but not pneumonic. A microscopic examination, however, proved that the so-called tubercle was composed of molecular exudation, and that numerous vesicles and small air-tubes were completely blocked up by a similar deposition, more or less mingled with blood. Hence it *was* a case of pneumonia, but of a form that has never yet been described, confined to groups of air vesicles and small tubes, and which may perhaps be designated as *vesicular*, to distinguish it from *lobar* or *lobular* pneumonia. I have no hesitation in stating that no one, from the ordinary morbid appearances alone, would have pronounced the lung in question to have been loaded with exudation, although when its structure was carefully examined with a microscope, this became evident to every one present.

Case III. was a very complicated one, and yet the bronchitic, cardiac, hepatic, and renal lesions were accurately made out previous to death. On one point we were wrong. The woman presented a most emaciated appearance, the surface was blanched, there was curvature of the spine from chronic disease of the bones, profuse sweating, severe cough with purulent sputum, marked dulness under the right clavicle, and on auscultation, coarse moist râles, with loud vocal resonance. The conclusion was, I think, inevitable, and *not to be avoided* —namely, that there was phthisis ; and yet on examination after death, there was no tubercle, but chronic pneumonia, limited to the upper pulmonary lobe on the right side.

TREATMENT OF PHTHISIS PULMONALIS.—THE CRYSTAL PALACE.

Patrick Barclay, æt. 17, was originally admitted into the clinical ward, June 25th 1849, labouring under cardiac disease, and a large tubercular cavern under the right clavicle. He was emaciated to the last degree, and had profuse sweating, frequent cough, and purulent expectoration. You will find the particulars of his case in the February No. of the Monthly Journal for 1850. By means of cod-liver oil, good diet, appropriate treatment, and counter-irritation, he became stout, and even fat—the cough greatly diminished—the expectoration ceased, and he believed himself well. But the physical signs of a dry cavity continued—he had also scrofulous caries of the femur, but was discharged much relieved on the 27th of February 1850.

He was re-admitted August 26th 1850. Since leaving the house he has been frequently exposed to cold, but has been at the Industrial School,—and latterly the cough and expectoration which he said had quite left him, have returned, and been gradually getting more severe. The sweating returned with the cough. A week before admission, he, with the other boys of the school, went to Portobello to bathe, and notwithstanding his remonstrances, the master insisted on his going into the water, saying it would do him good. He however

became much worse. On admission, the physical signs were coarse moist râle under the right clavicle, imperfect pectoriloquy, and creaking friction noises, harsh inspiration and prolonged expiration under left clavicle, but the dulness in this last position is very slight, when compared with that of the opposite side. He again, by means of cod-liver oil, good diet, and counter-irritation, became strong and stout; again the cough, expectoration, and other symptoms ceased, and he was discharged March 7th 1851. The report on that day, is "marked dulness and increased vocal resonance under right clavicle, the inspiration is harsh but dry."

He was admitted again July 5th 1851. He says that on leaving the ward in March last, he had two detached pieces of the right thigh bone, extracted by Mr Syme, and remained in the surgical hospital for five weeks. Since then he has been constantly employed in light garden work, and notwithstanding poverty of food, he has continued in tolerably good health till a week ago. On admission, he again looks pale and thin. There is severe cough, with mucous expectoration,—under the right clavicle, the inspiration is harsh and blowing, no moist râle. The voice is resonant to bronchophony. There is loud double friction murmur over the upper part of right lung, especially at the right apex —slight friction also over whole of right side—on percussion, slight dulness only under right clavicle. Posteriorly, the resonance on percussion is nearly equal on both sides. This is the condition in which he now is.

Commentary.—I have frequently tried to impress upon you that the general indications to be followed out in the treatment of phthisis pulmonalis, are first, to support the general nutrition of the economy—second, to keep down local irritation—and third, to avoid all those causes which can deteriorate the constitution, on the one hand, and induce pulmonary symptoms on the other. The first indication is to be followed out by cod-liver oil, good diet, exercise, and all those means which are necessary to keep up a vigorous nutrition. The second indication is to be met by counter-irritation, and the third by hygienic regulations, an equable climate, and attending to all those precautions liable to prevent "catching cold." Could all these indications be carried out, I feel satisfied the cure of phthisis would be more frequent; but in the treatment of this disease the physician has to struggle not only with the deadly nature of the disorder, but with numerous difficulties over which he has no control, such as, among the poorer classes, the impossibility of procuring good diet, and the thousand imprudences not only they, but the majority of cases, are continually committing. Then, another great difficulty is, to convince the patient that, notwithstanding the removal of his urgent symptoms, the disease is not cured, and that these will return, if the causes which originally produced them, are again allowed to operate. Sometimes I have found it difficult to keep hospital patients in the house when they are doing well, at other times they are sent out in accordance with certain regulations, which oblige the admission of more acute cases. This was the case with Barclay. So long as he is under treatment, or rather enjoys the comforts and good diet of the Infirmary, so long is he well, but send him out, expose him to misery, insufficient food, and work, and he gets worse. But you will observe from the progress of the case, that his dwelling here has still done him good upon the whole. The physical signs point out to us that the pulmonary ulcer is contracting and cicatrizing; and I consider that could he be kept here another twelvemonths or so, his ultimate permanent cure would be insured.

But the great difficulty we have to overcome in this climate after all, is the frequent variations of temperature, and the sudden changes from fervent heat to chilling cold. Supposing that you have the means of supporting nutrition and keeping down local irritation, it is by no means certain that good will be accomplished, from the impossibility of securing those hygienic regulations and that equable climate, which are necessary to carry out the third indication. In the first place, nutrition itself is more connected with proper exercise and breathing fresh air than many people imagine. It does not merely consist in sti-

mulating the appetite and giving good things to eat. It requires—1st, food in proper quantity and quality—2d, proper digestion—3d, healthy formation of blood—4th, a certain exchange between the blood and the external air on the one hand, and between the blood and the tissues on the other—and 5th, it requires that there should be proper excretion, that is separation of what has performed its alloted function and become useless. *All* these processes are necessary for nutrition, and not one or more of them,—they are all essentially connected with, and dependent on, one another ; for supposing that we can procure everything but pure and balmy air, still if that be deficient, the great problem of the cure of consumption cannot be worked out.

Now, it has been proposed that the Crystal Palace should remain, and be converted into a winter garden and public promenade. Not to speak of the intellectual and recreative purposes that such a plan would subserve, it is worthy of our consideration how far it would tend to promote health in general, but especially, how it would conduce to the cure of phthisis. Its great advantage would be offering the means of exercise in a pure atmosphere, at an equable temperature. It is easy for us, by confining patients in a suite of rooms in which the heat is regulated, to secure immunity from cold and change of air ; but such a contrivance is most intolerable to the patient, the mind becomes peevish, which in itself is a powerful obstacle to the proper performance of the digestive functions. But above all, the body is deprived of exercise—that necessary stimulus to the appetite, respiration, and other functions. Some years ago, I succeeded in confining a consumptive patient to his room for an entire winter. His spirits suffered greatly ; but on the whole he supported the imprisonment with resolution. Next winter, however, nothing could induce him to remain at home, and one day he rushed out of the house, ascended Arthur Seat, and was much better in consequence. Since then I have been convinced that, although by confinement you may gain some advantages, on the whole it is a prejudicial practice if rigorously carried out.

What is required in these cases is the means of exercise, whether on foot, on horseback, or in a carriage, where the patient is protected from cold winds, and where the mind can be amused by pleasant sights and cheerful conversation. Such is the case in all those favoured localities considered best for consumptive people, and such would be the advantages derived from converting the vast enclosure of the Crystal Palace into a winter garden and promenade. Delicate individuals could be transported, by means of a close carriage, in the worst seasons without difficulty to such a place, and on entering it could breathe for hours a pure, balmy air, meet their friends, take exercise in various ways, read, work, or otherwise amuse themselves. Such an out-door means of recreation, combined with careful hygienic regulations at home, would go far to remove many of the difficulties which we have to encounter in the ordinary treatment of consumption. So great would be the boon to the community, that, once established in London, we should soon see similar buildings in all the large cities of the kingdom. Indeed, its advantages are so obvious, that already the new Hospital for Consumption, erecting in Victoria Park, London, has procured the services of Mr Paxton, to design for them an enormous Green-house, or Sanatorium, which is to form part of the institution. It has also been suggested that some of the public squares in London should be covered in with glass for a like purpose, and I need not say how readily this plan could be carried out in Edinburgh.

All those interested in the health of the community, and in the successful treatment of the most common and fatal disease in this country, phthisis pulmonalis, must regard with great anxiety the question now agitating with regard to the permanency of the Crystal Palace. If unfortunately it should be removed, a great rational means of cure will be cut off from our resources, whereas, if allowed to remain, I cannot but regard it as the commencement of a new series of sanatory improvements, which will go far to mitigate many of the evils which the nature of this climate produces in pulmonary cases.

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

EXPERIMENTAL INVESTIGATION UPON THE MOTIONS OF THE BRAIN, AND THE CHANGES IN THE FULNESS OF THE VESSELS OF THE PIA MATER, AS OBSERVED WITHIN THE CLOSED SKULL. BY F. C. DONDERS, PROFESSOR OF PHYSIOLOGY IN THE UNIVERSITY OF UTRECHT. (Communicated by Dr Van der Byl.)

It is now well established by experiment, that in the opened or dilatable skull the brain presents a double rhythmical motion,—the one corresponding to the rhythm of respiration, the other to that of the heart's action. With every expiration the brain rises, it falls with the succeeding inspiration; and in this greater motion we can recognise every contraction of the heart by slighter ones synchronous with the systole and diastole. That these rhythmical motions corresponding to the acts of respiration take place within the cranium, is well enough seen in the large soft fontanelles of children; and that the mass of the brain itself is in motion, and presents a double movement, has been further determined by experiments on animals, and by the accidental exposure of the encephalon in man.

There is still diversity of opinion, *as to whether these motions of the brain exist in perfectly ossified undilatable skulls.* Ecker and Longet, who have been experimentally engaged with this subject, have come to very different conclusions,—Ecker believing in the existence of these motions, and Longet denying them. But we must not limit the question, to whether, in undilatable skulls, even a trace of motion be possible. It is far more important to know, what these forces producing extensive motions in the opened skull perform in the unopened or closed skull. For these forces certainly are not destroyed, even if they could not move the cerebral masses; and were they only to produce a trace of motion, they would still certainly act in some other way, because every force must have its equivalent.

The experimental investigation of the motions of the brain, which Longet unjustly disregards, is the source of our knowledge on this subject. It must, therefore, disclose to us what takes place in the unopened skull.

I. *The Motions of the Brain in a Dilatable or Opened Skull.*—On this subject the author communicates the following experiments :—

Experiment 1. A square piece of the skull was removed from a rabbit with a saw, and the dura mater incised. The trachea was exposed, and opened by a transverse incision between the cartilages. A glass tube, which terminated externally in two branches, was fixed in the trachea. To the one branch was attached a manometer filled with water, while the other remained open, so that the rabbit could breathe quietly. The respiratory motions of the brain were distinct, and answered to those of the column of water in the manometer. In proportion as the open branch of the tube was more or less covered with the finger, did the motions of the brain, and those of the fluid in the manometer, increase. Blowing in air into the open tube while covering the manometer, was followed by greater risings of the brain. By the small reflected images on the surface of the brain, the movements of the arteries or of the pulse were noticed,—which Ecker did not observe in the rabbit. The contractions and dilatations of the longitudinal sinus with inspiration and expiration were likewise distinctly visible.

Hence, in rabbits, also, two kinds of motion are observable in the brain when

the skull is opened, and these can be strengthened when the pressure of inspiration and expiration is increased.

Experiment 2. A sheep, suffering from cœnurus cerebralis, was trepanned in the right parietal bone, the opening enlarged, and the dura mater at once removed. The respiratory movements were not at first to be noticed. After a short time the cyst of the cœnurus appeared slightly raised with an expiration,—and with every succeeding expiration a larger portion became visible, while its size diminished with every inspiration. In this swelling and sinking of the cyst filled with fluid, we distinctly see the amount of the motion of the brain. These motions were proportional to the motions in the manometer, as in the first experiment. The closing of the tube through which the sheep breathed was followed by stronger motions. At length the cyst escaped. The motions of the brain were now very distinct, as well those of respiration as those of the pulse. To the open branch of the tube the stomach-pump (of Weiss) was now attached, to pump air into the lungs. By this operation the brain rose, but fell again as soon as the air was driven out.

From this experiment we learn that the motions are not always to be observed immediately after the opening of the skull,—that they become particularly distinct when, from some cause or other, the brain is somewhat pressed in and deep seated, as in the case after the escape of the cyst,—and that, by increased pressure in the lungs, the brain rises again. In subsequent experiments on rabbits, the motions of the brain did not appear until from fifteen seconds to a minute after the opening of the skull.

Experiment 3. A cat was tied by its four legs, and then chloroformed; a piece of bone was removed from the skull by means of a saw, without wounding the dura mater. The glass tube was fixed in the trachea, and connected as usual with the manometer, and, towards the end of the experiment, with the stomach-pump. The respiratory motions corresponding with those of the manometer were very distinctly observed upon the dura mater when the respiration was vigorous. Pumping air into the lungs produced rising of the brain, and with it of the dura mater. When it had reached a certain point the dura mater was opened to see if any fluid would escape; this did not, however, take place. By withdrawing the air from the lungs, the brain was seen to fall considerably.

This experiment teaches us that the motions may also be observed without the dura mater being opened; that with the rising of the brain the quantity of fluid on the superior surface does not increase considerably; that the brain rises through increased pressure produced by inflation of the lungs, and falls through the diminished pressure produced by pumping the air out of the lungs.

Experiment 4. In a cat which was not chloroformed, the skull was opened as usual,—excessive bleeding arose from a wounded sinus, which, though arrested a few times by cold water, was always renewed by the movements of the cat; so that precisely when the ligamentum obturatorium was to be opened, in order to remove the cerebro-spinal fluid, the motions of the brain were no longer visible; the brain was sunk below the level of the cranial bones, and the dura mater was depressed. A slight motion was still observed in a coagulum in the neighbourhood of the wounded sinus, along with inspiration and expiration. During the bleeding the blood could be distinctly seen to issue from the wounded sinus with great force, and in greater quantity on expiration. Air entered the vessel with inspiration, and the animal soon died. A great many air-bubbles were found in the vena cava superior, in the right heart, and in the pulmonary artery.

This experiment shows, among other things, that the motions cease after great loss of blood.

Experiment 5. The skull of a rabbit was opened as usual. The respiratory motions were very apparent. The ligamentum obturatorium was next exposed, and the cerebro-spinal fluid withdrawn by a small syringe. After the withdrawal of this fluid, the motions of the brain were not visible, whereas the motions of the fluid in the spinal canal still continued; and at the place where the ligament was opened, the fluid could be distinctly seen ascending and descending, or rather

returning into the spinal canal, and reappearing proportionally to the inspiration and expiration.

This experiment teaches us, that the motions of the brain cease as soon as the fluid in the cranial cavity is removed, either in one way or another, or as in the above example by the syringe.

The total results of the experiments performed in this investigation are briefly as follows:—

a. The respiratory motions of the brain in an open skull are especially dependent on the decrease and increase of the venous blood, as well in the cavity of the cranium as in the spinal canal, and most probably, also, somewhat in the very substance of the brain and spinal marrow itself.

b. The alternate higher and lower pressure of the blood in the arteries on expiration must cause the blood somewhat to increase and decrease, and the influence of this on the motions of the brain, even when much slighter, cannot be denied.

c. The cerebro-spinal fluid, by which the pressure is uniformly propagated, and the sum of the vascular dilatation concentrated on the now resistent parts, is a necessary condition for the existence of the motions of the brain.

II.—*The Motions of the Brain examined in an unopened undilatable Skull.*—The long dispute, whether the motions observed in the dilatable or opened skull also took place in the unopened skull, might have been decided by an exact knowledge of the mechanism of these motions. Professor Donders has never hesitated to deny the existence of these motions. When it is considered that the cranial cavity and spinal canal are filled,—that the quantity of blood cannot notably change in a moment,—that, further, the modifications of the pressure of the blood in the vessels produced by respiration may be said to be equal, and are produced simultaneously in all vessels, then it would appear that the respiratory motions are impossible, and may be denied on good grounds. Ecker, it is true, believes himself to have proved, by experiment, the rhythmical motions in the unopened skull; but the experiment performed by him, viz., by a small trepanned opening to see through the dura mater, did not give the author the desired results, though frequently repeated. And the possibility, by such a small opening, without opening the dura mater, to judge of the existence or non-existence of the motions of the brain, appears to him very questionable. Besides which, this view contradicts Ecker's theory of the mechanism of the motions; because, according to this theory, the quantity of cerebro-spinal fluid mounting from the vertebral canal into the cranial cavity during expiration, there should be found on the surface of the brain a thin layer of fluid. Longet's proofs, and Bourgougnon's experiments (*Diss. Inaug., Paris*, 1839), adduced to establish the contrary, only show, however, what requires no proof, viz., that the unopened skull remains filled, and that no vacant space exists, or is produced during inspiration. After many vain endeavours to decide the matter experimentally, the author arrived at the following satisfactory experiment.

Experiment 6. In a full-grown rabbit, a square piece of bone, measuring one centimetre in length, and somewhat less in breadth, was removed from the centre of the skull. The bleeding was slight, and soon ceased,—the dura mater was removed from each side of the superior longitudinal sinus, by which means the vessels of the pia mater, especially the venous twigs which empty themselves upon both sides of the longitudinal sinus, became distinctly visible. And shortly after the operation, when the brain had somewhat fallen, both the respiratory and circulatory motions of the brain became also visible. A little water was now dropped on the depressed brain, and a glass, half a millimetre thick, which fitted tolerably well in the opening, was wedged in, in such a manner, that only fluid, and not air, could remain between the glass and the brain, covered by the pia mater and arachnoid. The borders of the aperture were next well wiped with a dry linen cloth, and covered with powder of gum arabic, which adhered to the more or less moist surface, and was also applied over the edges of the glass, and

especially in the small spaces left here and there between the glass-plate and the aperture in the skull. Immediately afterwards the superabundant gum-powder was blown off, and that which remained was dried by means of a hot iron kept at a convenient distance. Upon the rough dry surface of gum arabic, collodion was now applied, thus still more intimately uniting the glass with the surrounding bony surface. The brain could be distinctly seen through the small glass plate, but not a trace of motion was observed, nor was any motion perceptible, even when the pressure of inspiration and expiration was increased by repeatedly closing the mouth and nose. At length, on the continuance of these attempts at suffocation, the glass suddenly separated during a powerful effort at expiration, and from this moment very strong double rhythmical motions were visible. Hence, no motion whatever was to be discovered, even by a pocket lens, as long as the skull remained closed, though with the strongest pressure against its internal surface. But motion immediately became distinct when the glass was suddenly separated from the above-named pressure.[1]

Pathological anatomy appears to throw doubt upon the motion of the brain and of the cerebro-spinal fluid during life. In autopsies, blood and purulent exudations are not found to be so intimately mixed with the cerebro-spinal fluid as would be the case did such continual motion exist, and as Magendie (*Rech. Physiol. et Clin. sur le Liquide Céphalo-Rachidien. Paris* 1842) suspected, although it must be borne in mind, that a gradual mixing with the neighbouring fluid can take place without motion. The final result of these experiments is, *that no motions of the brain are observed in the unopened and undilatable skull when a portion of the cranium is replaced by transparent glass. The motions cannot exist in the undilatable skull, because the cranial cavity and spinal canal are continually filled; because the quantity of blood cannot thus in a moment be notably changed; and because the modifications of the pressure of the blood in the vessels during life may be said to be equal and simultaneous in the arteries and veins.*

III.—*The effects of the motive powers in the unopened skull.*—This question is silently passed over by most authors. A greater or less compression of the brain alone is supposed by some to occur during the systole of the heart and expiration, and Longet certainly advances this view. According to him the brain must alternately increase and diminish in density or in bulk, as he terms it. But he admits that this compression is immeasurably small, and is, of itself, of no significance whatever. Distilled water is by the pressure of one atmosphere compressed less than $\frac{1}{20,000}$th. (Oersted-Colladon-Sturm.) How insignificant, then, must be the slight alternation of pressure in the substance of the brain so entirely saturated with water. Dr Berlin has, in his inaugural dissertation, pointed out, that increased or diminished pressure in the trunks of the vessels, propagates itself only in part along the smaller branches, and in still smaller amount along the capillaries, whereas the full increase and decrease of pressure are communicated to the cerebro-spinal fluid through the vessels which cannot become dilated. What is the necessary consequence of this? During increased pressure of the spinal fluid there is absorption—during diminished pressure there is effusion. If respiration exercises an uniform influence on the

[1] The glass was forthwith replaced with greater care than before, and with the almost incredible result, that on the eleventh day after the operation it still remained perfectly fixed, so that the examination could be repeated at any moment. No motions of the brain could be observed, even with a magnifying power of forty-five diameters, though the animal was cheerful and in good health. This rabbit lived for about four months, and died from neglect and want of food. It may be well, perhaps, to observe, that slightly convex glasses were found to be best fitted for the purpose. The glass should be inserted soon after the opening is made, since the brain is liable to protrude if the time be protracted.

brain and spinal cord, then during expiration the blood will increase and the fluid diminish in both; and during inspiration the reverse will take place. We recognise here a powerful cause promoting the interchange between the cerebro-spinal fluid and blood—between blood and the nutrient fluid of the brain—a powerful cause, therefore, of the interchange of material, and of the energetic operations of the brain. At every beat of the heart, at every expiration especcially, more ingredients are returned from the nutrient fluid of the brain into the blood of its capillaries than are received; at every dilatation of the heart, at every inspiration, more ingredients again pass from the blood to the nutrient fluid. This interchange goes on continually, and is strongest in the entirely undilatable skulls, and consequently after childhood, because the changes in pressure then only partially resolve themselves in motion.

It may perhaps be supposed that every inspiration and expiration, and especially every systole lasts too short a time to give rise to an appreciable exosmosis and endosmosis. We must, however, consider the great extent of surface of the combined capillaries, and somewhat larger vessels, along which exosmosis and endosmosis occur. Zimmerman's experiments (*Dynamik des Aderlasses Arch. F. Phys. Heilk. B.* iv. *s.* 65. *u.* 165) show that the withdrawal of a few ounces of blood, by which the pressure diminishes in all parts of the body, is followed almost in a few seconds by notable increase of the watery contents of the blood, by reabsorption of the nutrient fluid.

If the fluid on the occurrence of the increased pressure could immediately escape freely (pass into the blood for example), then this transposition would be the only consequence of the increased pressure. At present only a portion of the power is used for the promotion of interchange between the blood by nutrient fluid; another portion presses on all parts and, as we have seen, compresses them only in the slightest degree. Where does this portion of power remain? It is not destroyed. Heat is here, as in other places, the consequence of the alternate pressure. And here we become acquainted with one of the circumstances in which functional power is converted into heat—a conversion which arises under so many circumstances in the animal organism, as for example throughout the blood-vessels, where through friction a portion of the blood's pressure is lost.

But the power which produces motion in the open skull, also promotes the venous circulation in the undilatable skull. If, then, as we have seen, that the increased pressure of the blood in the arteries is communicated to the cerebro-spinal fluid, then this pressure propagating itself uniformly to the veins and sinuses, the return of the blood to the cranial cavity is thereby impeded. Only a very small portion of the increased pressure of the arterial blood is communicated through the capillaries and veins. The friction, especially in the small vessels, for the most part, stops the power of pressure, and converts it into heat; but the entire increase of pressure is now communicated through the cerebro-spinal fluid to the veins in the ventricles of the brain; and the flow of venous blood is promoted just at the moment that some impediment is afforded by the pressure of expiration.

If we accept the doctrine of Burrows, that a certain pressure is a necessary condition of the proper action of the brain, then it appears doubtful whether, as such, it can have any influence. And Prof. Donders thinks that he can satisfactorily explain how disordered functions of the brain are produced by disturbed changes of matter from varying pressure, inasmuch as, by quickly increased pressure, the nutrient fluid is absorbed by the vessels without being continually replaced, and by the rapidly diminished pressure much matter transudes from the blood; the product of the interchange of matter, however, only returning with difficulty from the nutrient fluid into the blood. It is not, therefore, strange that under both conditions, at first, phenomena of increased activity (delirium and convulsions), and afterwards of depression (coma-paralysis), should take place.

Our conclusion from these considerations is, that the forces which in the open skull produce motions, influence the changes of material, the production of heat, and the venous circulation; and that the functional disturbances produced by

changes in the pressure of the blood, have their foundations in the modifications which the changes of material by these means necessarily undergo.

IV.—*The changes in the quantity of the blood within the cranium by increased and diminished pressure of the blood.*—The generally received opinions that both during the increase and diminution of pressure, the quantity of the blood in the brain and spinal marrow varied, was confirmed by the author by direct experiment. It is true that the exceptional position of the brain and spinal cord in a close undilatable cavity, must impede this interchange in some degree. It does not, however, entirely arrest it. It is now to be considered as established, that, unless the brain is seen before death, we cannot, after death, judge of the greater or less congestion of blood. The increasing redness of the pia mater could be seen with the naked eye, while respiration was impeded by closing the mouth and nose ; and still more distinctly was this phenomenon observed by the use of the microscope, rendering the finer vessels visible, when the mouth and nose were closed for ten seconds. Two minutes always elapsed before the vascular congestion visibly diminished, and even a quarter of an hour later, the injection was still somewhat stronger than before the experiment.

When the rabbit was suspended with its head downwards, the congestion was likewise increased. Rapid bleeding produced very perceptible diminution of the blood in those vessels. Slower venesection in a certain degree operated less distinctly, because the loss is everywhere more or less modified by the longer continued absorption of the nutrient fluid. If the slow withdrawal be great, then the original quantity of blood cannot be replaced, and there is time enough to allow an increased transudation of the cerebro-spinal fluid—a necessary condition as we know for the increase of the blood in the cranial cavity.

We may, therefore, undoubtedly conclude, that whatever increases the pressure of the blood, and especially respiration, increases the fulness of the vessels in the pia mater within a very short time ; that bleeding, even when not so direct as in other parts of the body, also very rapidly diminishes the quantity of the blood in the pia mater.—*Nederlandeche Lancet, March and April* 1850.

[The experiments of Professor Donders add still further proof to the doctrine long taught in Edinburgh, namely, that the amount of fluid within the undilatable cranium must always remain the same, so long as its osseous walls are intact. In his opinion, the forces which are continually keeping up more or less pressure on the brain, produce absorption or effusion of fluid according to its amount. Supposing such changes to take place, the effect must undoubtedly be to keep the amount of fluid within the cranium always the same. The fact, also, that no motion really takes place in the undilatable skull is another proof of the correctness of the doctrine—a fact which was decidedly announced by John Reid as the result of reasoning on the subject, but which must now be considered as proved by the experimental researches of the author.

In section 4, it seems to us that Professor Donders does not reason with his usual acuteness, but has fallen into the error of Dr Burrows, who believed that because congestion was present in the pia mater, or certain other vessels, that therefore, the quantity of blood in the cranium was increased. But if the cranial cavity be completely filled, it must follow that it will hold no more fluid at one time than another, unless some equivalent amount of bulk be forced out to make room for the excess. Consequently, if some vessels be over distended with blood, others must be correspondingly compressed. It requires the weight of one atmosphere (15 pounds on the square inch), to compress water (not to speak of brain), even to the insignificant extent of $\frac{1}{20,000}$th part ; but at no time does the pressure exercised by the heart and respiration, ever amount to the weight of one atmosphere, and hence the brain may be considered as incompressible. It follows that redness in the meninges of the brain is no proof that the cranium contains more blood at one time than at another. J. H. B.]

PHYSIOLOGICAL EXPERIMENTS MADE ON CRIMINALS AFTER DECAPITATION. BY MM. DUVAL, ROCHARD, AND PETIT, OF BREST.

Circumstances, unhappily too frequent, have, during the last few years, enabled us to make upon the bodies of executed criminals certain experiments, which seem of sufficient interest to merit publication. We could hardly have been more favourably situated for profiting by such sad opportunities. The large number of criminals condemned for life and sent to labour in the galleys at Brest, render crimes and capital punishments of frequent occurrence there. The school of anatomy is only 260 paces distant from the scaffold, and in the course of our observations, there seldom elapsed more than five or six minutes after the head fell before the body was delivered to us. This particular is of great importance. Most of the phenomena which spontaneously succeed decapitation are so transitory, and become diminished in intensity so rapidly, that they must be caught as it were while on the wing (*il faut les saisir au vol*), and hence the extreme difficulty of conducting such experiments. However, thanks to the rapidity with which affairs are here conducted, we have always been enabled to observe for a few minutes the phenomena which we are about to describe; and if the time has been too short to enable us to analyse all their varieties, it has at least sufficed to enable us to appreciate them as a whole; for those phenomena which soon pass off—the spontaneous movements of the heart for example—have been witnessed so unequivocally, that neither we ourselves, nor our assistants who stood at a greater distance from the subjects, can entertain the slightest doubt of their occurrence.

Among the facts which we proceed to detail, a certain number suggest controverted questions in physiology. We have endeavoured to forget this, in order to approach our subject free from prejudice. We shall not even mention the opinions which our observations confirm or invalidate, but shall simply state what we ourselves and the naval surgeons present at our experiments have seen.

Our observations have in all amounted to six; but we shall only detail the two most recent, which are also the most complete; and, as the true order of succession of particulars might tend to confuse our ideas, we shall often abandon it for a physiological arrangement.

Observation 1.—François (Auguste) 36 years of age, condemned to the galleys for life, and sentenced to death for an attempt to murder one of the turnkeys, was guillotined in the square of the "bagne" on the 20th June 1850, at 3 p.m. He was of strong constititution and energetic character, mounted the scaffold calmly, and without resistance placed himself at the disposal of the executioner.

The head had been only five minutes separated when the body was brought to us. The first phenomenon which attracted our attention was the following:—

The divided extremities of the carotids rose by jerks above their plane of section and then fell back again; they were pushed outwards, undergoing a true elongation, and this perfectly regular movement cannot be compared to anything more similar than the motions of the principal artery of a limb to which a ligature has just been applied after an amputation. A small quantity of frothy vermilion-coloured blood escaped at each impulse from the open orifices of the vessels. Blood also issued, but not continuously, from the jugulars; it was also frothy but darker in colour than that ejected from the carotids.

No reflex movement could be excited by pinching or other modes of mechanical stimulation. In none of our observations have we ever been able to induce anything of the sort.

After a quarter of an hour spent in opening the abdomen, examining the chyliferous vessels, the motions of the intestines and the protrusion of the mucous membrane through wounds in their parietes, we opened the chest and saw the heart beating, and still enclosed in the pericardium; we hastened to open the membrane and the movements became quite distinct. The right auricle, especially its auricular appendage, presented energetic, sudden, perfectly regular

movements, occurring forty-three times in a minute, and continuing for an hour and a quarter, even after the liver, stomach, intestines, diaphragm and lungs were removed, finally ceasing when the cavities of the heart itself were opened. The movement was especially characterised by the erection or rising of the auricle, its separating itself from the aorta, which it left uncovered, and its then falling back to its original place. It will be described more fully in the second observation, for in both cases it was identical.

The mass of the ventricles also presented a species of undulating tremor or puckering which soon became imperceptible. We then had recourse to galvanism, and under the influence of a pile of 60 pairs, of 4 centimetres in diameter, the contractions of the auricle became accelerated, and the tremor of the ventricles was renewed. After varying the mode of applying the stimulus, we determined to open the cavities. The right auricle contained a clot of dark blood of the size of a nut, swimming in a large quantity of fluid blood of the same colour.

Seven or eight minutes after the body was brought to us, and before the chest was opened, one of us applied galvanism to the spinal marrow at the point of section, with the effect of inducing movements of inspiration and expiration, accompanied by very raucous sounds produced in the larynx; while these were heard, the inferior vocal cords approximated from before backwards, leaving a mere linear chink interposed, while the space between the arytænoid cartilages was also sensibly diminished. These phenomena were easily observed, the larynx being left intact by the blade of the guillotine, which had passed through the hyo-thyroid space.

After opening the thorax we applied galvanism to the intercostal muscles, and saw the ribs approach each other with force, narrowing the intercostal spaces, more especially in front. The effect produced was the same whether the internal or external intercostal muscles were stimulated, either where they are superimposed, or where the internal layer exists alone. On account of the previous removal of the sternum, we attach little importance to this observation.

The stomach, full of food, was detached by two incisions made beyond its orifices. No ligature was applied; and although the viscus seemed to react forcibly upon its contents, so strongly were the cardia and pylorus contracted that nothing escaped; but scarcely had an incision been made in its upper part when it was placed on a horizontal plane on a level with its lesser curvature, when the lips of this artificial opening fell backwards, and the paste-like contents of the stomach escaped slowly as if under the influence of concentric and continuous pressure. It was then laid open from end to end, and contracted upon itself with force sufficient to resist strong traction. It seemed to shorten in our hands; and on its smooth internal surface ridges more or less prominent appeared, and by their final approximation obliterated the lozenge-shaped intervals at first left between them.

The bile was of a very dark green colour, and syrupy consistence. The gall bladder and bile ducts were quite unaffected by the action of galvanism.

It was the same with the spleen; this viscus, wrinkled, and as it were, exsanguine, did not seem to exhibit the slightest contractile movement, although it was attentively watched for. In these experiments, it is true, we used no electromagnetic apparatus, having nothing of the sort at our disposal at the time. We were more successful in our experiments on the ureter, which we saw contracting under the influence of galvanism in the most distinct manner. When first exposed, it appeared large, thin, and flattened, as it usually is seen to be in the dead body. A few moments after the application of galvanism it appeared to contract itself with that peculiar deliberation observed in muscles of organic life, and in a few seconds its appearance was greatly altered (*il etait méconnaissable*); its diameter had diminished by a full half; it felt hard; had become completely cylindrical, and presented a likeness to the vas deferens, which struck all those who witnessed the experiment, and which contrasted remarkably with the appearance of the opposite ureter which had not been experimented on.

The vesiculae seminales and vasa deferentia seemed insensible to the action

of the pile. The semen contained in the receptacles was semi-liquid, &c. When subjected to the microscope an hour and a-half after death myriads of spermatozoa, some of them still moving, were observed. On the other hand, in all the subjects previously examined, the spermatozoa were quite motionless, which might possibly depend upon our previous prolonged persistence in the galvanic experiments.

Observation 2.—Ennesailles (Jean Pierre) aged 23, convict at the galleys for life, and condemned to death for attempting to murder the tailor of the prison, was executed on the 26th March 1851, at three o clock p.m. He marched to the scaffold firmly, and resigned himself calmly to his fate. Six minutes only elapsed ere his body crossed the threshold of the amphitheatre.

In this instance, also, the blood jetted in considerable quantity from the arterial trunks of the neck in feeble streams, and likewise flowed from the veins. One of us who firmly compressed the divided extremities of the arteries, perceived very distinctly the pulsation of the carotids.

The thorax was opened immediately and rapidly. The six upper ribs were divided about their cartilages with a strong scalpel. The sternum was divided transversely at the level of the lower edge of the sixth intercostal space, and turned upwards on the neck. The contractions of the heart were easily distinguished through the pericardium which contained some serous fluid. This membrane was opened, and the heart appeared, bathed in a very limpid fluid, of a light orange colour, and measuring in amount about 40 grammes. Our special object being to observe the heart *in situ*, and in conditions as nearly normal as possible, we avoided touching it directly or indirectly, lest its relations should be disturbed. We shall consequently only describe the parts which were exposed to view in this position, *i.e.*, the right auricle, the two ventricles, and especially the right ventricle.

1st. *Right Auricle.* If we suppose the auricle in a state of rest, *i. e.*, lying upon and embracing the aorta, its movements may be described as follows:—After a period of rest, always quite appreciable, the appendage *raised* itself suddenly, and was directed outwards from the axis of the heart, describing at least a quarter of a circle, and leaving the aorta uncovered; it then fell suddenly back to its original position. During the erection, the entire auricle seemed distended, as if in consequence of the afflux of fluid, its appendage was elongated, and the fringe which surrounded it spread out like the fingers of the opened hand, and then fell back again to a closed position. This double and perfectly rythmical movement took place forty-eight times in the first minute, but soon became retarded, and in the fifth minute of the experiment only occurred seven times. These two elements of the same phenomenon—the distension of the auricle and erection of its appendage—were not inseparable in our experiments; for we afterwards, by means of galvanism, succeeded in exciting the alternate contraction and dilatation of the cavity without the erection, the appendage merely flapping against the aorta without rising from it to any notable extent, as we shall presently explain.

2d. *Ventricles.* If we could not distinguish clearly the dilatation of the ventricles, their oscillatory or torsion movements, we at least saw, and *most distinctly*, the two ventricles contract upon themselves, shutting, in short, and closing towards each other by a concentric motion of the anterior sulcus, which seemed to serve as the axis of motion. They contracted synchronously and rythmically.

The movements, considered in their *ensemble*, proceeded from the auricle, and were propagated in an instant from the base to the apex of the heart.

The spontaneous contractions of the ventricles ceased much more promptly than those of the auricle; but after their cessation there was still to be distinguished, with each beat of the latter, a sort of starting or tremor of the entire ventricular mass. This inequality in the persistence of the contractility of these parts was far less remarkable in this than in our former subject, in which the *ultimum moriens* was so remarkably illustrated. When the auricular move-

ments became slower, and those of the ventricles ceased, we had recourse to galvanism, and the same phenomena were renewed under the influence of a pile of 50 pairs, each, 4 centimetres in diameter. Its effects were not, however, immediate. Thirty seconds elapsed between their reappearance and the application of the artificial stimulation. We successively placed the poles of the battery as follows:—both on the auricle; one on the auricle, the other on the right ventricle; one on the right ventricle, the other on the divided extremity of the spinal column; finally, both upon the columnæ carneæ of the right side of the heart, after opening its cavities.

In the first case, the palpitations of the auricle became so rapid and tumultuous that at first they could not be counted, but they soon became regular, and we were able to reckon 144 in a minute. They at this time assumed the flapping character above alluded to. In the second case, the artificial movements were identical with those which occurred spontaneously, with this difference, that the erection of the auricular appendage was not completely effected. The contractions of the ventricles only lasted for about a minute, and were then succeeded by the undulating tremor. After the direct application of galvanism to the muscular substance of the heart had ceased to induce any effect, one of the poles was applied to the divided spinal marrow, and this new mode of experimenting was successful in re-exciting the contractions of the auricle, and inducing faint undulations in the ventricles. When we experimented on the columnæ carneæ, they were seen to contract. In the ventricle, the columnæ of the first order became thicker and shorter, drawing down by means of the chordæ tendineæ the corresponding portions of the tricuspid valve. When the poles were applied at the orifice of the venæ cavæ, at the cross of the aorta, and on the divided extremities of the carotids, they produced no effect.

To complete our remarks on the circulatory system, we may add that there existed a considerable quantity of blood in the veins, especially in the cavæ. We were able to cause its reflux from the external and internal iliac veins into the trunk of the inferior cava. The blood continued fluid, with the exception of a small black fusiform clot, about 3 centimetres in length, situated in the right auricle, and less voluminous than that mentioned in the narrative of our former observation. The arteries and left heart also contained fluid blood, but in much smaller quantity; we, however, could force it back from the external and internal iliacs to the trunk of the aorta. These facts, together with the hemorrhage at first observed, favour the supposition that the blood continued to circulate during the first few instants after decapitation.

We have nothing to say regarding the larynx. The blade had, in this instance, struck below the glottis, dividing the lower part of the thyroid cartilage and a portion of the cricoid by a clean cut, directed from below and behind, upwards and forwards.

Galvanism, applied to the intercostal muscles, both external and internal, produced the same effects as were formerly observed, *i. e.*, forcible approximation of the ribs; but, as in this subject, the lower ribs still adhered to the portion of the sternum, which we purposely left in position, our results were rather more satisfactory. We saw *these* ribs approach each other, but with less force,—and this observation was made whatever muscular plane was acted on. The great bronchi and their smallest ramifications were alike insensible to the action of the pile; half-an-hour, however, had elapsed before they were subjected to experiment. We did not attempt to excite the contraction of the diaphragm by galvanising the phrenic nerve, having frequently performed this experiment before, and always with success. The lungs were gorged with blood; and next day presented a true hypostatic congestion, occupying a quarter of their bulk behind. The pulmonary tissue presented, at this part, a dark colour, and when cut into, permitted the escape of dark blood.

On opening the abdomen, when the intestines were exposed to the air, an undulatory movement was perceived, in which an attentive eye discovered peristaltic and antiperistaltic motions going on without order, alternations of rest and dis-

turbance, sometimes at one part, sometimes at another, the whole mass of the intestines seemed crawling (*parâit grouiller*) beneath our eyes. Incisions made in different directions through the outer coats of the intestines, were followed by protrusion of the mucous membrane, which, when the wounds were made in a transverse direction, spread over the other coats for a space of eight or ten millimetres; and when they were longitudinal and of small extent, stuck out in a prominent manner (*faisant une saillie en cul de poule des plus prononcées*). We had observed identical phenomena in observation 1. The solitary follicles were highly developed in the last yard of the ilium, simulating a true "psorenterite." The stomach, full of food, was of a greyish rose tint, and appeared granular to the eye and touch. The distinctions between its splenic and pyloric extremities were extremely well marked. The bile presented nothing remarkable. The gall-bladder and ducts were insensible to the galvanic stimulus; as were likewise the trunk of the venæ portæ and the spleen, which was small, exsanguine, and wrinkled; we isolated it with care by means of glass plates, and obtained no result, on applying the poles of a battery of 50 pairs of plates.

It was not the same with the ureter, in which in this instance likewise, we succeeded in inducing most evident contractions. We saw it, after a few seconds, contract itself, become hard and cylindrical, presenting the same resemblance to the vas deferens which we had formerly observed; and when the application of galvanism was suspended, returning to its original flaccidity, size, and thinness. A fresh application of galvanism caused it to contract again; pinching it at any point in its course caused contraction of its calibre, but to a lesser degree than the galvanic pile. Galvanism did not affect the vasa deferentia. We thought we could distinguish feeble contractions of the seminal vesicles, but do not state this positively, desiring, as we do, to pass over in silence, all observations on which we have not decisive evidence to offer. . . . The spermatozoa, observed three quarters of an hour after this subject's death, were motionless and deformed.

Among the most distinct results of our experiments may be placed the contractions of the iris, excited by galvanising within the cranium, the *motor oculi* nerve. While we were opening the chest, one of our assistants removed the calvarium and encephalon, exposing the third pair of nerves where it enters the cavernous sinus. This operation was so rapidly performed, that our galvanic experiment commenced twelve or thirteen minutes after the head was severed from the body. Its effects were instantaneous. The pupil, which was widely dilated, contracted instantly to half its size. This contraction was as rapid as that which is excited by the stimulus of light, and was followed by a dilatation almost as rapid, when the exciting agent was removed. Fresh applications of galvanism renewed the phenomena, but less decidedly; we obtained in all six successive contractions and alternate dilatations of the pupil. We may remark *en passant*, that in this subject, as in all others on which our observations have been made, *the pupil was found fully dilated a few moments after death*, but *in two days afterwards it was extremely contracted*. The cadaveric rigidity came on very rapidly; in endeavouring to overcome it, we broke one of the patellæ across.

We need make no remarks on the effects of galvanism applied to the facial nerve, as they presented nothing of interest. The duct of Steno was completely unaffected by the galvanism. The nervous centres, and spinal marrow especially, were of remarkably firm consistence.

Such are the principal facts to which we have thought it right to call attention. There are, without doubt, lacunæ in our observations, and the subject demands re-investigation. We ardently hope that similar opportunities may not present themselves; but should they occur, we shall endeavour to conquer our repugnance for such investigations, by reflecting that they are fully justified by their scientific object.—(*Translated from the Gazette Médicale de Paris*, 12 *Juillet* 1851.)

EFFECTS OF VARIOUS THERAPEUTICAL AGENTS ON THE ANIMAL HEAT.
BY MM. AUG. DUMERIL, DEMARQUAY, AND LECOINTE.

The authors, in their researches, propose to examine the effects on the animal heat of various agents, introduced into the economy in successively increased doses. The experiments were made in the laboratory of M. Flourens, at the Jardin des Plantes. The animals experimented on were full-grown dogs. As far as possible, they were kept fasting from the evening before till the evening of the day on which the experiment was made. The duration of the experiments —or rather the time during which it was thought requisite to keep the animals under observation—varied from six to twelve hours. The medicinal substances were introduced in three ways,—sometimes, and most frequently, by the stomach; sometimes by the veins; and sometimes by the cellular tissue. Before any experiment was made, it was ascertained that the ligature of the œsophagus, intended to prevent vomiting, did not produce any effect on the temperature of the animal during a number of hours equal to the duration of the experiment with the medicines.

The temperatures were taken with accurate centigrade thermometers, capable of being substituted for each other in case of accident. All the temperatures were taken by the rectum, the thermometer being introduced to the same depth, and being left in the gut, until it remained at a fixed point for five or six minutes.

By taking these precautions, and by repeating in some instances the experiments as often as ten or eleven times with the same substance, the authors have been able to assure themselves that the action of any given remedy is always the same as regards the animal heat, whether administered by the stomach, the veins, or the cellular tissue.

All the substances employed were prepared by M. Mialhe. They used sometimes powder or extracts, sometimes alkaloids or salts. The latter preparations were the only ones ever injected into the veins. The substances were dissolved or suspended in water, oil, or mucilage; the quantity of vehicle varying from one to four ounces, according as it was to be introduced into the veins or the stomach.

The temperature of the atmosphere in which the animals were placed was from 53° to 59° Fahr., and the temperature of the liquids containing the drugs was from 68° to 95° Fahr.

The results now published are confined to those which bear upon the animal heat. To give a systematic form to their series of experiments, they have adopted Trousseau and Pidoux's classification; they have omitted those remedies of each class which are not sufficiently energetic to produce some decided effect on the animal heat.

Excitants.—This class of remedies has been examined carefully; only the more powerful have been experimented with. These are,—cantharides, cinnamon, ergot, acetate of ammonia, phosphorus, and strychnia. As a general result, it may be stated, that all these agents have produced an elevation of temperature varying from a few tenths of a degree to several degrees.

Cantharides.—This drug was experimented with four times, and given in doses of 1¼ grains to 3 grains and 6 grains. At the dose of 1¼ grains the thermometer rose 2°. 1 (centig.) in a period of six hours, during which the temperature was noted at intervals of two hours. In three other experiments made in the same manner, and with equal care, in which the doses were on two occasions 3 grains and on one occasion 4 grains, there was again an augmentation of temperature which with the 3 grains approached to 2°, but with the 6 grains did not exceed 1°. It will be seen in the course of the experiments that cantharides is not the only substance which gives different, sometimes even opposite, results with varied doses.

Cinnamon.—Three experiments were made with this substance. It was given

in decoction twice in the dose of 7½ drachms, and once in 11 drachms. The animals were put under experiment between 10 and 11 A.M., and the temperature was observed for the last time at 11 P.M. In the dose of 7½ drachms in two animals, the cinnamon produced an elevation of 1°. 7; whilst in the dose of 11 drachms the rise was 2°. 7. Here, then, the increase of temperature was directly proportioned to the dose, and the action of the drug persisted in till the twelfth hour of the experiment.

Ergot.—This substance, whose special actions on the uterus are so well known, was given only once in the dose of a drachm; and in a period of five hours, during which the temperature was observed several times, there was ascertained to be a rise of 0°. 8.

Acetate of Ammonia.—Six experiments were made with this therapeutic agent. Five times it was introduced by the stomach, and once by the veins. By the stomach, in doses of 1¼ drachms, 2½, 5, and 5½ drachms, the temperature was always raised; and the same thing occurred when it was injected into the veins. The rise of temperature, however, varied. Thus 1¼ drachms thrown into the veins, raised the heat 0°. 8; 1¼ and 2½ drachms by the stomach gave as results 1° and 1°. 3. The dose was then increased in the ratios stated above; but the increase of temperature did not exceed that by the first doses.

Sulphate of Quinine.—It was introduced twice into the stomach in doses of 15 and 30 grains. In these two experiments the final results were increase of temperature, which varied from 1°. 5 to 2°. 2. But a remarkable phenomenon was, that, at the beginning of the experiment, the temperature fell during the first two hours some tenths of a degree.

Phosphorus.—Six experiments were made. In doses of $\frac{3}{10}$ths of a grain, and of ¾ths of a grain, there was a constant and successive augmentation,—1°. 7 in the first, and of 2°. 2 in the second;—whilst in the four last dogs to which the drug was given in doses of 1½ grains, 2¼ grains, and 3 grains, there was a constant lowering of the temperature, though to an inconsiderable amount; for it never was to a greater extent than 0°. 2, except in the case of one animal, which died in fifty minutes. Thus phosphorus in small doses raises the animal temperature very manifestly, whilst in large doses it slightly lowers it.

Sulphate of Strychnia.—Four experiments were made with this substance. Twice it was introduced into the stomach, and twice into the veins. Only slight results were obtained; but the temperature was slightly elevated. The animals, however, always died rapidly under the influence of this agent.—*Gazette des Hôpitaux*, April 5, 1851.

SURGERY.

CASE OF OBTURATOR OR THYROIDAL HERNIA SUCCESSFULLY RELIEVED BY OPERATION. BY MR HENRY OBRE.

(*Communicated to the Royal Medical and Chirurgical Society by Professor Erichsen.*)

After commenting on the extreme rarity of this form of hernia, and stating that he had been unable to find any record of its having been detected and relieved by operation during life, the author relates a case in which he operated successfully. The patient, a female, aged 55, the mother of a large family, was seized with symptoms which led her medical attendant (Mr Gardener) to believe that she was suffering from rupture. She denied that this was the case, and a careful examination convinced Mr Gardener that there was no hernia in the usual situations of that disorder. A little below the femoral region on the right side, however, he detected a degree of hardness resembling a small gland, and deeply seated; with some general fulness about the part. The author saw this patient on the fourth day after the symptoms had begun; at this time she was suffering extreme abdominal pain in the umbilical region; during the previous twelve hours her vomiting had been stercoraceous and incessant; the countenance pale and contracted; voice faltering; pulse weak, small, and intermitting—in short,

all the symptoms of pending dissolution from strangulated intestine were present. On careful examination nothing could be detected but a slight degree of fulness in Scarpa's triangle on the right side; that on the opposite side being well marked; on using firm pressure with the ends of the fingers over the neighbourhood of the femoral artery, and a little below the saphenous opening, a distinct hardness was to be felt, slight in extent, but giving the impression as if the sheath of the vessels was being pressed on. The state of the patient was such as to induce the author to propose to make an incision into the upper part of the thigh, down to the hard structure, in the hope that he might find confined intestine low in the femoral canal. He made a straight incision into Scarpa's triangle, as in the operation for tying the common femoral artery; beginning about three inches below Poupart's ligament. When the cribriform fascia was opened, and the saphenous opening exposed, no hernial sac was found, but the hardened structure could be felt lying deep to the inside of this opening. The dissection was with some difficulty continued downwards; the fascia lata was divided, and the pectinæus muscle exposed. The fibres of this muscle were divided transversely for about an inch and a half or two inches, and a hernial sac was exposed, which rose up into the wound to the size of a pigeon's egg. The finger being passed along the sac, entered the obturator opening. The sac was opened, and the intestine was found to be a portion of the small gut, blue and congested. The opening through which it passed did not tightly enclose its neck, but it was considered prudent slightly to divide the edge. In doing this the saphena vein was wounded, and it was necessary to apply a ligature to its upper part. This was the only ligature required. After the operation, no medicine was given; in the course of the day the bowels acted three times, and in the course of a few days the patient had quite recovered.—*Dublin Medical Press*, July 16, 1851.

A CASE OF OBSTRUCTION OF THE COLON RELIEVED BY AN OPERATION PERFORMED AT THE GROIN. BY MR JAMES LUKE.

The subject of this report was a man, aged sixty, who, on the 16th December 1850, first complained to the author of feeling generally unwell. He had no pain, but his countenance was depressed, his eyes sallow, and his tongue coated. The bowels were confined, and latterly medicines had acted with difficulty on them. An aperient was ordered, and on the following day he passed a small lumpy motion, but without relief to the symptoms. Castor oil was ordered, but after a time was rejected by vomiting. On the 18th there was no relief from the bowels, and he vomited everything he took. From this time he progressively got worse, in spite of all the means resorted to for his relief. He complained of pain chiefly about the region of the cœcum. The transverse arch of the colon could be felt distended and tympanitic. A careful observation of the case had led the author to believe that there was obstruction of the bowel about the sigmoid flexure of the colon, and it was resolved, as a last resource, to operate upon the patient. The operation was performed on the 23d. Not thinking it prudent to assume that the conclusion respecting the seat of the obstruction was certainly correct, the author determined to adopt that operation which would give him some opportunity of extending his search, provided he did not find the obstruction at the point where it was supposed to be. He, therefore, opened the abdominal parietes near the groin, by an incision four inches in length, a little to the outside of the course of the epigastric artery, the lower extremity of which incision terminated a little above Poupart's ligament. The peritoneum was opened to the extent of about two inches. On passing the finger down the surface of the intestine, which now protruded, a diseased mass could be felt, which appeared to encircle the intestine. The bowel was then opened above this part, a large quantity of feculent matter came away, and the patient expressed himself as relieved. On now passing the finger into the bowel it was found to be impervious about two inches below the aperture.

After the operation the recovery of the patient was rapid. On the second day fæces passed per anum, and continued to do so for more than a month, when

their passage through the natural opening ceased; it was again partially restored, but from this time the greater part of the fæces passed by the wound. It is closed by a well-fitted pad, and he has been enabled since to pursue his ordinary occupation almost without interruption.

The author then proceeded to remark on the danger of protracted delay in attempting to relieve such cases, a delay which is, however, to a great extent rendered necessary by the difficulties of diagnosis. The distension of the colon, and the evidence afforded by the proper introduction of the long tube, are pointed out as the two means of diagnosis on which reliance may be generally placed for the purpose of determining the seat of obstruction when it is situated at the lower part of the colon.

The advantages of the operations of Amussat and Littre are then compared, and the author, while admitting the advantage gained by operating in the loins, as proposed by the former, of not opening the peritoneal cavity, yet thinks that the operation in the loin offers certain advantages which render it in many cases preferable. By the operation in the loins nothing more could be done than opening the intestine; but this might in some cases be improper, as where obstructions were produced by fibrous bands overlying the intestine, or by strangulations the result of causes acting exteriorly to its tunics. In these cases the proper treatment is to divide the bands, or relieve the cause of strangulation. In the event, too, of an error of diagnosis, the opening in the loins does not provide any facilities for correcting the error. The danger of total failure of affording relief consequent upon this state of things must, therefore, be attributable as a demerit to the operation in the loins.

There are, besides the minor evils in this operation, that the opening cannot be conveniently attended to by the patient himself, and that there exists frequently a great disposition to contraction, arising from the great depth of the wound, which requires renewed surgical interference.

In all these particulars, with the exception of the necessary attendant of peritoneal section, the operation of opening the abdominal parietes at the groin, in all cases of obstruction, or suspected obstruction, in the lower part of the colon, appears to the author to be the operation which should be preferred. It affords facilities for modifying the treatment, either by opening the intestine, when incapable of relief by other means, or by dividing or removing any existing cause of strangulation. It enables the surgeon to extend his search within a limited range, in the event of the diagnosis proving incorrect; it allows him to open the bowel as close as possible to the seat of obstruction; and it secures to the patient the facilities for attending to his own comfort, which appear almost a necessary condition to make life endurable under such circumstances.—*Transactions of Royal Med. and Chirurgical Society, in London Medical Gazette*, July 11, 1851.

Part Fifth.

MEDICAL NEWS.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

MEETING VII.—*Wednesday, 21st May* 1851.—Dr BEGBIE, *P.*, in the Chair.

ENCEPHALOID DISEASE OF LUNG.

Dr Douglas laid before the Society a preparation showing the effects of an encephaloid tumour in the upper and front part of the chest. Dr D. first pointed out the seat of the tumour under the upper part of the sternum, extending to the left so as to occupy in part the left subclavian and mammary regions,

firmly adherent to the parietes, and dipping between the pericardium and left lung; the contiguous part of which had become involved in the cancerous mass—specks of pulmonary tissue still appearing in the tumour at some depth from its surface, and infiltration of cancerous matter having occurred in the lung, beyond the more marked boundary of the tumour. In the upper part of the sac of the pericardium, to the left, the tumour presented several irregular nodulations, not as yet fungoid. In the recent state of the parts, the pericardium was distended by a large quantity of blood and serum; the tumour and left lung adhered, except at the lower part behind, where the sac of the pleura contained about a pint of serum; there were evidences of general bronchitis; the head of the pancreas contained a dense schirrous tumour, as large as a hen's egg; emaciation was great; and there was considerable anasarca of the left upper extremity, and left side of the chest. The left carotid and subclavian veins were, at their origin, inextricably involved in the tumour; and they contained firm imperfectly decolorised coagula, which appeared to plug them up.

Dr D. said he would not detain the Society with a detailed history of the case; and, in the observations he had to make, would confine himself to certain of the physical signs, which in thoracic tumours are variable and obscure, and which, nevertheless, constitute the chief data from which a diagnosis is to be formed. In the present case, however, the general or rational symptoms had afforded indirect but important information. Hæmoptysis had occurred at a very early date, above a year before the patient applied for relief, with cough and difficult breathing when she exerted herself; for a long period, also, she had suffered from fixed pain high up between the scapulæ, and deep-seated; at the same time the general effects on the rest of her system had been slight, contrasting with the severity and duration of the symptoms referable to the organs of respiration; her pulse had not exceeded 100; and she had had no perspirations. From her own account, she had experienced more or less distress for two years before the symptoms became urgent, and two years and a half before her death. The chief difficulty in diagnosis had been to determine that the case was not one of chronic pleuro-pnuemonia, with pulmonary tubercle; and the physical signs did not throw much light on this difficulty; while the early hæmoptysis, and the absence of these cachectic states of the system which accompany the diseases usually causing hæmoptysis, afforded only negative evidence of the nature of the case. Of the physical signs, Dr D. drew attention to the absence of several which have been considered to be distinctive of such cases—the varicose state of the superficial mammary and epigastric veins; it was difficult to explain the want of this condition, and it would be found to be less invariably present than has been supposed. Dr D. referred to the occurrence of distension and even varix of the superficial mammary veins in some cardiac diseases in which dilatation preponderates.

The jelly-like expectoration described in cases of pulmonary cancer, had not existed, probably owing to the extra-pulmonic origin of the tumour. The sounds of respiration had not been suppressed to any extent; harsh mucous rattles having existed in the subclavian region. Two of the most direct signs in the case had been the existence of a pulsation more forcible and extended than occurs in other forms of pulmonary consolidation, and the diminished vibration of the voice, which is for the most part increased by consolidation, and by compression from pleural effusion—cancerous consolidation, however, always causing diminished vibration in the part, and deep-seated tumours in compressing the bronchial tubes, certain aneurisms, for example, inducing suppression of the vibration throughout the side of the chest. In conclusion, Dr D. directed attention to the following as the leading peculiarities of the case in its diagnosis:—The early occurrence of hæmoptysis and other local symptoms—the *general* effects of the disease, especially the want of frequency of the pulse and of perspiration being disproportionately slight; the peculiarities of the inter-scapular pain; the only physical signs of value being the *position* of dull percussion with pulsation and diminished vocal vibration.

CANCER OF THE LIVER IN A DOMESTIC FOWL.

Dr W. T. Gairdner exhibited a specimen of disease of the liver in a duck, accompanied by enlargement of the absorbent glands about the gizzard and commencement of the intestine. The liver was much enlarged, and presented, both on the surface and on section, many distinct tumours strikingly resembling the tubercles of Farre in the human liver. They bore a similar average proportion to the bulk of the organ, being generally about three-eighths of an inch, or half an inch in diameter; they were more or less accurately globular, but most of them, where they appeared on the surface, were either flattened, or had a cup-shaped depression. They were on section like the firmer varieties of Farre's tubercle, in which partial drying up of the cancerous matter has taken place; the colour of a grayish white at the circumference, and ochrey tint towards the centre; the different tints being marked off from one another by irregular lines concentrically disposed. The microscopic appearances could not be satisfactorily made out, as the preparation had been placed in spirit some time before an opportunity of examining it was obtained.

URINARY DEPOSIT OF URATE OF SODA.

Dr G. also showed a specimen of urinary deposit, having the microscopic characters ascribed by Dr G. Bird to urate of soda, but occurring in the urine of a patient suffering from pneumonia, and not taking any form of soda as a remedy. This circumstance justified doubts as to the nature of the deposit, which had not yet been submitted to analysis.

ARTIFICIAL NOSE.

Dr W. A. Roberts exhibited a model of an artificial nose and palate. He stated that, at the request of Dr C. Bell, he had attempted to construct an artificial nose and palate for a patient of his who had entirely lost the nose and a portion of the palate. After many attempts, he had at last succeeded by employing gutta percha.

The first step was to take an impression of the parts of the face with warm wax, and casting this with plaster of Paris, an accurate mould of the part desired was got. To this a nose of common wax was shaped and fitted; this wax nose was then carefully imbedded in plaster, the wax was melted out, and thus a counter mould was formed. He then got some whitened gutta percha, passed it through a pair of jewellers' rollers to bring it to the desired thickness of about a shilling; a piece large enough was first heated in warm water, then gently pressed into the counter mould; when cold, a form was given to it exactly the same as the wax model; this being carefully fitted to the face, a silver wire was attached to it on the inner side by means of a piece of heated gutta percha, the end of the wire was turned to a small ring to which a piece of fine sponge was sewn, sufficiently long to pass over the deficient portion of the palate. The palate, as usual, was made of gold, with a piece of sponge attached to the ridge, passing through the opening into the base of the lost nose. The nose was firmly held in its place by being pushed back close to the face, the palate being then put into its place, the sponge of the palate being in front of that of the nose held it in its place, the one supporting the other. The upper portion of the nose was held firmly to the face by means of spectacles having an elastic band going round the head. The nose was carefully coloured to suit the heightened spots and shades of the face, and, when seen at a few feet distant, was hardly observable. The patient wore these helps till his death. The sponge was found to be of great comfort, by absorbing any discharge that occurred.

CASE OF HÆMORRHÆA PETECHIALIS. BY DR IMLACH.

(This case appears among our Original Communications.)

MEETING VIII.—*Wednesday, 18th June.*—Dr SELLER, Vice-President, in the Chair.

CASE OF MALIGNANT DISEASE OF STOMACH, &c. BY DR WILSON.

The details of the following case are, I regret to say, so imperfect in many respects, that I should not have thought of submitting them to the Society had I not been requested to do so by Dr Gairdner, considering the case interesting in some respects in a pathological point of view.

The patient, an unmarried female servant, aged sixty-three years of age, had, for upwards of twenty years suffered from dyspeptic symptoms, viz. more or less pain in the stomach, anorexia, and pyrosis; these abated considerably about two years ago, but within the last six months had become greatly aggravated, especially the pain at the stomach, leading her to hold her hands firmly over it with the hope of relief; she had also some difficulty of swallowing, and the attempt to do so often brought on vomiting, consisting partly of the food and a quantity of tough mucus of a slightly greenish hue. At the time I first saw her, viz. about three months ago, these symptoms continued, and led me to infer ulceration of the stomach in the vicinity of the cardia, and that most probably of a malignant character. The patient did not present much of the peculiar hue of malignant disease, and there was no tumour to be felt externally; the liver also was normal in size; the urine was not coagulable by heat, and the other functions were normal,—heart, lungs, &c. The patient had some pills of aloes and iron to regulate the bowels, which were very constipated, Prussic acid, naphtha, and creosote, as also a blister, none of which, however, succeeded in allaying the vomiting, which gradually subsided spontaneously, apparently from want of material, as the appetite completely disappeared, and she consequently became more and more emaciated, till she died on the 11th June. A post-mortem examination was made on the 13th. Notwithstanding the emaciation, there was nearly an inch of fat beneath the abdominal integuments. On laying open the abdomen, there was observed a small tumour about the size of a nut, situated on the lesser curvature of the stomach; on attempting to remove the latter organ it gave way, and in doing so, displayed a mass of ulceration in the vicinity of the cardia. This was removed for examination along with the uterus and ovaries, and was submitted to Dr G. The spleen was healthy; the liver presented a nutmeg appearance when cut into. There was also a large mass situated along the spine, supposed to be of a cancerous nature, surrounding the spine, extending from the ninth or tenth joint to the lower curvature, and matting the vessels, &c., in that situation; the relations of which I now regret I did not examine more particularly. The kidneys were large and rather congested. The lungs were healthy. The heart was loaded with fat.

The parts examined by Dr W. T. Gairdner were the uterus, a portion of the stomach, and some of the adhering absorbent glands. The uterus presented a number of small fibrous tumours in its wall, producing considerable deformity of the organ; some of them had evidently undergone to a great extent the calcareous degeneration common in such growths. Several of the tumours, not exceeding the size of a pea, projected immediately beneath the peritoneum; one, which was equal in bulk to a small walnut, was attached to the exterior of the fundus uteri, by a pedicle three-quarters of an inch in length, and lay in the peritoneal cavity free from adhesions, but considerably congested on its exterior. Two of the tumours were in the cellular tissue, in the neighbourhood of the neck and body of the uterus; they were evidently of long standing, being completely calcified; but whether they had originally arisen in this part of the wall (which was a rare situation for such tumours), or in the fundus, it was not possible to form a conjecture, as they were totally detached from the uterine wall. The cervix and os appeared to have been normal, but were injured in removal. The ovaries and Fallopian tubes were healthy.

The portion of stomach removed presented no distinctly cancerous appearance;

the attached glands were large, soft, friable, and contained a whitish creamy juice which shewed large and highly granular nucleated cells under the microscope. Though these were not characteristic of a cancerous structure, yet it is impossible, viewing these facts in connection with the enormous tumour recorded by Dr Wilson in his account of the dissection, to doubt that these glands formed the appendages of an encephaloid mass overlying the vertebrae.

In this point of view, Dr. G. observed that the case was interesting as showing how very little tendency these fibrous tumours have to undergo any malignant degeneration, since they were found in the present instance to exist in every stage of development and of transformation into calcareous matter, without the least trace of cancerous structure in or around them. Cruveilhier, indeed, believed that the fibrous tumour of the uterus, or of any other part, conferred a comparative immunity from cancer of the same organ, and that these tumours themselves never underwent this degeneration. Dr G., however, had seen, in the museums both of London and Edinburgh, specimens opposed to this doctrine, in which the uterus, manifestly cancerous, contained also fibrous tumours, and these were undergoing at some points, fusion into the mass of cancerous tissue.

CASE OF ANEURISM OF THE THORACIC AORTA, WITH OBSERVATIONS ON THE PROPRIETY OF PERFORMING TRACHEOTOMY IN SOME CASES OF ANEURISM.

Dr W. T. Gairdner read a case of aortic aneurism, which presented the following peculiarities :—The sac, about the size of a walnut, arose from the back part of the arch, which was but slightly dilated ; the tumour pressed upon the left recurrent nerve, and had in this way determined frequent attacks of orthopnœa, with laryngeal obstruction ; at the same time that there was no evidence nor appearance of serious pressure upon the air-passages. The aneurism had opened into the trachea, but without important hemorrhage; nothing but a tinge of the expectoration had resulted, the sac being full of coagulum. There was no physical sign of the presence of a tumour, and the aneurism remained undiscovered till the patient's death, which occurred, not from hemorrhage, but from spasmodic suffocation. Tracheotomy was projected from the first, and repeatedly urged, but the patient would not submit to it until he was moribund, when accumulation in the bronchi had taken place to such an extent as to render it unavailing.

Dr Gairdner discussed several points in the pathology and diagnosis of this case, and concluded by observing that the state of the parts as found after death, fully justified the attempt to relieve the patient of his distressing symptoms by tracheotomy. He argued that there was distinct evidence of the frequent termination of aneurisms in this situation by spasmodic laryngeal suffocation ; and under such circumstances, if it was ascertained that the lower air-passages were tolerably free from impediment, he thought that tracheotomy was not only justifiable, but imperatively demanded as a means of prolonging life. That the operation of tracheotomy might have the effect of prolonging existence, even in aneurisms, more advanced, and circumstances far more desperate than the present, was proved by a case recorded in the Lancet for 1844, in which Mr Judd performed it upon a patient in articulo mortis, with the effect of prolonging life thirteen days, although a large aneurism of the aorta had been pressing upon, and had burst into the trachea, causing hemorrhage to the extent of thirteen ounces. And although few would be inclined to covet an opportunity of following this practice under the like circumstances, the case was instructive as pointing out how much might be done for the relief of those in whom the symptoms were chiefly or exclusively laryngeal. At all events, as the propriety of tracheotomy in aneurism was not, so far as he knew, formally recognised by any authority, and as in several well known works it appeared to be discouraged, or even viewed as unjustifiable, Dr G. had thought it his duty to lay the subject before the Society, with the view of eliciting the opinions of members present.

Dr Seller thought that the presence of an aneurism should in general counter-indicate the operation. In this particular case, however, had Dr G. been allowed to act at an earlier period, the life of the patient would, in all probability, have

been prolonged. But he repeated his conviction that tracheotomy, though justifiable in this case, was not applicable in cases of aneurism in general.

Drs Keiller and *Andrew*, who had had the care of the patient before Dr Gairdner took charge of the case, had detected no physical sign of aneurism, and quite concurred in the propriety of performing tracheotomy.

Professor Miller concurred in the main with the statements of Dr Gairdner, and thought that he had laid an important as well as interesting communication before the Society. Cases of aortic aneurism, with reference to the performance of tracheotomy, seemed to be divisible into two classes,—1. Those wherein the tumour was so circumstanced in situation and bulk as to compress the air-passage, narrowing its calibre, and producing a permanent obstruction to breathing. In those the dyspnœa would be continuous; liable, it may be, to aggravations and remissions, but never wholly absent; and, in most examples also, diagnosis both as to the existence of aneurism and of such mechanical interference with the air-passage, would not be very difficult to the experienced and careful observer. In such cases he would be inclined to refrain from operation, anticipating little or no benefit from it, even of a temporary kind; and fearful lest, by obvious failure, the department of operative surgery should be brought into discredit. Into this class the melancholy case of the late Mr Liston, alluded to by Dr G., evidently came; the windpipe compressed, the breathing continuously laborious, and no likelihood of satisfactory relief from artificial opening of the windpipe.

The case quoted by Dr G. from the Lancet, might seem to oppose this opinion. But he (Professor Miller) felt inclined to regard that case as quite an exceptional one. The man could breathe only through a small tube or catheter thrust down from the wound past the aneurismal swelling; and that tube—so small, so liable to displacement, so liable to obstruction, so certain to cause advance of ulceration in the part of the tumour on which it lay—seemed to him too miserable and slender a tenure of existence to warrant its being had recourse to with any hope of success. It was only surprising that through such a precarious access to the lungs, the man continued to live so long.

2. The second class included those cases in which the tumour, not so large as to compress and narrow the air-passage, is yet so circumstanced as to affect the larynx dangerously; as in Dr Gairdner's case, irritating or compressing the recurrent branch of the eighth nerve, so as to excite paroxysmal attacks of intense laryngeal dyspnœa, similar in their nature to those of laryngismus stridulus. These tumours, from their small size and deep locality, would be difficult of detection; and yet there were means, too, as Dr G. had shown, both positive and negative, by which their existence might be at least strongly suspected. And, under such circumstances, he (Professor M.) would have no hesitation in saying, as his own belief, that—provided the paroxysms of dyspnœa were such as to threaten instant extinction of life—he would have recourse to tracheotomy at a comparatively early period of the case, not waiting till the patient was in *articulo mortis;* and for the following reasons:—The operation in itself was not formidable, not difficult of performance, and not dangerous to life. By having recourse to it, he would relieve the patient at once from intense agony and suffering. Besides, at comparatively little cost, he would prolong existence—for hours, weeks, or months; and all well knew how vastly important even a comparatively short period of reprieve might be found to be under such circumstances. Further, it was not wholly utopian to suppose that such relief and prolongation of life might, in some rare cases, contribute towards the actual occurrence of spontaneous cure. In listening to the details of the post-mortem examination in Dr Gairdner's case, one could not fail to be struck with the near approach made in that case to cure. The small aneurism was almost entirely filled with clot, and the oval aperture by which it communicated with the aorta was almost wholly obstructed by a mass of fibrin. Very little more might have silenced the tumour. And that little might have been greatly contributed to by the relief of respiration afforded by tracheotomy timeously performed; while, on the other hand, the oft-

recurring attacks of dyspnœa, with the attendant excitement and distress of the patient, could not fail not only to oppose all attempts at spontaneous cure, but also to hurry on the fatal advance of the tumour.

Hitherto his creed had been—(he had taught it in his class, and published it in his System of Surgery)—in regard to the performance of tracheotomy in connection with aortic aneurism, that the operation should not be had recourse to, the cause of the suffering being apparently beyond relief by tracheal wound. But now he was bound to say, that he saw reason to alter that opinion, and to hold that, while the rule of non-interference still held good in the first class of cases which he had specified, in the second there was not only a warrant but a call to operate, with the hope of certainly relieving suffering and prolonging life, and with the chance of even contributing towards the occurrence of spontaneous cure. There might be a third class of cases (as suggested by Dr Gairdner) in which pressure on the windpipe was slight, but yet with a certain degree of contraction of its calibre, and in which *paroxysmal* laryngeal dyspnœa was also urgent. In such circumstances he would hold that the latter part of the case, or major, would rule the former part, or minor; and that, therefore (more especially if urged to it by the patient), he would be inclined to operate, though, of course, with a less favourable prognosis than when no compression and contraction of the windpipe existed.

On the whole, he considered that Dr Gairdner had made an important contribution to practical surgery; extending its operative interference to a class of cases—few in number, doubtless, and not holding out much hope of permanent benefit—hitherto supposed by many to be excluded from its range. It was the province and duty of the surgeon, as well as the physician, to palliate by every reasonable means where he cannot cure; and if unable to save life, at least to protract and soothe its closing day.

After some remarks by Dr D. Maclagan,

Dr W. T. Gairdner said that he had alluded to Mr Liston's case as conclusively showing that an aneurism of the aortic arch might remain without physical signs, or at all events, might present an extremely obscure diagnosis, up to a period when it had threatened life by one or other of its secondary accidents. The present case, as well as others, proved that spasmodic dyspnœa, a frequent cause of death in aneurism in this situation, might occur and prove fatal while the air-passages were exempt from pressure; and while the larynx, involved through its nerves, was the sole cause of dangerous symptoms. In such a case, the practitioner was surely not warranted in making the probable or certain fatality of the disease an excuse for non-interference, especially when the operation was one not in itself dangerous, and almost certain to secure to the patient not only a comparative immunity from distressing symptoms, but a longer lease of life and a slower progress of his disease. Nothing was more likely to accelerate the increase and fatal result of an aneurismal tumour, than the violent inspiratory and other effects which accompanied the paroxysms of stridulous breathing in these cases; and if these paroxysms could be entirely removed in many cases of aneurism by so simple a proceeding as tracheotomy, it would surely be allowed that this operation presented a fair claim to be admitted into the legitimate treatment of such cases; and to be resorted to, not unwillingly and as a last resource, but *as early as it could be ascertained that laryngeal symptoms were the source of the more immediate danger*. No doubt the operation became less and less eligible in proportion as *other* sources of danger became imminent; but so long as these did not reduce the laryngeal symptoms to a manifestly secondary importance, Dr G. did not feel that he would be justified in refusing to a *willing* patient, *after due explanation*, the chance of life and comparative comfort for an additional day or hour. The amount of pressure of an aneurism on the air-passages could always be estimated by careful stethoscopic examination with considerable precision; and though other sources of danger might remain unknown, this fact ought not to be made an argument against interference for the removal of the palpable and imminent hazard. And if these rules of conduct were sound when an aneurism

was *known* to exist, they were all the more evidently so in the case where such a tumour was only suspected, or where, as in the present case, the suspicion had been allowed on the strength of negative evidence, to drop too hastily from the mind.

An authority so justly and so universally known as Dr Watson, had stated that no man with a stethoscope in his hand, and the power to use it properly, ought to be seduced into the error of performing tracheotomy in a case of aortic aneurism. If the principles adopted by him (Dr G.) were correct, there was here an opinion extremely calculated to mislead; and no doubt there were many physicians who would be alarmed at the idea of opening the trachea, not for a laryngeal, but for a thoracic disease. For his own part, Dr G. had no desire to screen himself from criticism, if it was thought that by any reasonable precaution, a correct and secure diagnosis might have been formed in the present case; but on general, and not on personal, grounds, he thought that a different bias ought to be given to the practical precepts current on this subject, and that practitioners should neither be taught to be too sure of the diagnosis of thoracic aneurisms, nor too apprehensive of the performance of tracheotomy.

CLINICAL MICROSCOPE.

Dr W. T. Gairdner showed a very convenient and generally-useful microscope which he had recently had constructed by Mr James Bryson, and had already employed sufficiently to be convinced of its great superiority, for most purposes of the practitioner, to all the portable and pocket-instruments now in use. It consists of a simple but very carefully-ground Coddington lens of small size, and magnifying about 180 diameters, set in a convenient handle and adjusted to a shallow cylinder of silver, which at one end embraces and surrounds the setting of the lens, and at the other bears a small plate of glass for the object, which may, if necessary, be covered by another glass. The instrument is raised to the eye opposite a moderately strong light, and the object is focalised by a screw fixed in the handle, and bearing on the spring by which the silver cylinder is kept close to the lens. The instrument can be managed conveniently with one hand; and as it is considerably shorter, and not much thicker, than an ordinary pencil-case, it may easily be adapted, if requisite, for the pocket. It distinguishes readily all the ordinary urinary deposits, and to a practised eye, suffices in ordinary cases for the examination of blood, pus, and most of the elements of the tissues; although for delicate observations, a higher power is of course requisite. Perhaps the greatest recommendation of this instrument is the ease and comfort with which it can be used; the examination of a fluid being at once effected by dipping the glazed end of the silver cylinder into it, or placing a drop on the outer surface of the glass; then on shaking off the superfluous moisture and raising the instrument to the eye (at the same time using the focal adjustment), the fluid is seen to float down the glass, bearing constantly renewed objects over the field of the lens, and enabling a rapid and effective survey of the contents of the fluid to be made in much less time than it could be done by a compound microscope with a large field, even if ready on the spot for action.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

THE following omission occurred in the report of Meeting II. :—

Mr Cobbold showed several wax casts illustrative of a case of great hypertrophy of the tongue, which underwent a gradual cure by compression, under Mr Crosse of Norwich. The patient, from whom these were taken, was a girl six years old. On admission into the hospital the tip of the tongue protruded nearly three inches beyond the lips, and an inch below the chin, and at the expiration of one month the entire organ was reduced into the cavity of the mouth. The principal treatment

employed was gentle pressure by means of bandages, and local astringent applications. The case is mentioned in the last edition of Mr Samuel Cooper's Dictionary. See article Tongue.

MEETING III.—*June* 21, 1851.—Dr BENNETT, President, in the Chair.

REPORTS.

LEUCOCYTHEMIA.

1. *Mr Sanderson* read the following report on Dr Gairdner's case communicated to last meeting of the Society:—

I have carefully examined the structures referred to in Dr Gairdner's communication, and have arrived at results similar to those of its author. The soft light-coloured portions of the spleen, which to the naked eye so much resembled a deposit foreign to the normal structure of the organ, were found to consist of microscopic elements precisely similar to those constituting the mass of the healthy parenchyma. In this as in a few other cases which I have had an opportunity of examining, in which the obvious appearances were similar, I have come to the conclusion that the morbid structure differs from the normal, not in presenting any peculiarities in the elementary forms, but in the proportion which these bear to each other. A comparative examination seems to show pretty distinctly that in the softened portions, while the cellular element is abundant and well developed, the fibrous element, as represented by the trabecular tissue, is deficient or even absent.

From the fact that in the healthy condition of the organ—the cell-form passes by insensible gradations into the fibre, all the intermediate forms between the two being easily observed, we should expect the probable occurrence of an abnormal condition in which the one form should assume the place of the other, or *vice versâ*. Of this the above described lesion seems an illustration.

The lymphatic glands presented nothing abnormal, excepting their increased size.

The blood presented a very large excess of the colourless corpuscles over the usual proportion in which they occur. On adding acetic acid, the so-called nuclei were observed to assume the usual forms. They were found to differ considerably from any element which could be observed in the spleen parenchyma, and not least in the form and size of their nuclei, which last did not exceed from $\frac{1}{6000}$th to $\frac{1}{4500}$th of an inch in diam., while those of the splenic corpuscles measured not less than $\frac{1}{3500}$th of an inch. This circumstance, in so far as it is of any value, militates against the doctrine of those pathologists, who maintain that the colourless corpuscles of the blood, in cases in which they exist in abnormal abundance in connection with enlarged spleen, are directly derived from that organ.

If we consider that the colourless blood-corpuscles, the splenic corpuscles, and the corpuscles of the lymphatic glands, are bodies which, though not identical, are at least analogous, and probably perform the same or similar functions, is it not more reasonable to infer that the circumstance of the simultaneous occurrence of a multiplication of the whole mass of each of these is to be explained by supposing the existence of a cause operating equally on each, than to take it for granted that one cannot be affected excepting through or by means of the other, and to endeavour to bring evidence to prove the existence of an assumed relation, which, even if it were proved, would not bring us one step nearer our object?

Dr Bennett stated that a very important question arose out of Mr Sanderson's report, viz., whether difference in the size of nuclei in cells, was a proof of essential difference in the nature of the cell itself. Considering the great variations which existed in this respect among the same cells, he could not think that such variation could be considered as a ground of specific difference. With

regard to leucocythemia this question was important, for to what was the excess of colourless corpuscles to be attributed? Was the change owing to arrest of development of the coloured particles? was it primary in the blood, or secondary, dependent on the increased growth of one or other of the blood glands? He was inclined to favour the latter opinion, more especially as it was supported by the fact, that in this condition of the blood the glandular structures in relation with it were always hypertropied, whilst this hypertrophy depended more on the excess of the cell than of the fibrous element. Although he believed it was very necessary to be cautious in theorizing on this subject, he thought it very probable that the glandular cell-elements found their way into the blood, and there underwent certain changes, whereby they were converted into the colourless bodies which abounded in the blood, in the pathological condition of that fluid under consideration.

Dr W. T. Gairdner.—I avoided, in bringing this case before the Society, giving any opinion on the connection of the white corpuscles in the blood with those in the spleen and lymphatic glands, because I think that the solution of the obscure questions connected with the development of these corpuscles, can only be looked for in a much wider generalization than any that can be founded on pathological facts like the present. We shall probably not be able to decide upon these questions, until, from embryology and experimental physiology, we have learned with certainty the source, development, and function of the white corpuscles of the blood, and of the vascular glands, in the normal condition. If the white corpuscles in the blood are formed from those in the glands, or the latter from the former, it might naturally be expected that the transition stages would be found in one or other situation. Are they so? I am not prepared either to affirm this, or to deny it. The corpuscles in the blood have much more uniformity of character; those in the spleen and glands, on the contrary, whether in the normal or pathological condition, have so much variety, that I have no doubt all the transition stages might be found among them. But are they found in such proportions as to present a regular series? This is very doubtful; on the contrary, it has appeared to me, that white corpuscles of the exact size and characters of those of the blood are not numerous in the spleen, and that other bodies, larger and smaller, nucleated and non-nucleated, are disproportionately frequent. I have, however, on some occasions seen the spleen much more full than usual of bodies exactly resembling the white corpuscles of blood, or of pus. To some extent it was so in the softened parts of the spleen in this instance; but the most marked case of this condition which I ever saw was in the body of an elderly man, of small stature and very fat, who for years carried about with him a large swelling in the front of the neck, suspected at one time to be goitrous, but which proved to be nothing but accumulated adipose tissue. The state of the blood was not in this instance ascertained. The spleen was not much enlarged, but soft and pale, and contained a cheesy yellow mass the size of a walnut. The epithelium of most of the glands was very granular. The thyroid gland was present, and in this respect the case differed from two cases of very similar swellings in the neck, published by Mr Curling.

COMMUNICATIONS.

MELANOSIS.

The following communications on this subject were made to the Society:—

1. *Dr Bennett* exhibited to the Society a portion of a foot which had been amputated in the Infirmary by Dr Dunsmure. The patient was an engine-maker aged thirty-nine. About three years ago he observed, as the result of wearing a tight shoe, that there was considerable induration between the third and fourth toes of the left foot. This gradually increased, and a deep fissure occurred in the indurated integuments over the corresponding metatarsal bones. Gradually a tumour formed, which, on admission, was circular, raised above the

integument, the size of half-a-crown, hard to the feel, with a circumscribed edge, and posteriorly presented a dark melanotic appearance. It was removed on the 15th of June. On dividing the diseased part, it was observed that the tumour was about three fourths of an inch deep anteriorly, and that of half an inch posteriorly. It presented two distinct characters, the two anterior thirds being hard, white, crunching under the knife, and yielding no juice on pressure; the posterior third being softer, and completely black. A demonstration of the structure was exhibited under three microscopes, showing that the white portion consisted of a dense mesh-work of fibres and fusiform cells, mingled with oval nuclei; and that the melanotic portion consisted of a fibrous stroma, infiltrated with a mass of reddish brown molecules and granules, several of which were aggregated into opaque masses, whilst a few were contained within cells. Dr Bennett observed that he had no doubt these fibro-nucleated growths would be found on investigation to be more common than was generally supposed, and that their correct history was yet a desideratum. He presumed that the melanotic portion was owing to changes in the tumour following an extravasation of blood, and invited the members of the Society to express their views as to the nature of melanosis.

2. *Mr Murchison* presented to the Society a melanotic cancerous tumour, about the size of a chestnut, attached to the prepuce and glans penis; and also a portion of the pleural surface of the diaphragm from the same patient, covered with morbid deposits of a similar nature. The tumour had been found, on microscopic examination, to possess the ordinary structure of cancerous tumours, except that it was infiltrated with a quantity of dark matter, which under the microscope appeared in the form of numerous Sienna brown granules, both free and also enclosed in cells, in some of which distinct nuclei could be detected.

3. *Mr Drummond* read the following analysis of melanotic matter, from the pleura and lymphatic glands, in the case referred to by Mr Murchison:—

The pigmentary matter presented a dark brown colour, mingled with portions of a light brown, approaching to a red tint.

It was insoluble in water, alcohol, and ether.

When treated with hydrochloric, nitric, and sulphuric acids, it was dissolved—the solution being nearly colourless.

When chlorine gas was passed through it, suspended in water, it was bleached to a certain extent, but not entirely.

When boiled with potash, it dissolved with disengagement of ammonia.

The ultimate analysis yielded the following result:—

Carbon, ...	67.01
Hydrogen, ...	6.45
Nitrogen, ...	11.45
Oxygen, ...	8.36
Ash, ...	6.73
	100.00

The ash consisted in great part of peroxide of iron.

Mr Barlow stated that melanosis among the lower animals was almost exclusively confined to gray horses. The disease often appears locally at first, and manifests itself on the external parts of the body covered by dark-coloured skin, as the under surface of the tail, anus, margins of the eye-lids. When of longer duration, many of the internal organs, but more especially the mesentery, become involved, and instances are seen where the voluntary muscles are extensively affected. It does not evince any of those characters which in the human subject are taken as indications of malignant disease; on the contrary, unless exposed to mechanical injury, it does not ulcerate nor soften; and when a tumour is freely cut into and partly removed, the portion still remaining will

readily heal. As an illustration of this, he might mention that Professor Dick once amputated a horse's tail as high up as its second vertebral segment; it was a complete mass of melanotic deposit, and weighed 56 lbs. avoirdupois. In this operation a large amount of diseased mass was still left in connection with the anus and croup. This incised surface, however, healed, so far as could be judged, in quite as short a time as an incision of similar extent would have done if made in completely healthy tissues.

Dr Bennett stated that, putting aside that form of melanosis which was observed in the lungs and bronchial glands of colliers and others, and which undoubtedly consisted of the deposition of carbon into the tissue, he regarded other forms of melanosis as consisting essentially of altered blood. It might occur with or without malignant disease. In the former case, blood was extravasated into cancerous growths, and its colouring matter was either diffused through them, or passed into their contained cells. In the latter case, it was infiltrated through the structure, in a granular form, often collected together in masses, and occasionally cells were formed, which contained these granules. In the horse, the cell structures were often very perfect, the granules existing between the cell-walls and nucleus, which remained colourless and transparent. In that animal, he had been informed by Professor Dick, that it was not malignant. From all the facts he was acquainted with, he was led to infer that the black colour of melanosis was owing to some chemical change occurring in the colouring matter of the blood, the nature of which was unknown, and hence why black colorations were so common, under various circumstances, in the body. That it was not sulphuretted hydrogen acting on the iron of the blood—an opinion which had originated from the frequency with which such colorations existed in the neighbourhood of the intestines—was, he thought, shown by the fact that he had discovered them in the centre of the spinal cord, in a case of chronic softening of that organ. On one occasion, also, he had asked Dr Wilson and Dr Douglas Maclagan to test a mass of melanosis removed from the horse for sulphuret of iron. Both gentlemen experimented separately, and concurred in informing him that not a trace of that substance could be found. No doubt further chemical examination of melanosis was much to be desired.

Mr Sanderson remarked that, if the term melanosis were only understood to include those morbid growths which were analogous to the normal pigmentary structures—such as the *pigmentum nigrum* of the choroid—he was not aware of any evidence to prove that melanotic pigment was directly derived from hæmatin—the only circumstance leading to such a conclusion being the similarity in chemical composition, especially in respect of the presence of a considerable proportion of iron in each.

Mr Cobbold said he had examined the colouring matter of true melanosis, and believed it to be analogous to the *pigmentum nigrum* of the choroid coat of the eye, in which structure the dark appearance of the cells was merely owing to the presence of minute granules in their interior, possessing very high refractive properties, and which, when viewed singly, under high magnifying powers, presented a dark broad margin with a central clear space. If the granular material were a transformation of the colouring matter of the blood, we should expect to find the granules homogeneous in appearance, and not presenting the central clear space.

Mr Murchison could not agree with the opinion that melanotic matter was derived in all cases from extravasated blood undergoing change. He thought that the fact of the colouring matter existing in the interior of nucleated cells could not be accounted for by this theory. He agreed that the colouring matter was derived from the blood, but he was inclined to believe that its development depended not on a *mechanical* change in the blood itself, but rather on a *vital* action of the surrounding tissues.

Dr W. T. Gairdner—If we are to regard every morbidly dark structure as an example of melanosis, then there can be no doubt as to the propriety of establishing many distinctions or varieties, and particularly of distinguishing the

malignant and non-malignant kinds. In most of the latter the pigment seems to be derived from that of the blood, though not always in consequence of extravasation. The dark colour of gangrenous parts is a merely chemical re-action of sulphureted hydrogen on the pigmentary matter of the blood. Of a similar kind are the slaty discolorations found in the parts about the colon in the dead body. Again, the acid juices of the stomach have, as Cruveilhier has particularly pointed out, a tendency to blacken extravasated blood; and the same may be said of the juices of the colon. In both of these situations slate-coloured points or patches are common, not merely where blood has been extravasated, but where it is still within the vessels. The orifices of the follicles of the colon are often surrounded with a narrow rim of slate-colour. I have seen the point of every villus throughout the greater part of the small intestine tinged with the same colouring material, which, viewed under the microscope, was obviously contained in, or in contact with, the terminal vascular loops. In the hemorrhagic erosions of the stomach, the blood on the surface of the ulcer is generally slate-coloured. In old dysenteric ulcers a similar colour commonly prevails. In old corpora lutea and simple cysts of the ovaries there is often a slate-coloured tinge; and in various other situations, under circumstances not fully understood, the blood either within or without the vessels may undergo similar changes. But the colouring matter observed in these cases, even to the most unpractised eye, and still more when subjected to minute examination, differs widely from that observed in the true melanotic *tumours*. This is of a brown tint, like the choroid pigment, not of a blue or green shade, like the other; and is disposed in the form of lustrous angular solid granules, instead of being almost amorphous under high microscopic powers, or diffused in a fluid. I have found it, both in man and in the horse, formed in cells, often of large size and nucleated; although these cells do not exist in all parts of the growth, being often disintegrated, except in the most recent formations. The differences in this respect in the same subject, or at different parts of the same tumour, are considerable; but, making allowances for these differences of development, the nature of this structure in the horse has appeared to me identical with what I have observed in two excellent examples of the disease in the human subject.

Are the melanotic tumours malignant? It is clear that, both in the human subject and in the horse, they have a tendency to appear in considerable numbers, and to attack various parts of the body, the pigment being quite evidently carried along the great lymphatic vessels. They do not, however, spread indiscriminately through the tissues in the neighbourhood of those first invaded, and they are certainly less prone to softening or to rapid and destructive ulceration than most forms of cancer. In the horse it is said that the parts heal rapidly after their excision or even *partial* removal, and that life appears to be but little, if at all, shortened by them. The horse, however, is an animal little subject to destructive or malignant ulcerations of any kind; and it may very well be conceived that the same constitution to which it owes this exemption, may modify the progress of the melanotic growth, and render it less dangerous to life or tissue than in the human being, so frequently the subject of cancer in all its more ordinary forms.

4. *Mr Murchison* exhibited a preparation and drawings of a very interesting case of necrosis of the femur, in which the exfoliation of dead bone had penetrated the condyles of the femur into the cavity of the joint, notched the upper edge of the patella, and projected through the soft parts in front of the knee-joint. The patient was a girl, and her limb had been amputated by Mr Syme with success.

5. *Mr Murchison* exhibited two specimens of urinary calculi, lately removed by Mr Syme from patients in the Surgical Hospital:—1. A calculus weighing 1265 grains, the nucleus and external portion of which consisted of lithic acid, and the intermediate portion of oxalate of lime. 2. A calculus weighing upwards

of 300 grains, and consisting principally of triple phosphate, with a central nucleus of lithic acid. The latter was evidently the fragment of another stone, around which the phosphates had been deposited, and was interesting in connection with the fact, that the patient had a large stone crushed in his bladder eighteen months before.

ON A PECULIAR ARRANGEMENT IN THE VENTRICULAR FIBRES OF THE HEART.

6. *Mr Zaglas.*—I beg to exhibit to the Society, in the hearts of the bullock and sheep, a curious arrangement of the fibres in the wall of the ventricles, producing an appearance on section hitherto undescribed. If a section be made across the two ventricles of a sheep's heart, and carried parallel to the auriculo-ventricular septa, or nearly so, a phenomenon presents itself to our view, which is both beautiful and interesting. The surface of the section has of course the form of an 8, the circular halves of which are broadly blended together, to constitute the section-surface of the septum ventriculorum. The middle points of this surface are occupied by a beautiful marking of the same general form, but bearing a resemblance in its details to two circlets of laurel, the branches of which are represented by a tract of central fibres, and the leaves by lateral processes on either side. This marking is distinguished from the remaining surface, namely the outward and inward peripheral points of the section, by a paler colour, and an apparently greater compactness of its substance.

Writers, even monographists, so far as I am aware, do not mention this appearance, most probably because they do not attach any great importance to it. I believe, however, that the student of anatomical structures, in paying attention to it, will be rewarded, in the first place, by the sight of a structure by no means inferior in beauty of arrangement to that of the arbor vitæ; secondly, by gaining a useful aid in remembering the arrangement of the muscular fibres of the ventricles; and perhaps, thirdly, by meeting with a physiological arrangement admirably adapted to give to a certain quantity of muscular substance the greatest efficacy for the propulsion of the blood out of the ventricles, and enable this mass to resist the counter-pressure of that fluid.

The secondary fasciculi of the muscles of the heart are not composed, like those of the voluntary muscles, of parallel primary fasciculi stretched between two tendinous surfaces, that of origin and that of insertion; but they are rather fibrous tracts, composed of elements which are continually entering into new combinations; for most frequently fibres detach themselves from one tract to join some other of the surrounding ones, while these again send fibres to the first. Thus the substance of the heart is a dense meshwork of fibres, which, however, at certain depths, have certain main directions of union and separation. The latter circumstance makes it possible to distinguish layers in that substance, which succeed one another from the external to the internal surface, and are distinguished by an increasing obliquity of their fasciculi or tracts, to a plane passing through the axis of the heart. It is nevertheless necessary to take off a layer of muscular substance of considerable thickness, before arriving at one that has its fibres strikingly differing in direction from that of the one removed; for the transition is made by a gradual increase of the angle at which the superficial fasciculi of the two exposed layers are seen crossing each other, all the intervening degrees being represented by the deeper fasciculi of the removed layer.

Now in the section of the heart, carried in the plane that has been mentioned above, it so happens that the fibres of the middle layer run parallel to the plane of the section, while the fibres of the layers more superficial or more deep, are met with by the cut at an angle increased in proportion to their remoteness from the middle. This parallelism of the fasciculi with the surface of section, by diminishing the apparent number in a given area of interstitial spaces between the fibres, is one cause of the lighter colour in the central points of the section of the ventricular walls. Another circumstance tending to the same effect is the ar-

rangement of the minuter elements, the ultimate fibres having, in relation to the fasciculi, precisely the same disposition as these last, in relation to the whole wall of the heart.

I shall now say a few words on the arrangement of the fibres, and the purpose subserved by it. Having a heart before us with its apex downwards, we find, looking at it from either side, that the most superficial fibres begin at the sulcus horizontalis, and run downwards, and from our left to our right hand, thus describing an approach to a spiral curve around the ventricles. Let us keep in view one of these fibres, and then go to a deeper layer. The fibre of this second layer, which ought to be covered by that of the first, if both layers had their fibres in the same direction, will be found to form with the latter a very acute angle,—the angle above the point of intersection being to our left, the opposite one to our right, hand. A third layer will present, in its corresponding fibre, the same relation to the second which this did to the first, while to the last-mentioned its inclination will be double; and so on we may proceed, meeting every following layer in the same relation to the foregoing one, and their gradual increase of inclination to the superficial one, till we arrive at the central layer, the fibre of which will be horizontal, and corresponding to the shaft of the laurel-like marking. As the fibres in every layer describe spirals, the turns of these spirals must approach each other in every subsequent layer, until in the last-mentioned they coalesce to perfect circles. Beyond the middle layer the circles will again diverge into spirals, expanding more and more in every following layer. Our fibres, which we keep in view in the direction of tangents to the corresponding spire, will retain the same progressive relation to one another, although their prolongations beyond the point of intersection will exchange sides with reference to the horizontal. Comparing now the most superficial fibre to the deeper one, or the fibres of the two surfaces, we shall find that we have passed through an almost entire revolution. If the whole set of fibres be combined into a continuous structure, a double fan results, which expands to the right and to the left with an obliquity and incurvation of its surfaces corresponding to the shape of the heart, and an axis of rotation for its radii equal in length to the thickness of the wall.

We have had in view one fibre from every layer, and have thus followed but one architectural element of the whole. But since, in every layer, the tracts of fibres are parallel, we can easily see how the double fans are sheathed to build up the walls of the ventricles, and how every two respective points of the external and internal surfaces are united through an axis of rotation for the radii, both the surfaces consisting of such points. Thus, in all the directions from the peripheral points to the centre of a ventricle, the greatest as well as most uniform density of the walls has been attained, and two objects effected which are of great importance for the function of the heart and the safety of the individual. In the first place, the heart is enabled to apply, if necessary, the entire amount of its muscular force to the propulsion of the blood; in the second place, it appears to me that a provision has been made against the formation of diverticula.

7. *Dr W. T. Gairdner* exhibited a specimen of cancerous ulceration of the uterus, vagina, and bladder, from a middle-aged woman who died in the hospital. The cancerous deposit was limited to the edges of the ulceration, which was very extensive; and, notwithstanding that nearly the whole cervix and body of the uterus, and the greater part of the vagina, were ulcerated, the fundus was quite free from all appearance of morbid deposit. There was an opening between the vagina and the rectum about an inch and a half in length; but this seemed not to be due to the invasion of the rectum by the cancerous deposit, but to the effects of labour which had taken place three months before the death of the patient. There was no cancer in any other organ, or in the lymphatic glands.

The cancerous parts showed, under the microscope, very numerous cells, chiefly more or less fusiform, with nuclei ranging up to the $\frac{1}{70}$th of a line in diameter, sometimes with evident nucleoli; the nuclei and nucleoli mostly single.

(These appearances were demonstrated under the microscope.) Dr G. said that this might justly be called an "epithelial cancer," in respect of its origin in an epithelial tissue, and its tendency to spread through the parts immediately adjoining, producing extensive ulceration, while, at the same time, distant organs were unaffected. Its structure, however, had no particular resemblance to that of epithelium, and resembled closely that found in some forms of medullary cancer.

8. *Dr W. T. Gairdner* showed a specimen of tumours in the liver and gastric glands of a duck, which he regarded as being of cancerous character, although the microscopic structure could not be satisfactorily made out, the parts being preserved in spirit.—(See the Medico-Chirurgical Society's Report for May 1851.)

ENCHONDROMA.

9. *Mr Cobbold* read the following narrative, which was illustrated by drawings of the diseased parts:—

I am anxious to detail to the Society a few particulars respecting an enormous cartilaginous tumour, contained in the museum of the Norfolk and Norwich hospital, from a patient who died in that institution. The museum contains also a coloured wax cast, in which I have represented the external characters of the tumour, as they appeared in its fresh state.

The tumour occupied the major part of the pelvic and abdominal cavities, extending from below the tuberosity of the ischium to about an inch above the umbilicus. On further dissection, it was ascertained that the growth extended inwards from the internal surface of the left innominate bone, at a spot corresponding with the pectineal ridge.[1]

To the naked eye, the substance of the tumour was made up of large lobules, presenting an indistinctly cystiform aspect in the interior and outline,—the centre of the mass being denser, and of a more cartilaginous appearance, studded here and there with points of calcareous deposit. The fresh cut surface gave a pale yellowish hue, a tinge of green also being in places perceptible—the cysts contained a fluid of melicerous consistency. A portion of the tumour was forwarded to Professor Paget, to whom I am indebted for the following microscopic characters of the tumour:—

"The part which I received consisted of an aggregate of small masses or nodules, of various shapes and sizes, exactly like those which formed the tumour of the abdomen described in the College Catalogue as No. 207, and in our own, in Series 35, No. 49. Each such portion appeared thinly partitioned from its neighbour, and the outer surface of the tumour was invested with a thickish layer of cellular or fibrous tissue. The cartilage was clean, nearly colourless, and clear; but many of the nodules presented, at their centres, small cavities, with irregular but smooth walls, which appeared to be due to the softening of the cartilage, not different from the rest. In firmer parts of the cartilage, I could barely discern the outlines of any cartilage cells—indeed I doubt if there were any—but there were many nuclei, like those in the cells of ordinary cartilaginous tumours, of rather various size, but well formed and granular. These were imbedded in a nearly uniform, dimly granular, ground-glass-like basis, lying as wide apart as they would have been had each been contained in its proper cell, and some much more closely set together. In the parts which formed the walls of central cavities there were no outlines of cells, and very few or no nuclei. The whole substance appeared a plain dimly granular material, in parts being capable of being spread out like membrane. In a few spots this material appeared obscurely fasiculate; in other parts, delicately and very closely *lined*, resembling large muscular fibres (as those of the eel, for example) which have no transverse striæ."

There being some points in the history of the case worthy of notice, I proceed

[1] The pathological museum of the late Dr Crosse has been purchased by the Museum Committee of the Norfolk and Norwich hospital.

to remark, that several of the long bones were affected with exostosis,—these growths, however, presenting anything but the characters of cartilage, as you will readily perceive on looking at this drawing, which represents a section of one of these bony tumours, attached to the right tibia, and which was of almost ivory hardness; but, on making a section through the longitudinal axis of the head of the left thigh bone, we found the os femoris to be exceedingly soft, while the compact bony margin of the head of the femur, and the cartilage that usually covers it, had (as represented in these drawings) all but entirely disappeared. I must also be allowed to allude to another exostosis, which was situated at the upper and outer part of the right humerus, because in this position the occurrence of such growths seems very rare. Professor Syme remarked, in one of his clinical lectures last winter, after removing an exostosis from a similar situation, that it was the first example he had witnessed of its occurrence during thirty years' experience. This was, however, the second patient at the Norwich hospital in whom I had seen exostosis in that situation.

Further, in addition to the interesting data which the history of the case itself affords, there is this also to be remarked, with regard to the hereditary nature of the disease, that the parents of this individual, as well as others of his kindred, have all more or less been subjected to the debilitating influences of a perverted nutrition. The patient's father (who, I believe, is still living) suffered amputation by Mr Martineau, on account of enchondroma of the lower ends of the radius and ulna; and a brother (also, I believe, still living) is affected with mollities ossium.

The illustrations which, in conclusion, I exhibit to the Society, were taken from sections of the forearm, preserved in the museum of the late Dr Crosse.[1]

Dr Bennett said that the largest specimen of enchondroma he was acquainted with existed in the museum of Mr Lane, close to St George's Hospital, London. Muller considered that this form of growth was not malignant, and never returned. He had met with one case, however, of enchondroma of the arm, in which Mr Syme had amputated at the shoulder-joint, and where the stump healed. Subsequently, tumours of a so-called malignant nature returned in the stump and axilla, and ultimately killed the patient, a girl aged fourteen. He had always considered it most unfortunate that the structure of these secondary tumours had never been examined, as the question with regard to the malignancy of enchondroma would thus have been determined.

HOMŒOPATHIC CANDIDATES.

Sundry printed letters and pamphlets in the homœopathic interest have been extensively circulated of late in this city and elsewhere, relative to proceedings in the Royal College of Physicians and in the University against the delusions of homœopathy. Our readers would scarcely thank us, were we to make these productions the subject of criticism. But, as several of them make use of an incorrect account, which has been given in a homœopathic periodical, of a recent decision of the Medical Faculty in the case of a candidate for medical honours and homœopathic distinction, we feel compelled to supply the professional public with a true version of the whole matter. If we thus violate, for the first time, the well-understood compact between examiners and candidates in this University,—that the fact and circumstances of failure shall be confined to themselves,—the individual principally concerned has himself alone to blame. He must take the consequences of his own ill-advised publication, and his perverted report of the proceedings.

Mr Alfred Crosby Pope, having appeared before the Second Division of Ex-

---

[1] In making these dissections, I was kindly assisted by Proctor Wright, Esq., of Norwich.

aminers of the Medical Faculty in the middle of June, underwent the usual written examination on the practical branches of medicine and surgery, and was afterwards subjected to an oral examination on the same subjects. He had satisfied several of the examiners; but his surgical knowledge was evidently defective. While under examination in midwifery, he was asked what doses of calomel, opium, tartar-emetic, and aloïn he would give in certain diseases; in reply, he stated correctly the doses usually given in medical practice; and when asked whether these were the doses which he would himself prescribe, he replied that they were. He then underwent an examination on materia medica, the professors of that branch and of clinical surgery being present. His replies were satisfactory enough; the only objection of any moment indeed being, that his doses of medicines were somewhat large. The Faculty having been furnished with positive information that Mr Pope had avowed his purpose to become a homœopathic practitioner after graduating, it was determined that he should have an opportunity of answering to the charge. The question was put to him by Dr Christison, and the following are the very words of the conversation that ensued:—"Well, Mr Pope, I am satisfied so far with your answers; but there is another point on which I wish to be informed; and as it is best not to beat about the bush, I shall put to you a plain question, in order that I may get a downright answer. I am told by a colleague that he has been informed on good authority, that it is your intention to become a homœopathic practitioner after you graduate; *after the answers you have this day given me*, I feel bound to say I do not believe it. Am I right?" To which Mr Pope replied, "I am not now a homœopathist; but, after graduation, I mean to inquire into the truth of it." Professor Syme then remarked, "Now, Mr Pope, suppose that this inquiry which you meditate were to confirm your belief in the truth of homœopathy, what would you do with the diploma received from us? would you burn or return it?" "No," replied he, "I would keep it." "For what purpose?" "To show that I had regularly studied." "Studied what? Delusions! Fallacies! Nonsense! It would only show that you had misspent four or five years of your life in studying what could not possibly be of any service, according to your own view; and I am sure that on reflection you must see, how inconsistent it would be with common honesty or common sense to use a diploma, after ceasing to entertain the principles which were professed in order to obtain it. But recollect, Mr Pope, I offer this remark to you as a friend, and not as a professor." Mr Pope then withdrew, and in what remained of his examinations he made a satisfactory appearance, except in medical jurisprudence, in which, as in surgery, he was defective.

The case being a new one, it was referred by the examiners to the whole Medical Faculty for decision. Of the thirteen members, eleven were present. After considering the whole circumstances, the Faculty unanimously resolved—"That serious doubts are entertained as to the soundness of Mr Pope's principles of practice; and that on this account, as well as his insufficiency on some subjects of examination, he shall be remitted till the end of July, by which time he will have had ample opportunity of making the inquiry into the truth of homœopathy, which he says he contemplates." This resolution, which admitted of his graduating this year, in the event of his satisfying the Medical Faculty, was communicated to him in conversation by the Dean. Mr Pope, however, withdrew at once from the list of candidates.

This is a correct narrative of the whole official proceedings. Any thing else that may have been said to the candidate privately by individual professors, could have been said in kindness only, and by them as individuals alone, and is wholly irrelevant.

We must add a single word for the information of distant friends of the University, who may be led to surmise that the misfortune of this young man in some measure justifies the fears expressed in some quarters, lest students may be infected with the delusions of homœopathy while studying at Edinburgh. The Medical Faculty had distinct evidence, which will be produced, if necessary, that

when Mr Pope came to study here three years ago, he avowed that he had practised homœopathy, and came to study for a degree, in order to settle as a homœopathic practitioner; this determination was repeated afterwards; and it could neither have been engendered nor strengthened by any instructions received within the walls of the University, for the principles and practice of homœopathy have never been taught there by any professor.

THE LONDON MEDICAL GAZETTE.

One of the Conductors of this Journal lately felt it necessary to address a letter of remonstrance to the Editor of the London Medical Gazette, who published it in an imperfect form, under the pretext that the matter excluded would have been subject to the English law of libel. Two results have followed; in the first place the letter is rendered meaningless, and, secondly, the author is made to appear to have used libellous language. In order that our readers may judge how far this conduct was warranted, we now place before them the letter in its original form—the omitted portion being enclosed within brackets.

"Edinburgh, June 26, 1851.

"Sir,—I have only to-day happened to see your Journal of May 16, which contains some statements that certainly should not have remained so long unnoticed, if they had been known to me sooner.

"You say, 'a fierce paper war has arisen between the two Edinburgh professors—Syme and Lizars,' but you must, or at least ought to, know that I have not addressed a single word upon the subject in question to the so-called 'professor' [regarding him as long placed beyond the pale of professional respect and courtesy.

"In estimating the value of my operation, you proceed upon the supposition that the allegations of Mr Lizars and his assistant, Dr Müller, are well-founded; but in fairness to your readers, if not to myself, should have mentioned that the statements of these persons, in so far as they attribute bad effects to the operations which I have performed for the remedy of stricture by division, have been declared by me to be all utterly devoid of truth.]

"Within the last eight months I have performed this operation nine times in the Royal Infirmary of Edinburgh, in presence of the largest class of surgical clinical students in her Majesty's dominions. These gentlemen can testify that in no instance has there been bleeding, extravasation of urine, or any other unpleasant consequence, and that all the patients speedily and completely obtained the relief which they desired. As you say that 'something more than the guarantee of Mr Syme's reputation is wanting to assure the surgeon that he would be justified in having recourse to the proposed operation,' I beg to inquire if you think the evidence thus afforded sufficient, and if not, what further proof you deem requisite to establish the safety and efficiency of my operation?—I am, Sir, your obedient servant, James Syme.

"The Editor of the London Medical Gazette."

VARIETIES.

Vagaries of the Insane.—Some forty years since, a poor discharged soldier, a fine looking man, in the prime of life, and of very military and dignified demeanour, who, we believe, had become insane in the West Indies from the effects of a severe flogging,—a punishment wholly unsuited to men of high spirit—appeared in the town of Mallow one day, attired in the cast-off uniform and cocked hat of an officer; his head-gear adorned with a plume of peacocks' feathers, and a ponderous wooden sword, in a leathern scabbard, dangling by his side. As this hero stalked through the streets, he gravely announced himself—"Colonel Jack, commander in chief of all the milk-women in Ireland." Next morning Colonel Jack visited the milk-market at an early hour, marshall-

ing its nymphs, between twenty and thirty in number, seated in line on their stools, their tin milk-pails, with the lids closed before them, and their measures duly arranged. Then standing in front of the amazons—for such were some, at least, if provoked—he gave the word of command in a loud voice—*Open pans;* on which, in accordance with their previous instructions, all the lids were raised. The gallant colonel forthwith minutely examined, not only the pails and measures, but the faces, hands, and dress of each lady fair; kicking over her milk without ceremony, if there appeared to be any want of attention to cleanliness. The women laughed, and endured their misfortunes with the utmost good humour; there were no "peelers" at the time to interfere; and the magistrates and town constables, when they heard of the matter, thought it not only a good but a useful joke. Daily, for six months, Colonel Jack attended morning parade; but, after the first significant demonstration of his antipathy to dirt, had little or no cause to inflict punishment. At the end of half a year, he disappeared as abruptly as he had made his appearance—no doubt on an extended *tour of inspection;* but what afterwards became of him we could never learn.—*Journal of Psychological Medicine*, 1st July 1851.

Liebig on Quackery and the Puff Medical.—We extract the following "morceau" from the "Annalen der Chemie und Pharmacie" for April 1851. It is headed as follows:—

"*Goldberger's Chains.*

"After accurate trial of Goldberger's galvano-electric rheumatism chain, and after comparing it with several specimens of apparatus made in imitation, the undersigned has found that Goldberger's chain, from its correct construction founded on scientific principles, far exceeds in its *healing influence* on the human organism every imitation with which he has become acquainted, and is eminently *distinguished by its superior advantages.* (Signed) "Dr Carl Sterz,
Royal Imperial Primarius Physician in the General Hospital of Vienna, Ordinary Member of the Medical Faculty of Vienna, and of the Royal Imperial Society of Physicians of Vienna."

Vienna, 30*th May* 1840.

From among the great number of testimonials granted by physicians in favour of the efficacy of Goldberger's rheumatism chains, we select that of Dr Sterz (as his public position may justify the belief that he is acquainted with the fundamental principles of electric science), for a few comments, which, as editors of a scientific journal, we feel bound to append. We do not know what scientific foundation he has discovered in the construction of these so-called chains (see "Annalen," vol. lxxiii., p. 376), or wherein their *advantages above other imitations* are conspicuous. All such certificates from physicians must be regarded as very melancholy signs of deficient conscientiousness, of ignorance, or of charlatanism. No physician or natural philosopher knows with exactitude anything about the efficacy of electricity in the healing of diseases, nor can he show how its workings are to be recognised. That many lesions spontaneously pass off after the lapse of months is universally known; but to ascribe such cures to the wearing of Goldberger's chains, betrays, on the part of the patient, a childish credulity, and on that of the physician, at least gross ignorance. That the sheep were born in this world to be shorn, and the fools to be gulled, has been a maxim from the earliest antiquity; but that the sly child of Israel, who by this ludicrous invention has netted a large fortune and colossal income, should find so many helpers, probably from simplicity, among the medical public, is a great shame. An English or French physician of repute and position would never have lent his name to the propagation of such nonsense."

Medical Walking Staff.—Dr Gray of Belhaven Place, Glasgow, has constructed a very neat and portable case of walnut in the form of a walking stick, it consists of five parts joined together by brass screws, is not much heavier than an ordinary oak staff, and contains a test tube and test paper, a small pair of forceps, wax matches, four pill boxes, an elastic male gum catheter, and an enema syringe.

DR BALFOUR'S MANUAL OF BOTANY.

Mr Griffin, the publisher of this work, has circulated a printed statement, from which it appears that the question between him and the author is more complex than we had supposed.

Nothing could be more remote from the object of this Journal or our own desire than an attempt to adjust the merits of a commercial difference, and we therefore regret having expressed any opinion upon this part of the subject.

In so far as the literary part of the Review is concerned, Mr Griffin may, of course, if he pleases, regard it as partial or unjust, but he is not entitled to characterize it by the term "sham," which implies intentional misrepresentation on our part.

It was upon this ground that we felt it necessary to decline inserting the advertisement of his pamphlet. Had the advertisement been offered to us with the same title which the pamphlet now bears, its insertion would not have been objected to.

PUBLICATIONS RECEIVED.

A Practical Treatise on the Diseases of the Lungs and Heart. By Walter Hayle Walshe, M.D., &c. London: Taylor Walton, and Maberly. 1851.

Hand-Book of Natural Philosophy and Astronomy. By Dionysius Lardner, D.C.L., &c. First Course, including Mechanics, Hydrostatics, Hydraulics, Pneumatics, Sound and Optics. London: Taylor, Walton, and Maberly. 1851.

The Laws of Health in Relation to Mind and Body. By Lionel John Beale, M.R.C.S. London: Churchill. 1851.

Compendium of Materia Medica and Pharmacy. By J. Hunter Lane, M.D., &c. Second Edition. London: Churchill. 1851.

Miss Martineau and her Master. By J. Stevenson Bushnan, M.D., &c. London: Churchill. 1851.

On Gout: its History, Causes, and Cure. By William Gairdner, M.D. Second Edition. London: Churchill. 1851.

The Wisdom and Beneficence of the Almighty as displayed in the Sense of Vision. (Actonian Prize-Essay for 1851.) By T. Wharton Jones, F.R.S., &c. London: Churchill. 1851.

The Present Tendency of Investigation in Medicine, an Address to Suffolk District Medical Society. By Samuel Parkman, M.D., &c. Boston: Clapp. 1851.

The First Step in Chemistry By Robert Galloway, F.C.S. London: Churchill. 1851.

Mémoires sur la Digitaline, par MM. Homolle et Quevenne. Rapports faits à l'Académie Nationale de Médecine le 8 Janvier et le 4 Fevrier 1851. Commissaires: MM. Rayer, Soubeiran et Bouillaud, Rapporteur. Paris. 1851.

Alcoholismus Chronicus, eller Chronisk Alkoholssjukdom af Dr M. Huss. Stockholm. 1851.

Om Nödvändigheten af Vetenskaplig Kontroll öfver Gymnastiska Central-Istitut-ed. Kritisk Trumställning af D ——. Stockholm. 1851.

Ophthalmologische Studien. Von Dr Eduard Krieger. Berlin. 1851.

Second Class-Book of Physical Geography. —Organic Life. By William Rhind. Edinburgh: Sutherland & Knox. 1851.

Observations on the Site and Construction of Hospitals. By Sir George Ballingall, M.D., &c. Edinburgh: Maclachlan & Stewart. 1851.

On the Reciprocal Agencies of Mind and Matter, and on Insanity. (Lumleian Lectures, 1851.) By J. C. Badely, M.D., Cantab. London: Churchill. 1851.

The New London Pharmacopœia, translated and arranged in a Tabular Form, with the Edinburgh and Dublin Pharmacopœias, with Practical Remarks. By Peter Squire, M.R.I. London: Churchill. 1851.

Braithwaite's Retrospect—January to June, 1851. London: Simpkin, Marshall, & Co. 1851.

A Medical Man's Plea for a Winter Garden in the Crystal Palace. London: Van Voorst. 1851.

Singular Specimens of the Edinburgh Practice of Criticism. By John Joseph Griffin. London & Glasgow: Griffin & Co. 1851.

TO CORRESPONDENTS.

Communications have been received from Dr Morton, Glasgow; Dr Kilingner, Portsoy; and Dr Redfern, Aberdeen.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Healing of Wounds in Articular Cartilages, and on the Removal of these Structures after Amputations at the Joints, with Remarks on the relation which exists between the Diseases of Cartilage and Ulceration and Inflammation in other Textures.* By P. REDFERN, M.D., F.R.C.S.L., Lecturer on Anatomy and Physiology, and on Histology, at the University and King's College, Aberdeen.

(*Read before the Medico-Chirurgical Society of Aberdeen, July* 3, 1851.)

HAVING previously shown[1] by the result of about ninety observations made on the cartilages of the lower animals, that all changes which can be induced in them by experiments are precisely similar in their essential characters to those which are observed in the human body, I can with increased confidence refer to other actions observed to take place in the structures of the former, with a view to elucidate such as we have very rare opportunities of examining in the instance of the latter. I shall especially refer—1st, To the healing of wounds in articular cartilages, and show that this undoubtedly takes place, and that cut surfaces kept in apposition become firmly adherent by the formation of fibrous tissue out of the substance of the adjacent healthy cartilage, the union being so perfect that, after a short time, it is quite impossible to detect the injured spot with the unassisted eye; and, 2dly, To the process by means of which the removal of articular cartilages is effected after amputations at the joints, pointing out that the changes which cause this are such as are frequently observed in the cartilages of man and animals under other circumstances, and that the cartilage itself becomes converted into a layer of fibrous tissue, which constitutes a permanent covering for the end of the bone. I shall also endeavour to point out

[1] Anormal Nutrition in Articular Cartilages, with Experimental Researches, &c. Edinburgh, 1850.

the relation which exists between the diseases of articular cartilages and ulceration and inflammation in other textures.

Healing of Wounds in Articular Cartilages.—In my former observations, I found that from the 37th to the 49th day after incisions had been made into the articular cartilages of dogs, adhesion had taken place between the cut surfaces to such an extent as to render it easy to make thin sections through both and leave them attached. The uniting substance was composed of a granular mass, containing very imperfect fibres, and a number of corpuscles not to be distinguished from the nuclei of cartilage cells. The cartilage cells lying near the cut surfaces were very much enlarged, rounded or oblong in shape, and filled with corpuscles; in several places they projected into the mass between the cut surfaces, and were therefore in a position to discharge their contents into it whenever the cell-wall should offer too little resistance to their escape. The inter-cellular substance of the same part was somewhat softened and more transparent than ordinary, and presented indistinct traces of fibres. From these appearances, discovered on examination, and shown in Fig. 1, I concluded that the stimulus of the incision had produced

Fig. 1.

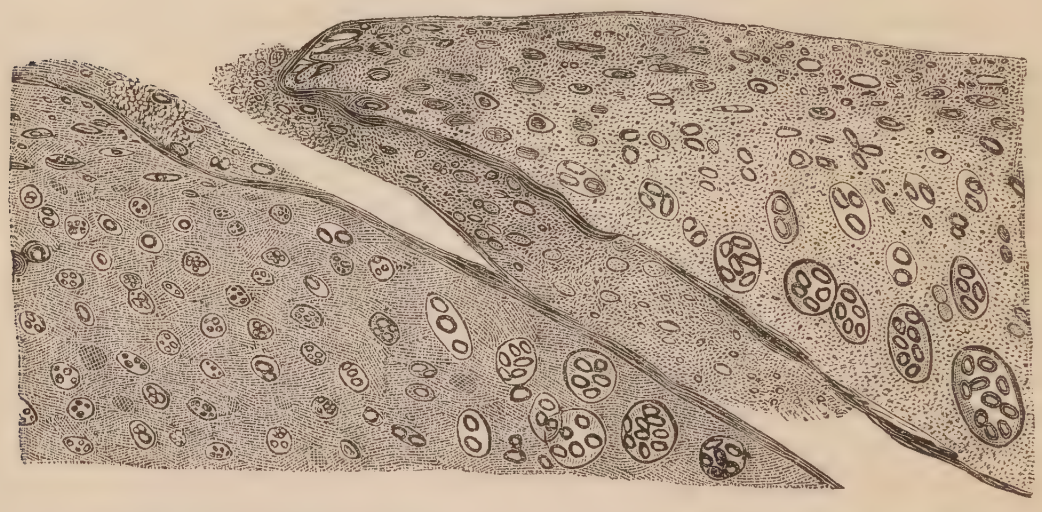


Fig. 1.—Section of the cartilage of the patella of a dog passing through an incision made forty-nine days before death.

an anormal development of the cells of the part through which it had passed, whilst the inter-cellular substance having become softened, received the contents of the cells after the destruction of their walls; and that this process, so similar in every respect to that by which a fibrous membrane is formed from diseased human articular cartilage, would terminate in a similar way, by forming a dense fibrous mass, with its fibres leading into the substance of the cartilage, and therefore attaching the opposed surfaces firmly to each other. This, however, was not proved, for the fibres observed were very imperfect, and the uniting medium was too soft to remain

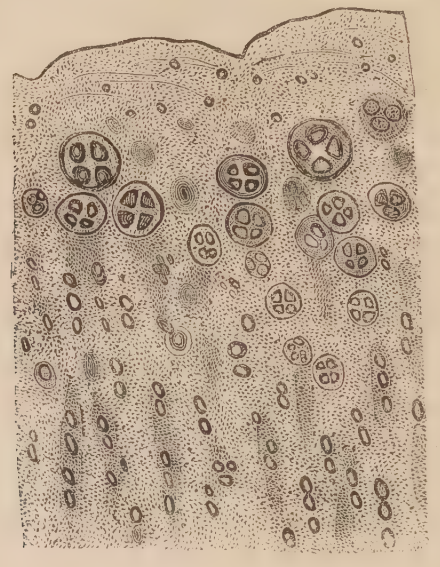
as a permanent structure; I therefore instituted other experiments, which were as follow:—Three incisions were made across the cartilage of the patella and obliquely from below upwards, in a young dog, and two others were made vertically and from the centre towards the circumference in the cartilage of the trochlear surface of the femur. The patella became dislocated on the inner condyle of the femur, and the dislocation returned instantly after reduction, so that it was allowed to remain. The dog suffered considerably, and could not put his leg to the ground for a fortnight, after which, slight lameness continued for some time, and then almost altogether disappeared, except when any unusual movement was produced. The joint was examined at the end of twenty-four weeks and five days, and presented the following appearances:—A slightly increased quantity of synovia exists in the joint; the patella rests by the external half of its articular surface on the inner side of the internal condyle of the femur, and the part of the fibrous capsule of the joint which lies upon the trochlear surface of the femur and glides over it, presents a dense, white, and smooth spot, similar in appearance to fibro-cartilage, and of exactly the same size and shape as the trochlea. The edges of the trochlear surface of the femur, and the vertical ridge on the cartilage of the patella, are less prominent than usual, and rounded. The cartilage of the femur appears perfectly healthy, that of the tibia somewhat softer than natural, and the cartilage of the patella is more transparent, and has a small perforation in its centre leading down to the bone. The position of the incisions is difficult to see on the patella, and is only marked by the slightest curvilinear depressions; on the femur, one incision is recognised with the greatest difficulty, and the position of the other cannot be seen with the naked eye.

On microscopical examination, the superficial cells over a considerable extent of the surface of the cartilage of the patella, and especially in the neighbourhood of the central depression, have become much enlarged, many measuring $\frac{1}{1000}$th of an inch in diameter; they are nearly spherical, and their contents appear to have divided into three or four masses like nuclei; they are lodged in hyaline substance of greater transparency, and much greater softness than ordinary, so that the cells can be pressed out of it. The appearance of the surface is in fact very similar to that shown in Fig. 2, and formerly described as showing the structure of the superficial layers of the cartilage of the tibia of a dog at a spot in which increased pressure had been made.

In the neighbourhood of the incisions into the cartilage of the patella, and upon the trochlear surface of the femur, though not extending upon the condyloid surfaces, the substance of the superficial layers of the cartilage has become converted into a membrane composed of granules, nuclei, and fibres. The formation of this membrane may readily be traced by an examination of it at the line of junction of the trochlear and condyloid surfaces, where it becomes

thin before it ceases to be observed. The contents of the cells first become granular, and the hyaline substance soft and more trans-

Fig. 2.



parent; the walls of the cells disappear gradually, and scarcely anything but cell-membranes, granules, and molecules lying in a hyaline mass, can be seen; fibres are then formed between the nuclei, and the latter either disappear altogether or elongate into nuclear fibres, and thus a dense fibrous mass is produced. If, during this process, the cells have become larger than natural, they give to the junction of the cartilage and membrane a notched appearance, by bursting and discharging their contents into the latter; but if no enlargement has taken place, the textures run into each other so insensibly that no distinct line of demarcation can be drawn between them.

The portions of the texture through which incisions were made present similar appearances in every instance. Not the slightest difficulty is experienced in making sections through both the cut surfaces and the substance by which they are firmly united. Such sections show the cut surfaces to be very uneven and hollowed into small pits of the size of the cartilage cells of these parts. The pits are obviously produced by the half-destroyed cells, the former contents of which are now seen lying on the surface. No evident change has taken place in the texture of the cartilage at a little distance from the cut surfaces, except that here and there the inter-cellular substance presents a fibrous appearance. There is no obvious enlargement of the cells, or crowding of their interior with corpuscles, as is frequently seen to occur under similar circumstances. The substance uniting the cut surfaces consists of a hyaline, granular, and indistinctly striated mass, in which there are numbers of rounded, oblong, elongating, or irregularly-shaped corpuscles,

varying in size from $\frac{1}{5000}$th to $\frac{1}{2300}$th of an inch in diameter, those in the adjacent cartilage measuring more uniformly about $\frac{1}{3000}$th of an inch. The corpuscles are more numerous in the substance connecting the cut surfaces than in the cartilage itself, and the inter-cellular substance of the same part is slightly more transparent than that of the adjacent texture, the appearance of fibres being most distinct in those parts which are directly in contact with the cartilaginous surfaces. (Fig. 3.) In sections obtained from the cartilage of the femoral trochlea the mass between the cut surfaces is less transparent, its fibres are much more perfect, and the corpuscles are smaller and less easily seen. (Fig. 4.) The fibrous and nucleated membrane formed on the surface of the cartilage is continuous with the uniting medium, and differs from it merely in having its fibres parallel to the surface.

Fig. 3. Fig. 4.

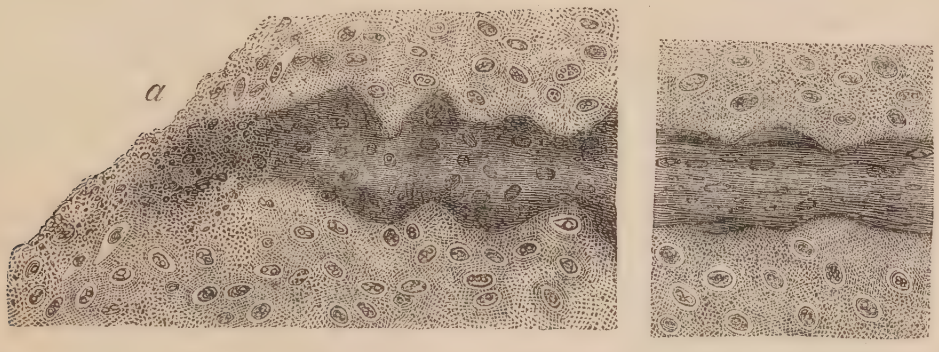


Fig. 3.—Vertical section of the cartilage of the patella of a dog passing through an oblique incision made twenty-four weeks and five days before examination, and showing perfect and firm union of the cut surfaces.

Fig. 4.—Section of the cartilage of the femur of a dog passing through an oblique incision made twenty-four weeks and five days before examination, and showing the perfect fibrous tissue developed in the healing of the wound.

It may be here remarked, that the lameness which occurred in this case is by no means to be viewed as the result of the injury to the cartilages; for except when dislocation, or some other accidental occurrence, takes place, incisions in the articular cartilages of the knee-joints of dogs are attended with no lameness or evidence of suffering whatever, after the first few days which are required for the healing of the external wound. Neither is it to be supposed that articular cartilages are in a favourable position for the healing of wounds; for, independently of the continued movement, which is no less detrimental here than in the healing of ordinary wounds, many actions which take place in these textures proceed very slowly, and, therefore, are in greater danger of being interfered with in their progress. In an instance in which I made three incisions into the cartilage of the patella, and two into that of the trochlear surface of the femur, no adhesion had taken place in any of the instances when the parts were examined twenty-nine weeks afterwards, though no inflammation of the joint, dislocation, lameness, or other apparent cause for the want of union had occurred. Yet some

of the cut surfaces were in such close contact, before the parts were examined by means of sections, as to lead to the supposition that union had taken place. On the examination of these wounds, no effused matter of any kind appeared on the perfectly smooth cut surfaces; but as the cells near to them in the substance of the cartilage were obviously enlarged and rounded, having in their interior three or four corpuscles into which their nucleus appeared to have divided, or a number of bright granules, there appears reason to believe that union might still have occurred had more time been allowed.

After the foregoing observations, I no longer entertain the slightest doubt that wounds in articular cartilages are capable of perfect union by the formation of fibrous tissue out of the texture of the cut surfaces. The essential parts of the process appear to be the softening of the inter-cellular substance of the cartilage, the release of the nuclei of its cells, the formation of white fibrous tissue from the softened inter-cellular substance, and of nuclear fibres, by the elongation of the free nuclei. It does not appear necessary that the cells should become much enlarged or crowded with corpuscles, as they are seen in Fig. 1, for the union represented in Figs. 3 and 4 is still more perfect, and it took place without any enlargement of the cells, or increase in the number of their corpuscular contents; these changes, therefore, though they may contribute materially in some cases to the rapidity of completion of the process under consideration, are clearly not essential in any of its stages.

Removal of Articular Cartilages after Amputations at the Joints.—The foot of a young dog was amputated at the ankle joint, and treated in the usual way; union by the first intention took place and was complete in a few days, after which the animal appeared in perfect health, and went about as if nothing had happened, only occasionally bringing the amputated limb to the ground. The part was examined at the end of forty-six days, and presented the following appearances:—The cicatrix is firm, and adheres to the end of the bones. Two or three of the ligatures applied upon the vessels are enclosed in small dense cysts containing a small quantity of reddish fluid. All the divided tendons have elongated swellings upon them near their ends. The swellings are composed of dense fibrous tissue with fusiform cells, and present a large number of nuclei after the action of acetic acid. Under the skin a considerable layer of very dense fibrous tissue covers the ends of the bones, and adheres very strongly to the cartilage, but can be detached in several places if much force be employed. The cartilage is but little thinner than usual; its deep surface presents a few calcified cells, but in other respects it appears healthy. The cells of the superficial layers are enlarged, many are rounded, and measure about $\frac{1}{3700}$th of an inch in diameter, others remain of the elongated form, and measure $\frac{1}{1850}$th by $\frac{1}{5000}$th to $\frac{1}{2460}$th of an inch, one measures $\frac{1}{1000}$th by $\frac{1}{1500}$th of an inch. Each cell contains a large nucleus and a number of bright granules, varying in size from $\frac{1}{10,000}$

to $\frac{1}{5000}$th of an inch. The inter-cellular spaces are much greater than in the superficial parts of the corresponding cartilage of the opposite limb, and are occupied by hyaline substance in which there are traces of fibres, which are continuous with those of the fibrous covering of the end of the bones. On proceeding in the examination towards the skin, the walls of the cartilage cells gradually disappear, and their contents—nuclei and granules—become mixed with the fibres of the fibrous tissue, where the nuclei can be seen distinctly elongating to form nuclear fibres, especially after the application of acetic acid. The formation of white fibres in the intercellular substance, quite independently of the cells, is also distinctly observed, so that there can be no doubt that the cartilage substance is converted into fibrous tissue, of which the white fibres are produced by transformation of the hyaline substance, and the yellow or nuclear fibres by elongation of the nuclei of the cartilage cells. The appearance of the cartilage at the point of connection with the fibrous membrane, is almost precisely similar to that which forms on the surface of costal cartilages in the neighbourhood of parts irritated artificially, and is represented in Figs. 5 and 6.

Fig. 5.

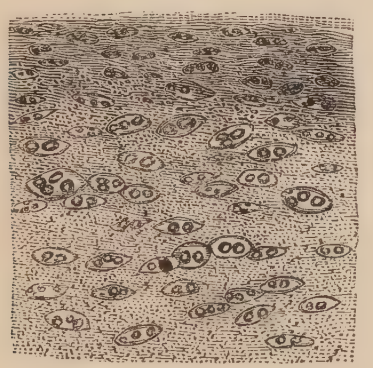


Fig. 6.

A different process, which, however, there is good reason to believe would have terminated in the same manner, occurred in an instance in which the leg of a dog was amputated at the knee-joint twelve weeks before the examination of the parts. The patella was left, but its lateral connections were sufficiently loosened to allow it to be drawn upwards on the thigh. The stump healed completely by the first intention in a few days; the animal got very fat, and did not manifest the slightest uneasiness when the part was freely handled, or at any other time. The following were the appearances:—The patella lies loosely on the surface of the femur, half-an-inch above the edge of its trochlear surface. The cicatrix is firm, and the covering of the end of the bone perfectly healthy, and very moveable everywhere, except in one spot, a line in diameter, near the lower part of the external condyle of the femur, where a little oozing occurs, and a firm adhesion to the deeper parts exists. The

muscles of the thigh are healthy; the fibres of the gastro-cnemius are pale, soft, and flabby. The integument can be dissected with the greatest ease from the subjacent parts, except at the spot above named. A thin, but very strong membrane, covers the articular surfaces of the femur, and its interior is lined by a synovial membrane which extends upwards covering the articular surface of the patella, is reflected upon the femur half-an-inch above its trochlear surface, and passes beyond its articular surfaces so as to cover a large portion of the lateral aspect of each condyle. A few adhesions exist between this synovial membrane and the cartilages of the condyloid surfaces of the femur and of the patella, at a little distance from the edges of the surfaces.

A superficial inspection of the cartilage might lead to the belief that a large portion of it is unchanged, but a more careful examination by the naked eye, or by the aid of a lens, shows that almost every portion of the surface is marked by a number of very small pits, producing something of the appearance of sand paper. The pits are very distinct on the patella, and on every part of the surface of the femur, except on the deepest part of its trochlea. They are obviously produced by the irregular removal or destruction of the superficial layers of the cartilage. Near the outer edge of the external, and the inner edge of the internal condyloid surface, there is a much more obvious destruction of tissue over an elongated surface of one-fourth of an inch in extent on each side. On the outer condyle the patch just named is sprinkled over with red spots and lines (blood-vessels seen through thinned and transparent cartilage), most numerous at the centre of the patch, where, also, are a number of small pits, erosions, or ulcerations, separated from each other by septa of less changed tissue. On the inner condyle the patch presents a distinct and continuous erosion, with well-defined edges, and a base sprinkled with red spots and lines, and marked by small reddish prominences, presenting, in fact, precisely the appearance of a granulating ulcer in any other tissue. The adjacent surface of the cartilage at the posterior extremity of this patch is marked by small pits or erosions, reddened at their deepest parts like the corresponding spots on the opposite side.

Fig. 7.

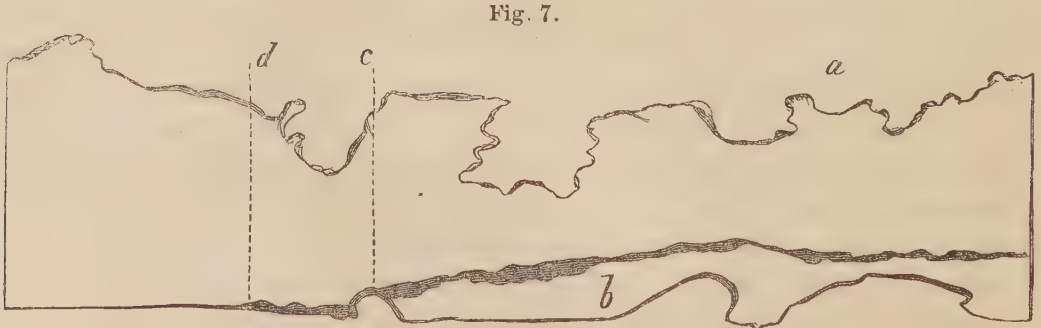


Fig. 7. Outline of a section showing the small pits or erosions of the cartilage on the inner condyle; *a*, the irregular free surface; *b*, a calcified layer of cartilage at the attachment to the bone. Magnified 66 diameters.

Microscopical Examination.—The muscular fibres, which are paler than natural, have indistinct longitudinal and transverse striæ; acetic acid acts more readily upon them, producing greater transparency, and showing the nuclei more distinctly than in healthy muscle. There appear to be more fat vesicles amongst the fibres than are found in the muscles of the healthy limb.

The superficial cells of the cartilage of the inner condyle are parallel to the surface, considerably enlarged, measuring as much as $\frac{1}{750}$th by $\frac{1}{2500}$th of an inch, finely granular, and, in some places, seen with difficulty, owing to their great transparency. They are clearly seen to open and discharge their contents, thus destroying the texture, and leaving the surface uneven and notched. The cells and inter-cellular substance of the deeper parts have their healthy arrangement and appearance as far as the eroded or ulcerated spots, on the edges of which the inter-cellular substance becomes softened and more transparent, allowing the cells to escape on the surface, where they also degenerate and are seen to be irregular, shrivelled, losing their walls and discharging their contents, or reduced to mere granules,—the whole texture being thus removed by ejection of variously-sized particles on the surface. Where the surface presents numerous small pits or erosions, the superficial parts of the cartilage have disappeared, and the deeper parts are undergoing the changes just named. (Fig. 8.)

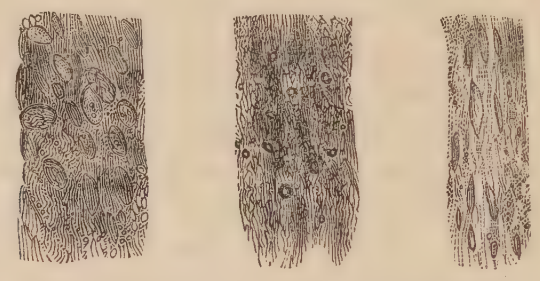
Fig. 8.

Fig. 8. Portion of the section shown in Fig. 7 between the lines *c* and *d*, magnified 200 diameters, and showing the gradual ejection of the tissue of the cartilage at the ulcerated spots.

The texture forming the surface of the large ulcer is composed of a dense mass, in which cells, granules, and imperfectly-formed fibres are found in various proportions in different parts. In some places there are patches, in great part made up of cells $\frac{1}{2200}$th by $\frac{1}{5600}$th of an inch in size, finely molecular, but difficult to discover from the granules and imperfect fibres in which they lie (Fig. 9); in other

places, elongated dark patches are the only indications of the existence of cells which have become further changed, and the fibrous mass appears more perfect (Fig. 10). When acetic acid is added to parts like Fig. 9, the mass becomes more transparent, and cells with a few granules are more distinctly seen; after the addition of the same acid to parts like Fig. 10, the granular and fibrous mass becomes hyaline and gelatinous, and numerous elongated cells or nuclei appear, which measure from $\frac{1}{2000}$th to $\frac{1}{1600}$th of an inch in length, by $\frac{1}{10,000}$th in width (Fig. 11).

Fig. 9. Fig. 10. Fig. 11.



Figs. 9 and 10. Fibrous layer at the bottom of the ulcer, showing its mode of formation.
Fig. 11. Portion of the fibrous membrane similar to Fig. 10, acted on by acetic acid.

On every part of the surfaces of the femur and patella, where any microscopical change has taken place, an appearance of erosion exists when the part is very closely examined by the naked eye; and whenever any change has occurred, it is marked by similar characters. On proceeding from a healthy towards an eroded part, the first change noticed is a darkening and indistinct appearance of the superficial layers continuing until a fibro-nucleated membrane becomes evident on the surface. This membrane consists of imperfect fibres formed by the breaking up of the inter-cellular substance, intermixed with small cartilage cells, and the discharged nuclei or granules of larger ones. Acetic acid renders the whole of the fibres transparent, and brings into view the nuclei, which are seen to be elongating to form nuclear fibres. In the absence of erosions, the membrane appears to cover the surface for some time; but when erosion commences, the indistinct fibres and the hyaline substance which remains become gradually softened, and the cells, nuclei, and granules thus set free, fall into the interior of the joint, and are probably removed by absorption. When the process has reached the deeper parts of the structure, these become in like manner converted into fibrous tissue, which remains, in many cases at least, to constitute a permanent covering for the end of the bone.

The conclusion to be drawn from the foregoing observations is, that when articular cartilage is rendered useless, and has been relieved from pressure by amputation through a joint, it gives place to another tissue, which constitutes part of the general covering for the end of the bone. The newly-formed tissue is fibrous, and composed of white fibres originating in the splitting of the inter-cellular sub-

stance, and of yellow or nuclear fibres produced by elongation of the nuclei of the cartilage cells. Or, should the synovial membrane opened by the amputation become completed, and again converted into a closed secreting sac during the cicatrization, so that free movement is allowed between the end of the bone and its fibrous covering, still there is no use for a covering of elastic cartilage on the end of the bone, and it becomes removed altogether, or partially converted into fibrous texture, as happened in the last observation.

Comparison of the diseases of Articular Cartilages, with the processes of Ulceration and Inflammation in other textures.[1]

Ulceration.—It may be useful to examine in this place the nature of the process by which portions of articular cartilage are removed during ulceration or erosion, with a view to ascertain whether it is in any respects similar to known processes occurring in other textures, and especially to the ordinary process of ulceration.

I think it will be sufficiently evident, from the observations now detailed, and from those formerly published,[2] that the essential parts of the process of removal of articular cartilages (ulceration) are, —*the softening of the inter-cellular substance, and the release of the cells, very often attended with the escape of their contents by the destruction of the cell-walls*, all the parts of the tissue being thus ejected on the ulcerating surface: whilst the cure consists in the transformation of the inter-cellular substance, and of the nuclei of the cells of the adjacent cartilage, respectively into the white and yellow tissue of the fibrous cicatrix. In different instances, the cells enlarge in very various degrees before they open, and their contents are,—1st, one, two, or more ordinary nuclei; 2d, the ordinary nuclei with the addition of bright, highly refractive granules, oil globules, or molecular matter; or, 3d, irregularly rounded corpuscles, from six or eight to sixty or more in number; whilst the inter-cellular substance softens into a gelatinous and finely molecular mass, which gradually disappears by its particles falling off on the surface; or, 2d, it is converted into an imperfectly fibrillated mass, destined shortly to become disintegrated; or, 3d, it is resolved into a more perfect fibrous tissue, which is to form part of the cicatrix.

Ulceration in other tissues consists in the gradual detachment and ejection of dead pieces of their substance visible to the naked eye (sloughs),—in the falling away of changed portions of texture, so small as only to be recognised by the microscope, as in caries,—or in the discharge of a multitude of granules or molecules into which the part has degenerated. Being generally accompanied by inflam-

---

[1] It must be fully understood here, that what follows is to be held as applicable merely to diseases of cartilage, which has not previously suffered any structural change, and to such diseases as are not complicated by affections of the neighbouring textures.

[2] Op. citat.

mation in tissues furnished with blood-vessels, part of the inflammatory exudation, consisting of blastema, granules, and cells, is thrown off, together with the disintegrated elements of the part, and these constitute together a very evident discharge.[1] The cure consists in the conversion of the nucleated cells of the inflammatory exudation into the white and yellow fibrous tissue of the cicatrix, the cells splitting to form the white, and the nuclei elongating to form the yellow fibres.

The process of ulceration in cartilage, and other tissues, is, therefore, precisely the same if we regard merely the actions of the essential elements of the textures;—in both, softening and disintegration occur, with ejection of the molecular or granular matter into which the part has been reduced; in both, small particles of the tissue are thrown off, and are capable of recognition by the microscope; in both, larger portions of the texture, visible to the naked eye, are at times detached, in the one case, in the form of sloughs of dead tissue, in the other, in that of large portions of much less changed cartilage; lastly, in both instances, the process may go on with extreme rapidity, or it may make scarcely perceptible progress for a very considerable period.

The points of dissimilarity are these,—articular cartilage contains no blood-vessels, consequently ulceration and other changes of structure confined to it are never complicated by inflammatory exudations, pus, &c., and can be examined very satisfactorily; ulceration in tissues containing blood-vessels is almost always complicated with inflammation, and the ejected particles of the tissues having undergone a very complete degeneration, and become mixed with a mass of structures formed in the inflammatory exudation, also degenerate, are, therefore, very difficult to recognise. Ulceration or other disease confined to articular cartilage has in no single instance been shown to be productive of pain,[2]—no doubt for the very simple

---

[1] For a very excellent view of the whole process of ulceration, I would refer to Professor Paget's fifth Lecture on Inflammation, Professor Miller's "Principles of Surgery," Ed. 1850, p. 143, and to Professor Bennett's Treatise on Inflammation, Ed. 1844, p. 46.

[2] It is somewhat gratifying to find that the decided statements formerly made regarding the diagnosis of disease of articular cartilages have recently undergone modification, in two very important quarters at least. Sir B. C. Brodie, at p. 192 of the fifth edition of his work on "The Diseases of the Joints," April 1850, in speaking of the pain which exists during ulceration of articular cartilages, adds, "Still I am myself more inclined to the opinion, that the increased sensibility in these cases is in the bony plate beneath the cartilage rather than in the cartilage itself, and that the presence of severe pains, with involuntary startings of the limb, is always to be regarded as a sign of the bone partaking of the disease." And Professor Miller, at p. 510 of the second edition of his "Principles of Surgery," 1850, says, that simple destruction of cartilage leads "to little pain, or perhaps none, if the cartilage alone be involved;" and again, at p. 512, after repeating his former statement, that "the symptoms of destruction of cartilage, although invariably indicative of that affection, are not

reason, that these structures contain no nerves; in other textures, ulceration is often attended with very severe pain from implication of the nerves ramified in their substance.

The *healing of ulcers* in articular cartilages, and in tissues freely supplied with blood-vessels, differs in this remarkable particular, that in the former, the fibrous cicatrix is invariably made up of the changed substance of a portion of the cartilage not subjected to the process of ejection,[1] and in the latter, the cicatrix as constantly results from the development of the inflammatory exudation. This difference admits of the following explanation: in the case of the ulcer affecting no texture but that of cartilage, there is no other matter from which a cicatrix can be produced than the actual cartilage tissue remaining; and it may be remarked, that cartilage is a very simple cellular texture, and that the greater number of the tissues are originally formed from cells,—that the cells of cartilage have a natural tendency to transformation into fibre, as is shown in the development of fibro-cartilage,—and that the inter-cellular substance shows as remarkable a disposition to produce the same result in its diseased state. In the ulceration of tissues supplied freely with blood-vessels the reverse maintains; for the degeneration and destruction of the tissue actually involved is so complete, that this can in no way assist in the formation of the cicatrix; and there is, therefore, nothing left in this instance but the inflammatory exudation from which that substance can be formed.

Inflammation.—In now directing attention to the process of inflammation, to ascertain how the diseases of cartilage stand in relation to it, it is, in the first place, absolutely necessary to state what is meant by inflammation,—for, though we may not be able to frame a definition of it which will be universally acceptable, and which will exclude tubercular, cancerous, and other morbid growths, every man is bound, in the use of a particular term, to intimate what he means by it, if it be one which has no general acceptation.

What is to be understood here by the term inflammation, is "*a peculiar perversion of nutrition or of secretion,*" *attended with certain changes in the blood and blood-vessels, and including exudation as its most important and characteristic phenomenon.*[2] This definition in-

---

always indicative of its amount, because not uniformly proportioned thereto," adds, "They rather indicate the amount of disease in which the other textures of the joint have been involved."

[1] When the whole thickness of the cartilage is thrown off without the occurrence of disease in the neighbouring parts, a fibrous membrane has never been found on the end of the bone, and the only cure which has been observed is that by eburnation or porcellanous transformation on the articular surface of the bone.

[2] Professor Alison first pointed out that a peculiar perversion of nutrition or of secretion is essential to the very existence of inflammation, and that effusion from the blood-vessels must be included in our general notion of this process. Dr Bennett subsequently showed in what manner inflammatory exudations become developed, and that the perversion of nutrition which establishes the process exerts its influence in very many instances until its completion.

dicates sufficiently well for our present purpose the nature of the process understood in ordinary professional language and in practice as inflammation, but it does not separate cancerous and tubercular exudations, &c., from those which take place in ordinary inflammation in healthy persons; nor is this to be expected,—for, as there is scarcely any healthy structure, in man or other organised beings, which is capable of being separated from all the rest by definite lines of demarcation, it is not to be expected that diseased structures and the phenomena which occur in them will be capable of subjection to strict limitation and definition. As it is clear, however, that inflammation, and every process which is at all allied to it, are strictly processes of anormal nutrition, it is necessary to inquire, What are the essential conditions of nutrition in general?

Healthy nutrition requires a supply,—1st, Of material of a certain definite amount and quality; and, 2d, Of vital force competent to convert this into tissue.

Hypertrophy consists in an increase in the amount of material and of organising force.

Atrophy results from a deficiency in the amount of material, or of both material and vital force.

Anormal nutrition is produced by various alterations in the quality of the material supplied, and in the nature and action of the organic force,—conditions which are generally, but perhaps not necessarily, associated.

Now, for the establishment and continuance of these processes, it is not of the least importance whether,—1st, the nutritive material be supplied directly from without, as in the lowest animals and plants, or be carried to particular parts by means of a circulating system, for, in either case, when material is supplied of a certain definite quantity and quality, the first condition is fulfilled; nor, 2d, does it matter whether the vital or organic force belong directly to a particular tissue, constituting its only distinction from dead matter, or be communicated to it or controlled in its action by a special nervous system; if the force exist, it is enough for the second condition.

When, therefore, we are engaged in the examination of the healthy or diseased states of the lowest tribes or tissues of organised beings, we have nothing to attend to but the structure and mode of action of the part itself; but when we take up a part which contains living vessels and nerves, as well as its own proper texture, the examination becomes a very complicated one, and requires a thorough investigation of the nature of the changes in structure and action of every part of which the compound body is composed.

I think that the demonstration has been fully made, that every morbid action which takes place in the structure of cartilage is referable to an anormal nutrition of its texture, and, in so far, all these morbid actions resemble inflammation: they differ from it, 1st, in not being attended with exudation, because the texture in which

they occur contains no blood-vessels; and, 2d, in not giving rise to pain on account of the absence of nerves.

If, therefore, inflammation be *merely* a process of anormal nutrition, it takes place in cartilaginous as well as in other textures; but if we include *exudation* as an essential phenomenon of inflammation, it never affects the human articular cartilages, which contain no blood-vessels, and present no exudation in disease.

So with ulceration,—if we are to separate the softening, degeneration and ejection of tissue in the formation of an ulcer from the exudation, which is an almost constant attendant on this process in vascular tissues, and forms cicatrices in them, then ulceration in cartilage and in other tissues is identical, though the method by which ulcers heal in the two forms of texture is altogether different; if, on the other hand, we include in the term ulceration, the production of exudation and its transformation into the tissue of the cicatrix, then it is equally clear that ulceration in cartilage and in vascular tissues differs in the occurrence of those phenomena in the latter case and not in the former.

Finally, I would call attention to the following conclusions:—

1st. Wounds in articular cartilages heal perfectly by the formation of fibrous tissue out of the cut surfaces.

2d. The fibrous cicatrix consists of white and yellow fibres, which are formed out of the inter-cellular substance of the cartilage, and out of the nuclei of its cells respectively.

3d. Articular cartilages disappear after amputation at the joints, either by being transformed into fibrous tissue, which is mixed with that of the cicatrix,—or by slow ejection of their particles into a newly completed synovial sac.

4th. Ulceration in articular cartilages differs from that in other tissues, in neither being accompanied by exudation, nor attended with pain,—differences which depend on the absence of vessels and nerves.

5th. Ulcers in articular cartilages heal by transformation of the surrounding cartilage tissue into fibre, but those occurring in other textures are cured by the formation of a cicatrix out of newly exuded blood plasma.

6th. Inflammation, regarded as a process of anormal nutrition, attended with changes in the blood and blood-vessels, including exudation as an essential phenomenon, does not occur in articular cartilages in man, simply because these textures contain no blood-vessels.

7th. The whole diseased states of cartilage are referable to a changed or anormal nutrition of the texture, and to this alone; when unaccompanied by disease in other textures, they produce no pain or other symptoms by which they can be recognised, and have much less surgical importance than they have for many years been supposed to possess.

ARTICLE II.—*On the Metamorphosis of the Coloured Blood-Corpuscles and their Contents in Extravasated and Stagnant Blood.* By JOHN SCOTT SANDERSON, M.D., Vice-President of the Physiological Society of Edinburgh.

THE question of the function of the spleen has probably been hitherto investigated with less satisfactory results than that of any other organ of equal importance. The object with which the following observations are brought forward is not to enter on a field of inquiry so unpromising of good results, but simply to bring additional evidence to that which has been already recorded to prove that neither of the two theories of the function of the spleen, which have attracted most attention during the last few years, are tenable, and, at the same time, to indicate the real signification of the facts upon which they are founded. This evidence is derived not only from what may be observed in the structure of the organ itself in its healthy and morbid conditions, but still more from the fact that the very same appearances as those to which so much importance has been attached as they occur in the spleen, present themselves in other organs and tissues under similar conditions.

The two theories above referred to, it is scarcely necessary to state, though antagonistic to each other, agree in supposing the existence of a physiological relation between the spleen and the coloured corpuscles of the blood. According to the one, it is the function of the organ to form, according to the other, to destroy, these corpuscles. The decision of the whole controversy depends on the signification which is to be assigned to certain structures, which observers holding both opinions concur in considering as normal constituents.

I shall not occupy space with any lengthened detail as to the progress of inquiry on the subject before us, but shall give a short abstract of the opinions at present maintained, referring those who desire further information to the original papers.

In the spleen of man and the mammalia, and especially in that of reptiles and fishes, certain cell-like[1] structures have been frequently observed, apparently containing in their interior coloured blood-corpuscles. These are distinguished from the ordinary cell forms of which the organ is principally constituted, by their much greater size, their peculiar contents, and very different general appearance. They are described by Ecker as occurring in the spleen of the calf under the following forms:—1st, Cells existed about $\frac{1}{700}$th of an inch in diameter, or rather larger, containing one or two

[1] The term "*Cells* containing blood-corpuscles," as applied to the structures in question, having been used by all previous observers, I shall avail myself of it in the following pages, although, as we shall see, it is probable that, in many instances, they are neither anatomically nor physiologically cells.

blood-corpuscles, the rest of the cell being either quite clear or finely granular. On adding water to these, Ecker observed that the membrane burst, and the corpuscle having escaped became clearer and disappeared. 2d, Large cells more than $\frac{1}{300}$ths of an inch in diameter, which were filled with shrunk blood-globules of an intense yellow colour, and no longer capable of being affected by the action of water.—(See Ecker, Zeitschrift für Rationelle Medecin, bd. vi. s. 241.)

Professor Kölliker represents these cells (blut-körperchen-haltenden zellen) as originating, not from a nucleus, but by the formation of a membrane round a heap of agglomerated blood-corpuscles, in the same way "as the inflammatory globules of Gluge convert themselves into cells," or as the "smaller globules of fission in the yelk form themselves into vesicles." He, however, believes that the nuclei, which either originate previously to the process of the aggregation or as a consequence of it, are directly concerned in affording the conditions necessary for the development of a cell-membrane. Kölliker succeeded in detecting unchanged blood-corpuscles in man, the rabbit, the sheep, calf, and dog, in all of which he found the number of included globules to be from one to twelve. By the shrinking together and falling to pieces of these, he considered that "granular cells" filled with golden yellow or rusty brownish-yellow contents were formed, which were destined, by the further breaking down of the granules and their final disappearance, to become entirely colourless.

The fact that the coloured corpuscles of blood which is stagnant or has been extravasated, are apt to undergo a more or less complete conversion into a peculiar form of granular pigment, is acknowledged by all observers. The occurrence of cells containing the form of pigment, not only in the spleen, but in other organs, is also generally admitted. With respect, however, to the relation which these cells bear to the process of transformation, there is more difference of opinion. Thus, while Vogel believes that pre-existing cells form within themselves by their metabolic power, pigment granules as contents, Bruch, on the other hand, maintains that the pigment granules originate before the cells; globules being first formed by the union of elementary granules, which become infiltrated with hæmatine escaped from blood-corpuscles, a nucleus being finally formed and then a surrounding membrane, (Vogel, Path. Anat., Translation by Day, p. 191. Bruch, Untersuchungen ueber das körnige Pigment, 1844, s. 50.) Rokitansky (Allg. Path. Anat., s. 301) maintains a doctrine analogous to that of Vogel.

Another class of observers, who entirely deny that there is any evidence of the existence of the process of transformation above alluded to, assign to the cells containing blood-corpuscles a totally different signification, and found upon them most opposite results.

They have considered them as indications rather of an eliminative than of an active disintegrative metamorphosis—of a new formation, not of decay; and believe that, as they occur in the spleen, they point to the development of blood-corpuscles as one of the normal functions of that organ. Dr Gerlach, who is the principal upholder of this doctrine, asserts that the cells in question only occur in the cavities of the Malpighian sacculi. He further believes that the sacculi communicate with the lymphatics of the organ, and, consequently, that the blood-corpuscles, as soon as formed in them, are thrown into the current of the circulation.—(Gerlach, Handbuch der Geweblehre, s. 217.—Zeitsch. für Rat. Med. bd. vii. s. 75.)

The observations of Gerlach, though contradicted by other anatomists, have been confirmed by Dr Schaffner—(Schaffner, Zur Kentniss der Malp. Körperchen der Milz, und ihrer Inhalts.—Zeitschrift für Rat. Med. bd. vii. s. 345)—who draws from them the same conclusions.

The fact that, in the perfectly healthy condition of the spleen, the cells containing blood-corpuscles, or the results of their transformation into granular pigment, almost never occur, as I have myself proved by very numerous examinations, is of itself sufficent to throw something more than doubt on any theory assigning to them any constant *relation* to a *physiological* process of such great importance as that of the development of the coloured corpuscles of the blood. When, however, we find, from observations on other organs, that similar obstructions occur in them as purely pathological products, we are at once led to refer all to the same category. If, as will be shown in the following pages to be the case, we find the surrounding circumstances to be the same or similar, we are confirmed in our supposition, and are assured that in making it the object of our inquiry to ascertain in what this similarity consists, we are pursuing that path which is most likely to lead to the desired result.

(*a.*) About the second or third day of incubation of the chick, structures are occasionally observed to exist, which bear a striking analogy to the cells containing blood-corpuscles above described. These consist of agglomerations of unaltered blood-corpuscles which are surrounded by what appears to be a clear and smooth membrane. They have been described under the title of "*Blut-inseln*," and have been considered by some to be "mother blood-cells," and to be concerned in the development of the primary blood-corpuscles. That they perform no part in this process is evident from the well-ascertained facts which we possess proving it to be of a different nature. They occur in fully developed blood vessels in the *area vasculosa*, and seem to owe their existence to partial stagnations. They possess much interest, when considered in connection with what follows.

(*b.*) Professor Kölliker, in his paper "On the State of the Blood-vessels in Inflamed Parts," relates that, in wounding the hemispheres of the brain of a pigeon, and thus producing extravasation and san-

guineous infiltration in the substance of the brain, many of the so-called "inflammation-globules," which were developed in the neighbourhood of the wound, contained, besides their usual constituents, unaltered blood-globules.—(See Kölliker, Zeitschrift für rat. Med. S. bd. iv. s. 9.) With a view of verifying this interesting result, and ascertaining the further history of these "cells containing blood-corpuscles," I made the following experiments:—In a number of pigeons, at different times, I passed a common surgical suture-needle through the upper part of both hemispheres of the brain with the view of producing extravasation. Of these a considerable proportion died from the effects of the injury. In others, little or no extravasation was produced, and in these, on examining three or four days afterwards, I only found the usual compound granular cells, indicating the presence of exudation in the track of the wound. From those which lived a sufficient time, and in which other circumstances were favourable, I obtained the results detailed below:—

First, From three to four days after the infliction of the injury, it was found that the substance of the brain in the immediate neighbourhood of the track of the wound had assumed a yellowish-green colour and somewhat soft consistence, the track of the wound itself being occupied by a clot. In the softened part, on microscopic examination, the following elements presented themselves,—viz., 1st, Cell-like bodies provided with a distinct membrane, in the fluid contents of which floated fine molecules and one or two larger oleo-albuminous granules. 2d, Similar bodies completely filled with oleo-albuminous granules (so-called "compound granular corpuscles"). Both of these last elements measured from $\frac{1}{2000}$th to $\frac{1}{1300}$th of an inch in diameter. 3d, Similar bodies containing both oleo-albuminous granules, and perfect blood-corpuscles. These were very numerous, and some of them were observed as large as $\frac{1}{800}$th of an inch in diameter. The blood-corpuscles contained in them resembled entirely those which were floating free around them; and when water was added, it was observed that they were similarly acted upon, their nuclei being displayed. The drawing (Fig. 1.) was taken from the brain of the first pigeon successfully operated on. It was compared with the appearances observed in subsequent experiments, and corresponded exactly. I may add that Professor Bennett was kind enough to verify these results.

Secondly, From seven to eight days after the infliction of the same injury, the following appearances were observed:—In the case to which the description refers, no signs of a clot were to be seen, as before, in the track of the wound, which was only indicated by a yellow softening. In the softened part were observed similar bodies to those last described, but which appeared to have diminished somewhat from their original size when compared with those from the more recent lesion, their average diameter being about $\frac{1}{1500}$th of an inch. They possessed well-defined margins, and contained oleo-

albuminous granules and bright yellow *finely molecular matter*, besides occasionally altered and contracted blood-corpuscles. When submitted to the prolonged action of acetic acid they remained unaltered, and were quite unaffected by water. On adding solution of caustic potash, the colour of the granular matter was converted into a rich brown.

Thirdly. In a pigeon, the brain of which was examined nine days after the infliction of the same injury, it was observed, that while the greater part of the blood was generally diffused in the neighbourhood of the wound, as indicated by the yellow coloration, at one spot a minute but distinct coagulum, not larger than a mustard-seed, could be distinguished. On microscopical examination, the following elements presented themselves:—Generally diffused on the track of the wound, there occurred bodies altogether similar to those last described, containing oleo-albuminous granules, and a bright granular amorphous material. In some of these the one constituent predominated, and in some the other. Their diameter was about $\frac{1}{900}$th of an inch, and they almost all presented, either at their centre or circumference, a spherical clear space, indicating a vesicular nucleus. In the coagulum above referred to, similar bodies of smaller diameter, as if contracted, which contained almost nothing but the yellow amorphous matter, were observed. These differed from those above described in not presenting the appearance of a smooth transparent membrane surrounding them, so that they rather resembled masses of conglomerated amorphous matter than cells. Their colour was not so bright as that formerly mentioned, rather being of an ochre than of a gamboge yellow. (See Fig. 2.)

The results of the above observations illustrate and explain, in the first place, the mode in which the appearance of cells containing blood-corpuscles, on which so much stress has been laid by Gerlach and Schaffner, may be produced, and afford an experimental proof of the most satisfactory nature of the truth of the doctrine of Professor Kölliker, that under certain circumstances the coloured corpuscles in extravasated blood become enclosed in what appears to be membrane before undergoing further change. In taking into consideration the nature of these changes, it is necessary to remember, that besides the liquor-sanguinis of the extravasated blood, exudation is also present, and may modify the result.

Secondly. Conclusions as important may be drawn respecting the mode of origin of the so-called compound granular corpuscles, which are characteristic of the *acute* inflammatory softening of the brain, the circumstance of their containing unaltered blood-corpuscles affording the most distinct proof that the membrane is formed *subsequently* to the aggregation of the contents. To this, if it be objected that the identity of the cells containing blood-corpuscles with the ordinary compound granular corpuscles is not proved, it may be answered that the identity is evident from the following circumstances:—

(1.) They are formed simultaneously, and under precisely the same conditions in every respect.

(2.) They merge insensibly into each other in regard to all their characters,—thus some contain numerous blood-corpuscles, and but few oleo-albuminous granules, others are filled with the latter constituent, and contain only one or two blood-corpuscles, &c.

(3.) In those cases in which, owing to accidental causes, there was little or no hemorrhage, the cells containing blood-corpuscles were entirely absent, their place being taken up by the ordinary form of compound granular corpuscles.

Lastly. In the observations which were made at later periods after the infliction of the injury, we have an excellent illustration of the mode in which the blood-corpuscles, or rather their contents, undergo conversion into granular pigment, of that form which occurs by far most frequently, namely, the yellow amorphous.

(*c.*) In a limited red hemorrhagic softening, which occurred in a patient named M'Pherson, who died in the Royal Infirmary on the 2d of January 1851, affected with pulmonary apoplexy, along with obscure cerebral symptoms, and an account of whose case will be found in No. 4 of Dr Bennett's "Lectures on Clinical Medicine," the following interesting facts were observed:—The softened portion did not exceed a garden pea in size, and was situated at the centre of the *Pons Varolii*, a little to the right of the middle line. On microscopic examination it was found that, besides blood-corpuscles and the somewhat broken-down elements of the normal structure of the part, consisting of fine granular material and nerve tubes, there were to be observed large cell-like bodies of various diameters (from $\frac{1}{220}$th to $\frac{1}{100}$th of an inch). Of these some contained apparently nothing but unaltered blood-corpuscles and a transparent fluid, the proportion of the one to the other varying considerably in different instances. In others the corpuscles had become more or less completely converted into granules, and had assumed during this change an intense golden-yellow colour. They were all little affected by water. Their membrane was perfectly homogeneous, and apparently structureless. In some cases they assumed the appearance of flask-shaped dilatations of minute vessels, a circumstance which probably arose from their having been accidentally developed within them after the blood which they contained had ceased to circulate.

These remarkable structures, to which my attention was directed by Dr Bennett, and which I subsequently examined as carefully as the circumstances would permit, must be considered to be similar in nature to those described in the last paragraph, although differing so much in size, some of them being so large as to be visible to the naked eye. They cannot, however, be admitted to be identical, as, besides the difference in size, they present the peculiarity of their solid contents, consisting entirely and exclusively of blood-corpuscles, or the results of their metamorphosis. They at all events afford an

excellent illustration of the mode of origin and history of cells containing blood-corpuscles. From the total absence of the oleo-albuminous granules, which seem to be characteristic of the inflammatory process in the nervous centres, we are perhaps warranted to conclude that in this case the *softening* depended entirely on extravasation.

(*d.*) The abdominal lymphatic glands, like the spleen, are liable to congestive hemorrhages, and when so affected present the same appearances, along with softening and enlargement. On microscopic examination, we find that the blood-corpuscles undergo similar changes to those which have been observed in the last-mentioned organ, becoming aggregated into masses, and in some cases surrounded by a membrane while still remaining in their unaltered condition, and afterwards being subject to those further transformations which have been already so frequently alluded to. The appearances presented by these structures, in their earliest and most perfect condition, are shown in Fig. 4.

(*e.*) The following case from Professor Ecker is so illustrative of the subject under consideration, that I make no apology for introducing it in an abbreviated form.

A little girl, $4\frac{1}{2}$ years old, who was attacked in the course of an epidemic of hooping cough, which prevailed at Basle in the winter of 1846, after two months' duration of the disease, died with the symptoms of general tuberculosis. On making a post-mortem examination, a softened portion of the brain, about the size of a dollar, was detected, which was scattered over with numerous bloody points. The microscopic elements met with in this portion were, besides the usually observed fragments of nerve-tubes, &c., "1st, Unaltered coloured blood-corpuscles; 2d, Colourless blood-corpuscles; 3d, An extremely finely granular material, soluble in caustic potash (coagulated fibrine); 4th, Cells which contained, besides a greater or less number of dark or yellowish granules, blood-corpuscles. In these the cell-membrane was remarkably distinct, and the blood-corpuscles perfectly unaltered." The number of blood-corpuscles contained in one cell was various. A few contained only one, and had a diameter of ·010 m.m. Others, which measured from ·015 m.m. to ·025 m.m., contained from six to eight blood-corpuscles, or even more. In saline solutions the surrounding membrane, from its adhering closely to the contents, was less distinct. Water made it swell out, and it was easy to satisfy oneself by rolling the globules, that the blood-corpuscles lay within a membrane. In most of the globules, the membrane was unaltered by the prolonged action of water, while in others it at length became indistinct. 5th, Heaps of granules, similar to those occupying the cells above described, were observed, some of which were surrounded with a membrane, others not.

It is unnecessary to point out the complete analogy which exists between the facts, as above related by Professor Ecker, and those detailed above, as observed in the brains of the pigeons.

To show that the so-called cells containing blood-corpuscles are

not necessarily dependent for their origin on the presence of exudation, but may arise as well in an organising blood plasma as in an exudation plasma, I venture to quote a short notice of one more case, recorded by a German pathologist. It may also serve as an illustration of what is generally to be observed in large sanguinolent accumulations in the various mucous and serous cavities.

(*f.*) In the case of a young woman who, from congenital imperforate hymen, had never menstruated, several pounds of a brown coloured fluid like tea were evacuated by an operation. Microscopic examination of this fluid showed,—1st, a few normal coloured blood corpuscles; 2d, flattened and wrinkled blood-corpuscles, as they appear after treatment with saline solutions; 3d, all transition stages of shrinking could be demonstrated from these down to bodies of the size of a pigment grain, to which many of the corpuscles had the greatest resemblance. These displayed an active molecular motion. Concurrent with the shrinking there was an alteration in their behaviour with water and acetic acid, those least altered immediately swelling out and disappearing, others only after prolonged action, while very many did not appear at all affected; 4th, dark bodies of various sizes, much resembling the so-called inflammation-globules, occurred in quantity, which were evidently conglomerates of the bodies above described. Their external surface was sometimes uneven, sometimes smooth and rounded. Perfectly unaltered blood-corpuscles could not be detected in them.—Zeitschrift f. Rat. Med., bd. v. s. 140. (H. Müller, Ueber die Blutkörperchen in zurück gehaltenen Menstrua.)

Keeping in view the facts related in the preceding pages, let us return to the investigation with which we set out, in relation to the occurrence of cells containing blood-corpuscles in the spleen, and the explanation which they are supposed to afford of the functions of that organ.

(*g.*) Professor Kölliker admits that in man and the mammalia it is *difficult* to demonstrate cells containing *completely* unaltered blood-discs, though he states that with care it is perfectly possible to satisfy oneself of their existence, especially in the rabbit, calf, sheep, and dog. A series of experiments instituted at Zurich by Landis (Beiträge zur Lehre von der Verichtungen der Milz., Zurich, 1847) at the instance of Professor Kölliker, on no less than thirty rabbits, tend to the conclusion, that the cells containing blood-corpuscles are normally formed in animals in great numbers at a short period after feeding. In fifteen of those observed two, five, and eight hours after eating, cells with unchanged blood-globules were found in eleven, while in the other fifteen, which were examined twelve, twenty-four, and forty-eight hours after, they were in as many cases absent. How it happened that Landis arrived at such remarkable results, I cannot conjecture, as I am quite certain, that observations which I have made with the greatest care on animals similarly circumstanced as regards their periods of feeding, have failed in educing corresponding ones. I have been unable to satisfy myself of the existence of

cells containing perfectly unaltered corpuscles in the spleen of man and the mammalia in one single instance, although it seems highly probable that they do sometimes exist, not only from the frequency with which structures appearing to correspond with the later stages of their conversion into granular pigment cells, are to be observed, but from their being found in organs so analogous anatomically as the lymphatic glands. I cannot help hazarding an opinion, that those who maintain that their occurrence is so frequent as to be observed in eleven cases out of fifteen, must have committed an error, which conclusion appears more probable when it is considered that the diagnosis between cells containing numerous nuclei or secondary cells, such as are sometimes observed in the spleen, and cells containing blood-corpuscles, is very difficult, especially if the observation be made with one of the less perfect instruments which are employed on the Continent.

In reptiles, and especially in the naked amphibia, the case seems to be somewhat different. Even here the constant occurrence of cells containing blood-corpuscles cannot be admitted, as, in numerous examinations of the spleens of recently captured newts, I have either found no traces, or very few, of the existence of such bodies. That they occur in the spleens of newts and frogs in a state of partial starvation seems to me difficult to deny, as the appearances frequently presented will scarcely admit of an opposite explanation. We cannot, however, for a moment suppose, that they have any relation to the function of the organ either in the state of health or as modified by abnormal conditions.[1]

(*h.*) It is not a little remarkable that there are as distinct evidences of the existence of cells containing blood-corpuscles in the liver as in the spleen. In Fig. 7 I have represented the appearances observed in the liver of a frog taken last October about a week after capture. Cell-like structures existed in pretty considerable number, which contained in their interior other smaller bodies, presenting the form and general aspect of blood-corpuscles, in which nuclei could often be observed. With respect to the uncertainty of the real nature of these, the same remarks are applicable as to those observed in the spleen.

Having now endeavoured to exemplify the various modes in which cells containing unaltered blood-corpuscles may occur, I next proceed to point out a few more facts, which will serve to illustrate the further and ulterior changes which these bodies may undergo.

---

[1] In relation to the question, whether or not the bodies enclosed in what appears to be a cell-membrane, *as observed in the spleen*, are really unaltered blood-corpuscles, I am unwilling to express any decided opinion. I have, however, in the plate taken special pains to afford an accurate representation of the appearances as actually seen, which, I may add, has not been done by any other observer. In this way I hope that I have placed the reader almost in as favourable a position for forming a correct opinion, as if he had himself been conversant with the structures themselves. (See explanation of Figs. 5 and 6.)

(*i*) Whenever extravasated blood, or blood which is, from whatever cause, in a state of stagnation, remains for a sufficient period in contact with living tissues, the changes which take place in the coloured contents of the corpuscles are of so uniform a nature that their chief modifications may be mentioned under the following heads:—

1st, In every case of extravasation, a large proportion of the colouring matter, becoming dissolved in the fluids of surrounding tissues, is at once absorbed.

2d, In most cases the coloured contents of a small proportion of the corpuscles assume the insoluble form. This may give rise to the production either of (*a*) molecular pigment, (*b*) amorphous coloured masses of various forms and sizes, or (*c*) opaque-coloured spherical or spheroidal bodies, somewhat smaller than blood-corpuscles (altered blood-corpuscles).

3d, In some instances the colouring matter becomes united with an oleaginous material, so as to give rise to the formation of coloured oleo-albuminous granules.

4th, More rarely, a crystallisable fat being present, the colouring matter unites with it so as to form coloured crystals.

5th, Occasionally the colouring matter unites with a crystallisable albuminous compound, so as to form coloured albuminous crystals.

It will be seen from the facts to be related in the following pages, that the evidence in favour of all these propositions, except the fourth, is conclusive. This last rests at present merely on strong probability.

The changes above described may take place, either in connection with the development of cell-structures, such as those of which we have already considered so many examples, or entirely independently of them; nor is there any reason to believe that the nature of the process is at all modified by their presence. Thus, in many cases the molecular pigment is enclosed in such cells, and in as many it is perfectly free; and a similar observation might be made in reference to all the other forms.

(*k*) The conversion of the coloured contents of the blood-corpuscles into granular or molecular pigment is illustrated in several of the observations related above, especially those which have reference to cerebral extravasations. These may be advantageously contrasted with the following, in which the seat of the extravasation was an encephaloid morbid growth.

In the case of a man, Bernard Linden, who died under the care of Mr Syme, in the Royal Infirmary, on the 19th of December 1850, affected with encephaloid disease in various organs and tissues, numerous cancerous masses, of various sizes, were distributed through the substance of the lung. One of these, much larger than the rest (as large as two fists), occupied the right lateral region of

the chest, involving both the right lung and pleura. The external portion of the tumour, which appeared here and there to be limited by a distinct fibrous cyst, was of brain-like consistence, and distinctly fibrillated, the direction of the fibrillation being perpendicular to the surface. Towards the centre of the mass, which was occupied by a recent soft coagulum, the texture was more friable, and surrounding the coagulum it had assumed a dull reddish-yellow tinge, owing to the presence of diffused blood.

On microscopic examination, in the central coagulum nothing was found but unaltered blood-corpuscles. In the soft yellow encephaloid substance which surrounded it, besides the ordinary cancer-cells containing nuclei and nucleoli, many of which were loaded with oleo-albuminous granules, other cells were found which contained granular matter of another kind, namely, pigment of a bright golden-yellow colour. These were spherical or spheroidal in form, and varied in diameter from $\frac{1}{2500}$th of an inch to $\frac{1}{450}$th of an inch. They always possessed a surrounding membrane, which occasionally appeared to be *double*, a well-defined peripheral space being distinguishable between the part occupied by the coloured material and other granular contents and the external surface. They contained, besides pigmentary matter floating in a yellowish fluid, a greater or less number of oleo-albuminous granules. The pigment assumed somewhat different forms in different examples. In some it consisted of grains of various sizes, which seemed to be contracted blood-corpuscles, either isolated or in little masses, while in others it had assumed the finely granular form, and was generally diffused throughout the cell cavity. In some of the cells blood-corpuscles, in a nearly unaltered condition, were observed.

In reviewing the foregoing facts, the circumstance which appears least explicable is, that the cell-structures, which contained altered blood-corpuscles, and the results of their transformation, were identical with the larger forms of cancer cell among which they occurred, being of the same size and containing many of the same oleo-albuminous granules with which they were loaded. This seems to indicate that, in some instances at least, we may be wrong in calling these larger cancer cells "mother cells," and in supposing them to exist previously to the cells and granules which they include. Unless we make some such admission, I do not see how we can consistently explain such cases as the above.

See on this subject, Bruch, Ueber Entzündungs-kügeln, Henle's Zeitschrift, Bd. IV., s. 51.

(*l*) In a frog, which had been kept without food for several months, I found a hemorrhagic tumour, which presented the following characters :—The size of the tumour was between that of a large pin-head and of a millet-seed, and occupied the loose subserous areolar tissue of the meso-gastrium. It was of an oval form, and consisted of a fibrous cyst, enclosing semi-solid contents. The latter, when microscopically examined, presented a finely molecular

material, of a grass-green colour, among which were dispersed numerous spherical fatty granules, which possessed the same colour, and were of very various diameters. None of these appeared to be enclosed in cells. They were unaffected by acetic acid, sulphuric acid, and solution of caustic potash, though they seemed entirely decolorised by nitric acid of sufficient strength.

The external portion of the cyst consisted of well-developed white fibrous tissue; but it was lined by an inner layer, which presented the following remarkable elements, namely, cell-like bodies, each of which contained an irregularly spheroidal or mulberry-shaped central mass of pigment. This mass presented a granular appearance, and was of a dark-brown colour at the centre,—of a lighter yellowish-brown externally. It was surrounded by a distinct membraniform structure, which under the microscope displayed *faint concentric* lines, as if it were composed of several laminæ. On adding diluted acetic acid, the membrane remained unaltered, but it was dissolved by that re-agent when more concentrated. Caustic potash solution and nitric acid produced no effect on the membrane, but the latter converted the brown colour of the pigmentary matter into a yellow.

In the observation just detailed, there are several points of interest and importance in relation to our present subject. We have an exemplification of the mode in which a blood coagulum becomes converted into an apoplectic cyst, by a process of fatty degeneration and consequeut softening taking place towards its centre, while externally fibrous tissue is developed so as to form a cyst, which cyst is lined by a layer of cell-like structures, including more or less altered blood-pigment.

The green coloration of the granules also illustrates the mode in which the colouring matter of the blood may be dissolved in an oleaginous material, and by thus combining with it give rise to one form of pigment. The cell-like bodies present several peculiarities of interest, and none more remarkable than the apparent structure of their surrounding membrane. From the concentric lines which could be easily observed surrounding at various distances the central pigmentary mass, no doubt could be entertained by any observer that the membrane was not a simple one, but consisted of a succession of concentric laminæ—the most internal probably immediately investing the contents. Admitting this, it must follow that we should be more correct in considering the whole as a solid body, than as a cell or vesicle. May it not be, that a similar conclusion is applicable to other bodies of the same class at some period of their existence, and especially to those which contain unaltered blood-corpuscles? It appears to me the more probable, the more that I weigh the evidence in its favour; for, in the first place, in almost every instance in which these forms of organization are being developed in number, even while the greater number of the spherical bodies present the appearance of a surrounding envelope distinguishable from

the contents, yet many can be observed in which this is not the case, but in which the contents appear to be held together by a mere transparent intermediary substance. This is more particularly observable in extravasations and acute exudations in the brain.

In other cases, a number of conglomerates, including blood-corpuscles or pigment, &c., are observed, not one of which presents the appearance of a surrounding membrane, and must, therefore, be held together in the same manner, as is frequently the case in the spleen.

Such a doctrine would explain much more satisfactorily than any other the appearances described above in the case of Bernard Linden.

Many of the "cells containing blood-corpuscles" are in reality, however, vesicular, as may be proved by rolling them in water between two plates of glass under the microscope. In making this observation, it may often be observed that a distinct external membrane exists, to which the contents are altogether unattached.

The most safe conclusion with respect to the subject probably is, that a certain form of organization exists, which, in its primary condition, is a solid spherical conglomerate, its constituent elements being held together by a transparent and structureless intermediary substance—that it increases by further additions of a similar substance *to its surface*, giving rise to a distinctly visible transparent membrane, capable of being swollen out by the action of water, but distinguished from a true cell-wall by its not being further affected by acetic acid. In some cases, probably, this margin may assume the character of a distinct membrane, from the internal surface of which the contents become disengaged and free. This must be owing to a softening of the central or first-formed intermediary substance, of the same nature as that by which the whole structure eventually breaks down and disappears.

EXPLANATION OF THE PLATE.

Fig. I.—Cell-like bodies, including blood-corpuscles, from the brain of a pigeon.

Fig. II.—Cell-like bodies, containing yellow granular pigment, also from the brain of a pigeon.

Fig. III.—Cell-like bodies, containing unaltered blood-corpuscles from an apoplexy of the medulla oblongata. The magnifying power according to which these are drawn is only *half as high* as that employed for the rest.

Fig. IV. — Cell-like bodies, including blood-corpuscles, from a lymphatic gland.

Fig. V.—Cell-like bodies, from the spleen of the triton, containing the bodies supposed to be unaltered blood-corpuscles by Kölliker and others.

Fig. VI.—Similar bodies, from the spleen of the newt.

Fig. VII.—Cell-like bodies, from the liver of the Frog, which include what appear to be blood-corpuscles, but respecting which there is little certainty.

Fig. VIII.—Cell-like bodies, containing yellow molecular pigment, from an encephaloid tumour of the lung. From the appearance of *concentric* laminæ, which many of the envelopes presented, they appear to be *solid*, and not in reality vesicular.

METAMORPHOSIS OF THE BLOOD CORPUSCLES. Plate I

Fig. 1.

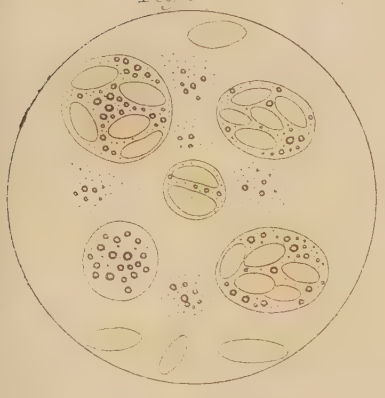


Fig. 2.

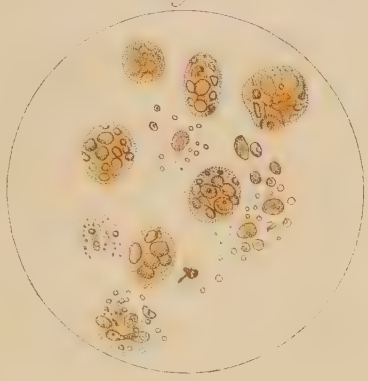


Fig. 4.

Fig. 3.

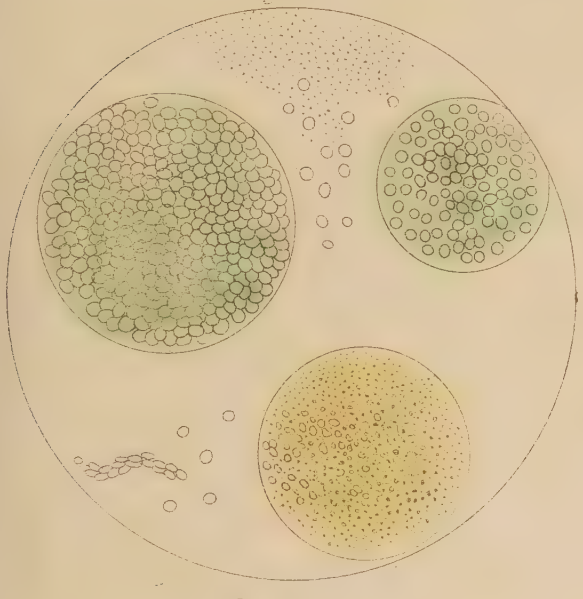


Fig. 5.

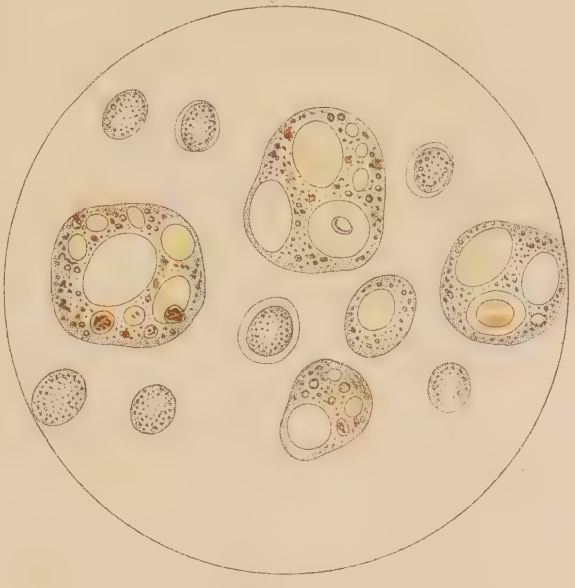


Fig. 6.

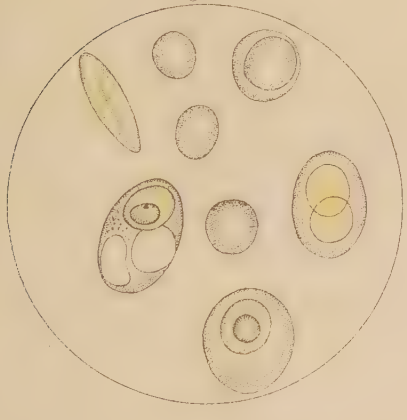


Fig. 7.

Fig. 8.

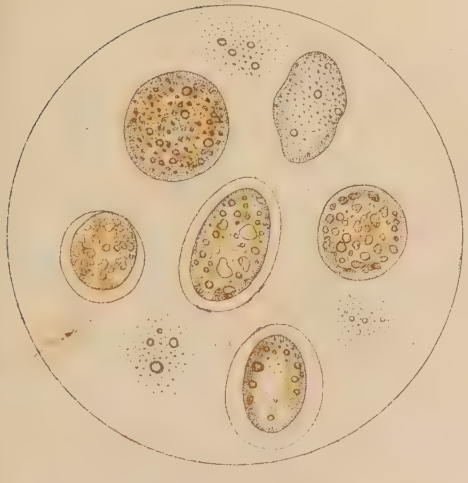


Fig. 9.

Sanderson, Del. *Monthly Medical Journal.* September 1851. J. Cellaly, Lithog

Fig. IX.—Cell-like bodies, obtained from an apoplectic cyst from the mesentery of a frog, displaying the concentric arrangement of the layers of the envelope, after the addition of nitric acid.

(*To be continued.*)

ARTICLE III.—*Observations on the Use of Capillary Tubes in Vaccination.* By WILLIAM HUSBAND, M.D., & M.A., Edinburgh.

(*Abridged from Communication read before Medico-Chirurgical Society, on Wednesday, 9th July* 1851.)

ABOUT four years ago, I happened to see, at the Edinburgh New Town Dispensary, some of the glass tubes with bulbs, which were occasionally used at that institution and elsewhere for preserving vaccine matter, and transmitting it to distant parts of the world. The great advantage of these tubes I understood to be, that fresh lymph introduced into them, and hermetically sealed up, retains its virtue absolutely unimpaired for any length of time, and it occurred to me that, instead of being only occasionally and rarely used for the purpose of sending matter abroad or to a distance, they might be serviceable in ordinary practice as a substitute for the glass plates, which are at present in all but universal use. A few experiments with them satisfied me that I was mistaken; but, before laying them quite aside, I broke the bulbs off a few of them, and found that, when divested of these appendages, they answered my purpose somewhat better. Still, however, they were much too large, and consequently very unmanageable. I saw that it was necessary to have them made on quite a different scale; and at length, after repeated trials, I succeeded in constructing them of such dimensions, that they perfectly fulfilled the expectations which I had formed of them. Nor has four years' subsequent experience in the use of them tended to alter my opinion. On the contrary, the longer I employ them the more evident do their advantages appear to me; and I feel the more convinced that whoever gives them a fair trial will come to the conclusion, that they are calculated to effect a great saving of time and trouble to the practitioner, and to render the practice of vaccination much more satisfactory and efficient than it has hitherto been.

I am aware that I must appear singular in holding such an opinion as this. For since the employment of capillary tubes in vaccination is not of recent origin, but was first suggested as far back as the beginning of the present century, and has been repeatedly noticed or described by different authors since that period, it may be said, and not without apparent reason, that the mere fact of their having fallen into neglect and oblivion is a sufficient proof that they were tried long ago and found wanting. In reply

to this objection, it might be shown, by a reference to the various notices which have been published at different times respecting them, that they have *not* been fairly tried, and that therefore their having been so generally forgotten and thrown aside is no presumption at all against their utility. But, instead of pursuing this argument, I proceed at once to describe the tubes which I have been in the habit of using, and to which alone the remarks which I am to make are to be understood as applicable, and then to give a short exposition of the method of using them, and of what appear to me to be their peculiar advantages.

In describing them, it is important to state at the outset, that there are certain conditions which must be attended to in constructing them, and upon which their utility, as a means of preserving vaccine lymph, essentially depends.

I. In the first place, it is indispensable, that the tubes be so small that they can be sealed instantaneously at the flame of a candle.

II. Secondly, that they be large enough to contain each of them one sufficient charge of matter.

III. Thirdly, that each be long enough to admit of its extremities being hermetically sealed, without subjecting the contained charge to the heat of the flame.

IV. And fourthly, that they be not too easily broken.

These conditions can only be fulfilled by taking care to have the tubes made according to a certain standard, and since this standard, from the nature of the case, is evidently not an arbitrary one, which may be altered to suit the notions of different individuals, I shall endeavour to lay it down, by giving, as accurately as I can, and after careful investigation, the dimensions of those tubes which appear to fulfil most perfectly the essential conditions just now stated.

Their average length is $2\frac{1}{2}$ inches. The thickness of each is about $\frac{1}{24}$th, and that of its wall $\frac{1}{300}$th of an inch.

They are straight, and open at both ends, and are made from larger tubes, which, of course, may be of various sizes, provided their diameter and thickness of wall are nearly in the ratio of 300 to 24.

A piece of conducting tube, about $\frac{1}{4}$ of an inch in diameter, and $\frac{1}{50}$th of an inch in thickness of wall, has the requisite proportions, and will be found of a convenient size for working with. It is melted with perfect facility at a common blowpipe flame, and is then drawn out, and broken into lengths by the help of a small file.

I have been thus minute in describing them,—First, because I believe a proper description of them has been hitherto a desideratum. Secondly, because almost everything which I have read upon this subject, as well as every day's additional experience regarding it, has tended to confirm me in the opinion, that all along, from the first partial introduction of the tubes into notice nearly fifty years ago, the importance, or rather the absolute necessity, of

their being made according to a certain fixed standard, has not been insisted on nor even understood; and that this is the principal, if not the only, reason why they have not long ago come into universal use. And I may add, thirdly, because, since I began to use them, there have been and are now circulating through Edinburgh and elsewhere tubes, which so far deviate from the proper standard as to be nearly if not quite useless, in consequence of their not sufficiently fulfilling the conditions which, as I have said, are necessary to success in the use of them, and especially that most important one in virtue of which they can be sealed instantaneously, or nearly so, at the flame of a candle.

I do not mean that it is necessary to resort to minute measurements of 100ths of an inch as often as one requires a new supply of tubes. It is not at all necessary to do so, any more than it is for the surgeon to test the sharpness of his lancet by measuring microscopically the breadth of its edge. I have never found it difficult to judge of them almost at a single glance. But that is quite consistent with the necessity of having a standard laid down, and kept in view at least by the workman who makes the tubes,—to which I do not say they must strictly and rigidly conform, but from which they must not vary, except within certain limits. For, of course, there are limits within which they may vary very considerably, and still possess in a sufficient degree all the valuable properties which I assign to them.

The method of using them is shortly as follows :—

The vaccine vesicle having been opened in the usual way, nothing more is required than to dip one end of the tube which is to be charged into the exuded lymph, and to allow a sufficient quantity to enter by the force of capillary attraction. This takes place instantly, provided enough of lymph has exuded. If the vesicle do not discharge much, and it is well known that the quantity varies greatly in different cases, the end of the tube may require to be drawn several times more or less obliquely across the surface, until a sufficient charge has entered. It is then to be held in a vertical position for a single second, in order that the contained matter may gravitate towards the middle of it. Sometimes a few slight succussions are required to effect this, when the matter is more than usually viscid, or the tube rather small. In this way, as many charges may be taken as is thought desirable, without the least delay, except what is occasioned sometimes by the vesicle containing but a scanty supply of matter, or not yielding it quickly. Each tube is then to be sealed by applying first one end, and then the other, to the flame of a candle, or to a common gas flame. That of a small wax taper, it may be observed, is quite strong enough to seal them instantaneously, or nearly so, if they be of the proper size. After being filled and sealed, they may be kept in any convenient receptacle for future use: in a glass tube, for instance, of which it is expedient to have two, one for empty tubes, and another for full ones. For several reasons,

however, to which I shall not advert at present, I prefer carrying them in a small box.

In vaccinating, all that is necessary is, to break off the two sealed ends of a tube, and to blow out its contents gently upon the point of the lancet or vaccinator.

There are only two methods which I think it necessary at present to compare with that which I have been describing, viz., the method of depositing the matter, and allowing it to dry between two square pieces of glass, and the other in which it is preserved in a fluid state in a stoppered phial.

When contrasted with the former, some of the advantages of the tube method are very obvious.

Thus the tubes are charged and put aside for future use, much more easily and quickly than the squares of glass are.

In vaccinating with them, much less time is occupied in breaking off the sealed ends and blowing out the lymph than in restoring to its originally fluid state matter which has been allowed to dry on the glass plates; besides that this last process is sometimes through haste imperfectly performed, and the operation fails in consequence.

As to the stoppered phial, it is not difficult to see, that whatever advantage it has or is supposed to have over the glass plates, the tubes possess in a far superior degree. The phial is designed, by exclusion of the external air, by means of a well ground stopper,—1st, to preserve the lymph fluid for a time; and, 2d, to preserve it fresh. But it does not exclude the air perfectly. For the most accurately fitting stopper cannot be implicitly trusted to, and, besides, the phial being constructed to contain several charges, the air is necessarily re-admitted as often as the stopper is removed for the purpose of vaccinating; whereas, it is evident that the tubes, being hermetically sealed, provide for the complete accomplishment of these two ends, so far as exclusion of the external air is capable of accomplishing them, and each containing only as much lymph as is to be used at any one time, its contents are never exposed to the air till they are required for immediate use.

But there is one great advantage connected with the tubes to which every other superiority they may possess is subordinate and subsidiary, viz., that they afford a ready means of accumulating a quantity of vaccine matter, in probably the best circumstances for retaining its virtue unimpaired, and which may serve as a kind of capital stock, to be had recourse to as occasion or necessity shall require.

The phial obviously does not furnish us in any degree whatever with the means of doing this; and although the square pieces of glass charged in the usual way may be accumulated together, yet, in point of fact, they never are, partly on account of the trouble and loss of time connected with the process of charging them, but chiefly because it is well understood that vaccine matter so preserved becomes very soon inert, and good for nothing, and, therefore, that the

labour of collecting it would be in a great measure thrown away. Hence it is that medical practitioners in town, as well as in country, are continually running short, and find it necessary to obtain a new supply from time to time by borrowing from their neighbours, or from one of the public institutions where vaccination is performed on a large scale. It is everywhere reckoned a good excuse for delay in vaccinating, that the medical attendant has no fresh matter, but expects to have some in a few days, or it may be weeks. The public is quite familiar with this excuse, and accepts it as a reasonable and valid one. We often hear such observations as these made both among medical men and by our patients,—that vaccine matter is at present very difficult to be had, that there is a great scarcity of it, that the doctor has been often promising to come during the last few weeks to vaccinate, but he has got no matter. And it is evident that we have become reconciled to this state of things as a necessary evil, but not the less certainly an evil for which it were well that we had some remedy. It will hardly be said that the amount of inconvenience and loss of time which falls to the share of any one individual is inconsiderable; but at all events, the aggregate amount is very great. In country districts especially the evil often reaches a great height, as is proved by the frequent and often urgent applications for matter which are continually coming from all quarters into Edinburgh and other large towns, and which indicate much more than that medical men are suffering inconvenience and loss of time. They signify, that small-pox is epidemic in various quarters from time to time, kept in check indeed by vaccination, but still committing ravages, and cutting off valuable lives because the antidote is not to be had. I believe ample evidence could be brought forward to establish the proposition, that a certain proportion, and not a very small one, of the deaths from small-pox, both among children and adults, is traceable not to the neglect of vaccination on the part of parents, or to any other cause than to the frequent scarcity of vaccine matter, arising from the inefficiency or inadequacy of the means employed to preserve it. These means are not sufficient, in short, to enable any one, unless in unusually favourable circumstances, to keep up a constant supply of vaccine matter in his own hands, for his own use, without having often to borrow from others. But the tubes are sufficient for that purpose. No one who uses them need ever run short of matter. For as they may be filled and sealed with a facility not easily conceived without trial, and as the opportunities for renewing the supply of lymph far more than compensate for the single charge expended in each vaccination, the practitioner need have no difficulty in keeping up his stock. And it is well worth while to do so, because lymph, hermetically sealed up, has what the same substance preserved between squares of glass has not, viz., *a permanent value;* by which I mean, that of a given number of charges preserved in this manner, for long periods, the

vast majority retain their virtue unimpaired. I do not, however, intend now to enter at length upon the proof of this assertion. For the present it may suffice to say, that I am not unprepared with evidence to prove it. For having, during the last four years, carefully registered the results of about 500 vaccinations from tubes, I find that in vaccinating with matter which has been kept hermetically sealed for periods varying from six months to two years, I have perfectly succeeded at least seven times for once that I have failed; and while I willingly state this as so much evidence tending to lead to a decision respecting the merits of the tube method, I must add, at the same time, that this proportion of seven to one does not, for several reasons, give an adequate idea of its efficiency.

ARTICLE IV.—*Case of Poisoning with Strychnia.* By ALEXANDER SMITH, M.D., Edin., Forfar.

T—— H——, a gamekeeper in the neighbourhood, who had been missing from the 7th of May last, was found dead in a plantation on the 11th. A phial, labelled "Strychnia, poison," and containing a small quantity of a whitish powder, was found lying by his side; which I took possession of. On my arrival at the spot, the body, which had not been moved, was lying on the face, with the hands firmly clenched, the feet very much extended, and the head considerably bent backwards. Every joint of the body was excessively rigid, and some of the muscles exhibited the same appearance as when in powerful action during life. There were no marks of violence, except a few superficial scratches on the eye-brows and tip of the nose, probably produced by the stems of the rough strong heather, among which the face rested. The countenance was very livid, and somewhat swollen. Except some shrinking of the eyeballs, and the usual opaque appearance of the corneæ, observable soon after death, there was little indication of putrefaction.

The body having been removed to a convenient place for further examination, the head was opened, and the blood-vessels on the surface of the brain found very highly congested. On cutting into the brain, the texture of which was healthy, numerous bloody points presented themselves. The choroid plexus was very turgid. On cutting the scalp, dark-coloured fluid blood flowed freely from it. The blood manifested the same appearance throughout the body. The lungs were healthy in structure, but much gorged with blood, and the heart, which was also natural, had both ventricles very much distended. The stomach, which externally exhibited no trace of disease, and a portion of the duodenum, with their contents, were removed for more minute examination. All the other viscera of the abdomen were in a healthy condition. The spine was not examined.

On opening the stomach a patch of the villous coat, near the cardiac extremity, presented considerable congestion, and a small quantity of a whitish powder adhered to the membrane at this part, and also at some others where no congestion was manifest. It contained no food, and only a few ounces of fluid, which exhaled a strong spirituous odour. A minute quantity of the contents was then mixed with very dilute sulphuric acid, and boiled in a glass vessel for about half an hour. A portion of the supernatant liquor, when cold, being treated with nitric acid, was changed to a light orange colour, and another portion, upon adding solution of tannin, deposited a pretty copious grayish-white sediment. A small quantity of the powder found in the phial was treated in the same manner, and similar results produced.

The detection of strychnia, so far as my information goes, is at present surrounded with great difficulty and uncertainty. On this account, and also from the fact of very few cases being on record where a thorough and scientific examination has been made by men of authority on the subject, I transmitted the stomach and remainder of its contents to Professor Christison, who kindly undertook to inspect them, and has since furnished the following account of his examination :—

"The contents, and washings obtained with water acidulated with sulphuric acid, were filtered, treated with milk of lime, concentrated to a thin syrup, and filtered again. The insoluble lime-salts and excess of lime would contain strychnia, if there was any. The mixture, dried at 212°, was treated with boiling rectified spirit, and the solution was evaporated to dryness. The extract had the intense persistent bitter taste of strychnia, and was turned bright yellow by nitric acid. Another test mentioned in my Dispensatory did not act characteristically, the colours being altered by organic matter. There is, in fact, a want of positive tests for strychnia when mixed with organic matters, unless the proportion were considerably greater than in this case, so as to admit of some complexity of chemical handling.

"I think there is satisfactory evidence for a case of suicide or accident. It may be doubted whether this chemical evidence, however, would be enough in a charge of murder,—*in which there was no proof of the nature of the symptoms.*"

ARTICLE V.—*On the Remedy of Stricture by External Incision.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

J. H., aged forty-three, late sergeant in the —— Dragoon Guards, was admitted into the Royal Infirmary on the 19th of June, on account of stricture. He stated that the disease had originated from gonorrhœa, and troubled him for eighteen years,—in the earlier part

of this period being relieved from time to time by the introduction of instruments, but latterly resisting all attempts to accomplish this, so as at length to require his discharge from the army, which had taken place seven months before. During the last eight or nine years, he said, that although under the care of very many practitioners, civil as well as military, some of whom had tried daily for months to introduce instruments, *he never had had one passed through the stricture.* About six years ago an attempt had been made to force a way through the contracted part, which was at the bulb, by means of a lancet-catheter, but without success. Notwithstanding some relief which the patient had derived from passing a flexible bougie down to the obstruction, which he did daily, all the symptoms of stricture existed in a very severe form, the urine frequently escaping only by drops, and not being always under the power of retention.

After repeated ineffectual attempts, but without ever drawing a drop of blood, I succeeded on the 6th of July in passing a No. 1 bougie fairly into the bladder. I then endeavoured to carry on the dilatation, but without success; and in order to obtain room for the introduction of a grooved director, on the 11th tied in a catheter of the smallest size. On the 13th, as the patient began to complain of this instrument, I removed it, and found that the grooved director could be passed with ease. Circumstances induced me to delay the operation until next day, when I found that the canal was again so tightly contracted as to admit only the small catheter, which was therefore retained for other two days. On the 16th I performed the operation as usual,—the patient declining chloroform, and treating the affair as unworthy of complaint. The catheter (No. 7) was removed on the 18th, and the urine then issued in a full stream entirely through the urethra. A few days afterwards, the patient, having got up and walked about, found that a part of the water escaped by the wound, but soon observed the amount of what passed in this way diminish to a few drops, and on the 6th of August reported himself quite well. He was dismissed cured on the 12th.

With regard to this case, it may be remarked, in the first place, that the disease which was the subject of treatment had deprived the army of the finest looking soldier I have ever happened to see. Secondly, it affords an instance of a stricture for nine years reputed impermeable by metropolitan as well as provincial, public as well as private surgeons, but which, nevertheless, proved permeable to the simple bougie, when employed under the impression that there is no true impermeability. Thirdly, the case presents another instance of the ease, safety, and efficiency with which the most obstinate stricture may be remedied by incision upon a grooved instrument passed through the contracted part of the canal.

Mr Acton, in his lately published work on "Diseases of the Urinary and Generative Organs," says (p. 160), with reference to the opera-

tion I have recommended, "I have witnessed this perineal section performed several times in the most able manner, but the patients have all died." A correspondent of the "London Medical Gazette," writing from Cheshire, directed attention to this statement, and expressed his opinion, that Mr Acton should state the particulars of "such appalling mortality;" and in reply to this call, Mr Acton has referred to the cases which were published by Mr H. Smith in the "Medical Times" of May 1850. I have looked into these cases, and find that they are fifteen in number, of which the following are the prominent features:—

1. Treated by Mr Fergusson,—"No instrument whatever could be passed through the stricture." The *old* operation was performed, and the patient *recovered.*

2. Treated by Mr Fergusson,—"The urethra was impervious to instruments." The *old* operation was performed, and the patient *recovered.*

3. Treated by Mr Fergusson,—"No instrument could be got into the bladder." The *old* operation was performed, and the patient *recovered.*

4. Treated by Mr Fergusson,—"A terrible stricture." "No instrument could be got into the bladder." The *old* operation was performed, and the patient *recovered.*

5. Treated by Mr Fergusson,—"No instrument could be passed." The *old* operation was performed, and the patient *recovered.*

6. Treated by Mr Fergusson,—"No instrument whatever could be passed into the bladder." The *old* operation was performed, and the patient *recovered.*

7. Treated by Mr Bowman, who "endeavoured for about three months to introduce a catheter, but vainly." The *old* operation was performed, and the patient *recovered.*

8. Treated by Mr Fergusson,—The *old* operation was performed, and the patient *died.*

9. Treated by Mr Partridge,—"No instrument could be passed into the bladder." The *old* operation was performed, and the patient *died.*

10. Treated by Mr Fergusson,—"An impervious stricture; a fine healthy man about thirty-five years of age." The *old* operation was performed, and the patient *died.*

11. Treated by Mr Nunn,—"No instrument could be passed into his urethra." The *old* operation was performed, and the patient *died.*

12. Treated by Mr Fergusson,—The operation recommended by me was performed, and the patient *recovered.*

13. Treated by a surgeon, whose name is not mentioned. The operation recommended by me was performed, and the patient *died.*

14. Treated by Mr Fergusson,—The operation recommended by me was performed, and the patient *recovered.*

15. Treated by Mr Fergusson,—The operation recommended by me was performed, and the patient *recovered.*

It thus appears that of eleven patients upon whom the *old* operation was performed, seven recovered and four died; and that of four patients upon whom the operation which I have advised was performed, three recovered and one died. It also appears that the astounding statement of Mr Acton has no other foundation than the bad result of *one* case, treated by a nameless operator; and, so far as can be learnt from the history, improperly treated, since it is said that during the night after the operation there was a "smartish bleeding." How this insulated and questionable fact can be reconciled with the statement, that he had "witnessed this perineal section performed several times in the most able manner, but the patients have all died," I leave Mr Acton to explain. For my own part, I have now operated, without the loss of a single life, on fifty-one cases of stricture deemed irremediable under ordinary treatment; and I therefore venture to hope that the persuasion which I originally expressed as to the operation, when properly performed, being absolutely and entirely free from danger, will not appear unduly sanguine.

ARTICLE VI.—*On the Pathological States of the Lung connected with Bronchitis and Bronchial Obstruction.* By W. T. GAIRDNER, M.D., Pathologist to the Royal Infirmary of Edinburgh.—(*Continued from p.* 19.)

PART II.—PERMANENT LESIONS OF THE AIR-VESICLES AND BRONCHI DEPENDING ON BRONCHITIS.

Permanent Atrophy of the Lung in relation to Bronchitis.— In the preceding investigation of the causes and mechanism of pulmonary emphysema, the connection of that lesion with all causes leading to partially diminished bulk of the lung has been fully displayed. Among these causes of emphysema, bronchitic collapse of the lung and its sequelæ unquestionably hold the first place in importance and frequency; about 67.5 per cent. of the cases of emphysema in the table, having been manifestly connected with these affections. The forms of condensation here referred to may be divided into the more recent and the more chronic; the former being pure instances of bronchitic collapse, usually capable of easy removal by artificial insufflation of the dead lung, and therefore probably remediable in their character; the latter, on the other hand, having acquired more or less of a permanent type. The latter kind of condensation I distinguish by the name of *pulmonary atrophy*, a condition of the lung, the various forms of which are very imperfectly described by systematic writers, and by no one, so far as I am aware, except by Dr Stokes, ascribed to bronchitis.

The connection of atrophy with bronchitic collapse of the lung, can

require but little explanation to the reader of this memoir. That the lung affected with collapse should after a time become altered in its structural relations, and become the subject of a permanent contraction or even obliteration of the air-vesicles, is no more than might have been apprehended from the knowledge of what takes place in the pulmonary tissue when subjected to long-continued pressure from pleuritic effusion. In such cases, it becomes, after a time, more or less impermeable to air, and incapable of its former expansion, even when the fluid has disappeared from the pleura, and all the mechanical conditions are favourable for its return to the normal state. Observation and experiment on the dead body also shew, that in those cases the proper tissue of the lung has in part disappeared, and that the air-vesicles which remain are incapable of assuming their original volume by any amount of expanding force. In some instances, indeed, where compression has not been too long continued, we have good evidence that time will do much towards restoring the lung to its former condition; but in the majority of cases of chronic pleurisy, a permanently contracted side, with some degree of hypertrophy in the opposite lung, form the nearest approach to a perfect cure.

A precisely similar series of changes may be observed in the collapsed bronchitic lung, and has, indeed, been already alluded to in the first part of this memoir (Cases III. and IV., Monthly Journal, Vol. XI., pp. 133-4). In the dead body, the following gradations may be traced—1. In the quite recent forms of the affection, the collapsed lobules yield before a force somewhat greater than that which inflates the sound portions of the lung; when inflated to the utmost, they are pale, emphysematous in appearance, and of volume equal to the surrounding parts; and when allowed again to subside to the ordinary condition of the dead lung, they are undistinguishable from the originally sound portions. This is the condition of simple collapse without atrophy. 2. The collapsed lobules cannot be inflated without the application of considerably greater force than in the former case; they then yield, however, and though perhaps not gaining altogether the full volume of sound lobules, are, on subsidence, not very easily distinguishable from them. 3. On insufflation of the lung, the collapsed lobules yield after very considerable resistance, but evidently not to the full extent; on allowing the lung to subside, they return more or less completely to the collapsed and non-crepitant condition. These phenomena are of course best observed in the well-defined lobular collapse which affects the anterior and lower edges of the lungs, as in Case IV., already alluded to. 4. The collapsed lobules cannot be inflated with air, except perhaps by a force sufficient to rupture the tissue; and then the air passes more readily into the interlobular spaces than into the obliterated air-vesicles. This is the condition of complete simple atrophy.

In *simple atrophy of the lung*, the result of uncomplicated bronchitic collapse, the affected parts usually present somewhat different

characters from other forms of pulmonary atrophy. They are, in fact, reduced to a lax fibrous or areolar texture, inclosing the remains of bronchi and vessels; perfectly flaccid, free from all induration or abnormal exudation, and very frequently, in the purest form of the lesion, free even from that excessive deposit of carbonaceous pigment, which is so apt to accompany all chronic affections of the lung. Such atrophied lobules will almost invariably be found, on examining the free anterior or lower margins of old emphysematous lungs; and, in more recent specimens of emphysema, the anatomist will generally be able to trace several of the stages which I have indicated above, as intervening between collapse and atrophy. The atrophied lobules at the edge of the lung, correspond to the indentations and grooves between the emphysematous parts. On examining them closely, there will often be found a thin lamina, spread out between two emphysematous prominences, like the web of a frog's foot, and composed of the two pleural layers, enclosing the attenuated remains of the pulmonary tissue. Such portions are generally clearly and definitely marked off by the interlobular septa from the emphysematous lobules in their neighbourhood. In other instances, scarcely even this amount of tissue can be traced, and the two pleural layers may appear to be almost in contact over a small space, with little or no intervening substance. To any one who attentively studies a variety of such specimens, it will be apparent that simple atrophy of the lung, in its most complete form, is a lesion only to be distinguished by negative characters. The proper and special elements of the pulmonary tissue have disappeared; but they are not replaced (as in atrophy from other causes) by any adventitious structure, or even by the thickening or induration of the fibrous basis. For this reason, simple atrophy is sure to be overlooked, unless its traces be sought for in the manner I have described. In the centre of a lung, very many lobules may be entirely atrophied, and leave no visible or tangible evidence of their previous existence.

Simple atrophy, like the lesion which gives rise to it, occurs in the lobular and the diffused form. The latter is chiefly found in the posterior portions of the lungs, near their root, among the great bronchi and the bronchial glands, which are often, in these cases, dark coloured from infiltrated carbon, even when the lungs are by no means remarkably so. In diffused simple atrophy, the lung is rarely entirely condensed, generally retaining a certain degree of crepitation, but being dense, tough, and fibrous; sometimes dark slate-coloured, at other times not so; and in the most marked and exaggerated examples, crossed in every direction by fibrous processes, or septa of considerable thickness and density, corresponding to numerous depressions and irregularities on the surface of the lung, which is usually in these cases very emphysematous in front, and over the surface generally. Such lungs will always be found, when a fresh section is inspected with or without a lens, to present the most remarkable varieties in the size of the air-vesicles; some of

which are entirely obliterated, or very small, and others greatly expanded beyond the normal volume; the latter condition prevailing, of course, towards the anterior margins in the most emphysematous parts.

The changes impressed upon the form and movements of the chest, by the chronic sequelæ of bronchitis, form so marked an illustration of the doctrines above recorded concerning the supervention of atrophy on bronchitic collapse, that some reference to them here is quite necessary to the complete treatment of this subject. I have already alluded in the first part of this memoir to the modifications of respiratory movement which take place in acute bronchitis in children, while the bones and cartilages are, as yet, inadequate to the task imposed on them of expanding the chest under conditions of increased resistance. Under such circumstances, it is not very unusual to find the movements of the lateral regions of the chest actually reversed, the parietes being, as it were, sucked inwards at each descent of the diaphragm, owing to the external atmospheric pressure overcoming their power of expansion. This yielding of the ribs I have indicated as probably one of the causes of the extremely frequent occurrence and great extension of bronchitic collapse in very young subjects. In rickety individuals, it is not only more marked, but apt to become permanent, especially when such subjects are affected with any considerable or persistent bronchitic affection. In such cases, the reversed movement of the ribs is stereotyped, as it were, in the form of chest called *pigeon-breast*, in which the sternum is protruded, particularly below, and the whole lateral region, including also the lower costal cartilages in front, flattened, or even at some points rendered irregularly concave.

Many slighter and more partial permanent irregularities in the form of the chest are no doubt owing to infantile bronchitic attacks, either modifying the original expansion of the lung, or producing subsequent partial collapse of its tissue. The immense frequency of such diseases in childhood, and the unquestionable tendency which they are now shown to have towards structural changes, will probably go far to account for many of those disorganizations in the lung, revealed by morbid anatomy in a large proportion of cases, and which often seem to have no connection with any thing in the history of the individual. To a similar source may, in all probability, be justly traced most of the so-called "physiological heteromorphisms" of the chest, described and investigated with such elaborate minuteness by M. Woillez.[1] According to this writer, these slight and trivial deviations of the apparently healthy chest, occupy, for the most part, the same situations as those which are known as the results of disease; and, indeed, it would appear that the "physiological" and "patho-

[1] Recherches Pratiques sur l'Inspection et la Mensuration de la Poitrine. Paris, 1838.

logical" irregularities are by no means separated by a very distinct line of demarcation. It is quite true that, in many of the individuals presenting these changes, no history of chest-disease can be procured; but every one accustomed to the task knows that the elimination of information in regard to diseases of early life, is, in the cases of most hospital patients, nearly impossible, even where the disease has been of considerable importance. In so far, therefore, as these "physiological" irregularities are worthy of consideration at all,[1] I cannot but think that infantile bronchitis may probably have a large share in their production.

In adults, the motions of the chest are altered to a considerable extent in bronchitic affections, though not nearly to the same degree as in children. The greater solidity and firmness of the bones and cartilages opposes an effective resistance to that abrupt and well-marked retraction of portions of the thoracic wall which has been noticed as occurring in infantile bronchitis; the chest expands more uniformly and forcibly over its whole surface, and the phenomenon which gives rise to the pigeon-breast, is not observed when the bones are healthy. That the respiratory motion may be seriously limited, and sometimes even reversed, at certain points in the adult chest during bronchitis, is demonstrated by the observations of Dr Sibson, made with the aid of his ingenious and useful instrument, the chest-measurer.[2] These observations I regret that I have not yet been able to repeat, but of their general accuracy I can entertain no doubt. It is evident, indeed, to the eye (which, when employed with the requisite care, is in this case a far less deceptive, and more instructive instrument, than the ordinary measuring tape) that, while even in the severer forms of bronchitis, the chest on the whole expands both in its upper and lower zones, the movement of the latter is much more restricted than that of the former; and that while the *lateral* expansion of the thorax is circumscribed, the *anterior* movement of projection of the sternum and costal cartilages is usually even exaggerated. The result of this curious modification of respiration is, that in cases of long-continued chronic bronchitis, even during the

---

[1] This qualification is not unimportant, seeing that M. Woillez, by dint of rather ponderous statistical machinery, has arrived at the singular conclusion, that only 1 in 3 of the *healthy* chests, and about 1 in 5 of *all* the chests examined by him, present a strictly *regular* conformation. It is obvious that, with a few more refinements such as those to which this observer has devoted so much labour, the ideal of regularity would require to be sought altogether beyond the pale of humanity. It appears very doubtful whether even the Apollo or the Antinous could withstand the search for "physiological heteromorphisms" by M. Woillez. At all events, artists and anatomists are well aware that, among the poor sons of Adam, strict symmetry and regularity in every point of form, is an occurrence of almost fabulous rarity. The very general lateral curvature of the dorsal spine, and the all but invariable lateral deviation of the nose, are glaring instances known to every one. How often do the phrenologists find a *regular* head?—or would any two of them agree upon the subject?

[2] Medico-Chirurgical Transactions, Vol. xxxi.

intermissions of accumulation in the air-tubes, an altered habit of breathing is acquired and permanently retained; and the stethoscope, as well as the inspection of the chest, can often determine in such cases that respiration is effected chiefly by the upper and anterior portions of the lung, and by the movements of elevation and projection of the sternum; while the parts of the lung corresponding to the lateral and posterior regions of the chest, remain comparatively little affected by the respiratory act. The modification in the permanent form of the chest which supervenes upon this condition, is tolerably well-known as the "cylindrical" or "emphysematous" chest; it is marked by increased fulness and prominence of the whole anterior thoracic vault; often also, but not invariably, by increased arching of the sternum from above downwards; and perhaps yet more characteristically by a diminution in the lateral, and a relative increase of the antero-posterior, diameter of the thorax.

The true relation of these changes to the existence of collapse posteriorly, and emphysema anteriorly, in the lungs, is not altogether so clear as it may at first sight appear. That the permanent modification of form is the consequence of the peculiarly altered movement of the chest which I have described above, will admit of little doubt to those who have witnessed this movement in characteristic cases of acute and chronic bronchitis. It may also be freely admitted that the diminished lateral motion is the direct effect of the diminished expansion of the lung in consequence of bronchitic accumulation, with partial collapse, and perhaps subsequent atrophy of its tissue. But to ascribe the increased movement and consequent deformity of the anterior part of the chest, to the production of emphysema, appears to me an error both of logic and of observation. I believe, on the contrary, that whatever be the relation of emphysema of the lung to the "emphysematous chest," *it is not directly or indirectly the cause of that deformity.* And this conclusion appears to be borne out by the following considerations :—

In the first place, the increased respiratory movement in the anterior part of the chest, which appears in all cases to be connected with the generation of emphysema, as well as of the deformity above-mentioned, exists in a large number of instances of bronchitis, before either the one or the other condition has yet arisen; indeed, in its slighter degrees, I believe the increased anterior thoracic movement to be an almost constant concomitant of that affection. In the second place, the existence of very well-marked emphysema, though unquestionably concurring with the highest degrees of the deformity, has always appeared to me to tend to diminution of the abnormal excess of motion; this excess being always detected most characteristically in company with simply puerile, not emphysematous, respiration. It appears, therefore, impossible that the generation of emphysema can be the cause of that exaggerated motion. Lastly, according to arguments and observations already laid before the reader, it appears that emphysema is a lesion directly due to the

forcible expansion of the chest under peculiar circumstances, which seems fairly to exclude the opposite proposition, that undue permanent expansion of the chest can ever be owing to the existence of emphysema.

From observations on this subject, it appears to me susceptible of demonstration,—that the abnormal motion of the chest, in the cases above alluded to, always precedes both the deformity and the emphysema; that the emphysema frequently precedes the deformity, but in its more chronic and exaggerated forms generally follows in its wake; that a certain amount of emphysema may exist without deformity, and a certain amount of deformity without marked emphysema; and that, in any given case, when emphysema supervenes on exaggerated anterior movement, with or without deformity, its natural effect is to diminish that excessive movement. This last proposition is strongly confirmed by the state of the lung in extreme emphysema, in which the emptying of the air-vesicles is effected with great difficulty, or even may be absolutely impossible, owing to the existence of an apparently valvular obstruction to the egress of air; a condition which suggested to Laennec his theory of emphysema, but which I believe to be a secondary effect, and not a cause of that structural alteration.

Were I to hazard a speculation as to the mutual connection of this complicated series of phenomena, it would be that indicated in the following propositions, which I submit to the reader, not as ascertained truth, but simply as being the most probable conclusions at present attainable in relation to this subject:—1. The direct tendency of bronchitis is to produce bronchial accumulation, and thereby to restrain the expansion, or even to produce retraction, of the whole lung, and consequently of the chest. 2. To overcome this tendency, forced respiration is at once thrown into action, and the breathing, from being, as in the normal state, mostly diaphragmatic, becomes in a high degree costal and thoracic. 3. In overcoming resistance, by means of costal superadded to diaphragmatic respiration, those parts of the chest whose movements are performed by the most powerful muscles, acting at the greatest mechanical advantage, tend to assume the principal function, while the remaining portions fall into abeyance, or yield in part to the opposing resistance. 4. On this principle the elevation of the sternum, and of the anterior ends of the true ribs, which is effected by the powerful aid of the cervical muscles in addition to the intercostals, becomes the predominating movement along with the descent of the diaphragm; while the motions of the posterior and lateral parietes of the chest, which are maintained, in the normal state, by a much weaker force, tend to fall into abeyance. 5. The respiratory forces, instead of acting equally on all parts of the pulmonary surface, and tending to expand it from all points at once, are thus spent in greater measure upon the anterior edge and upper part of the lung, which are in contact with the most mobile parts of the thorax, as well as upon

the lower edges and diaphragmatic surface; and these parts, therefore, become the principal seats of respiratory movement, while the root of the lung and its lateral and posterior surfaces only receive the inspiratory impulse secondarily, or in greatly diminished ratio. 6. The consequence of the inferior power of movement in the posterior and lateral parts of the lung, is accumulation and stagnation of mucus in the tubes; thence a greater liability to pulmonary collapse and atrophy as the consequence of bronchitis. 7. The consequences of the superior power and greater extent of movement at the edges and upper parts of the lung, and on the diaphragmatic surface, are comparative freedom from mucous accumulation, and consequently from pulmonary collapse and its consequences, and on the other hand, much greater tendency to the development of emphysema from violent and repeated forced inspiration, when partial collapse or atrophy is present elsewhere. 8. The irregularities of movement of the thorax tend ultimately to affect its form, producing in the child the *pigeon-breast*, by lateral flattening of the yet flexible and soft ribs, with depression of the lower costal cartilages, and protusion of the sternum; in the adult or older child, slighter lateral flattening, with expansion or bulging of the cartilages, and arched protrusion of the sternum; and in both the child and the adult, increase of the antero-posterior diameter relatively to the lateral, and of the upper zone relatively to the lower. 9. The deformity of the chest usually accompanying emphysema of the lungs is neither a cause nor an effect of that lesion, but both emphysema and the "emphysematous chest" depend on the altered respiratory movements in bronchitis, and the exaggerated respiration necessary to overcome the tendency to bronchitic collapse of the lung.

It may appear to some readers that the above explanation of the seat of election of pulmonary collapse and emphysema is superfluous, and that the gravitation of the mucous obstructions in bronchitis to the posterior portions of the organ, is a sufficient reason for the occurrence of collapse in that situation, and of emphysema in the opposite region. To this opinion, however, some facts stand in direct opposition. The most important is that in the horse, in which emphysema and the other diseases of the lung are common, and in which the position of the lung as respects the effect of gravitation is precisely the reverse of what occurs in man, the seats of election of emphysema and of pulmonary condensation are nevertheless nearly as in the human subject. In various experiments on the rabbit, also, I have noticed the same tendency of emphysema to the borders of the lung, and of collapse to its root, although the animals were allowed to maintain the natural position, in which the force of gravitation ought to have had an opposite tendency. For these reasons, I have been induced to ascribe very much less effect than most observers to the simple statical condition of the fluids in pulmonary diseases, and to look for some dynamical cause which would explain the position of the lesions found in bronchitis, pneumonia, and em-

physema, in a more satisfactory manner than hitherto. To what extent the preceding paragraph is a successful attempt at such an explanation, must be left to the judgment of the reader, and to the future observation of facts bearing on the subject.

Pathological alterations of the Bronchi in Pulmonary Atrophy and in Emphysema.—The memoir of M. Reynaud[1] on obliteration of the bronchi, has been referred to by most subsequent writers as having enumerated and described with great completeness all the more ordinary varieties of permanent contraction or dilatation of the air-passages. Indeed it is difficult to conceive anything more completely exhaustive than this memoir, when considered purely in an anatomical point of view, and solely with reference to the air-passages; and having frequently had opportunities of verifying nearly all of his observations, I find it, like most others who have referred to them, not easy to state anything novel upon this subject. But M. Reynaud's researches, though full of anatomical truths, are strangely barren, at least in his own hands, of real pathological interest; which arises chiefly from his having too exclusively pursued the inquiry relative to the bronchi themselves, and not having sought to connect their alterations with those of the pulmonary tissue, with which they are, according to my experience as well as that of others, constantly and indissolubly associated. Somewhat of the same objection applies to Laennec's observations on dilatation of the bronchi, which first gave to this disease a place in pathological anatomy. Accordingly it has been reserved for future observers to discover, that both the dilatation and the contraction of the bronchi are almost always secondary lesions, or at least invariably connected with some kind of disorganization of the pulmonary air-cells. Several of the later pathologists have adopted these views, with more or less decision; among whom may be mentioned Hasse, Rokitansky, Stokes, &c.; but the ideas of Laennec and of Reynaud have still been adopted by many writers on the diseases of the chest, with perhaps too little discrimination, and very little real advance has been made in the pathology of these affections.

The forms of obliteration and contraction described by Reynaud are numerous. It is scarcely necessary to enter into the consideration of the anatomical varieties. The different kinds of dilatation, as described by Laennec and Reynaud, and their relations to obliteration of other parts of the same bronchial divisions, are of more interest. The most frequent are the following:—A small bronchus of normal caliber suddenly opens out into a sacculated dilatation, lined by smooth thin membrane, and of more or less rounded form. This dilatation sometimes terminates all trace of the bronchus; in other specimens, the contracted and obliterated remains of bronchi pass from its opposite end towards the circumference of the lung.

[1] Memoires de l'Academie de Médecine. Tome iv.

Again, a bronchus may show a succession of marked irregular dilatations through its whole length, at some parts having the sacculated character, at others being irregularly cylindrical, with partial annular projections, and transverse septa arising from the walls of the dilated tube. Finally, the whole of a lung or of a lobe may be broken up into a series of cavities, having free communication with each other, and with the main bronchi of which they are presumed to be dilatations.

On all the changes here described, there is one important remark to be made—they are invariably found in close connection with atrophied lung, either of that kind which results, as we have seen, from bronchitic collapse, or some of the more complex varieties which proceed from other lesions, such as tubercle or chronic hepatization. With regard to obliteration or contraction of a bronchus, it is indeed self-evident that this must be the case; and the reader of the foregoing parts of this memoir will see without difficulty by what steps the obstruction of the air-tubes leads to their contraction, along with the collapse and gradual atrophy of the vesicles to which they lead. In the case of dilated tubes, it is an observation of all the later pathologists, in which I fully concur, that the pulmonary tissue around these dilatations is usually impermeable, and in a condition of fibrous atrophy, most commonly without marked induration. It was this circumstance which gave rise to the theory of bronchial dilatation by Dr Corrigan, which led him to call this disease "cirrhosis of the lung,"[1] conceiving that the formation of a peculiar contractile fibrous tissue in the interstices of the bronchi, and the obliteration of the air-cells, led to the expansion of the tubes, by the gradual operation of the inspiratory forces, and constituted the true pathological condition of the disease. Although I have little doubt that Dr Corrigan's theory is fallacious, in so far as regards the existence of any *new* or *peculiar* fibrous element in this affection, yet there is no doubt that his observations were in other points correct, and that in particular he has the merit of being the first to draw attention in a decided manner to the morbid alteration and obliteration of the air-cells, a fact singularly enough nearly overlooked by Laennec.

What, then, is the origin of bronchial dilatation? The explanations of Laennec and others, which ascribe it to violent coughing, to distension by accumulated mucus, &c., are clearly unsatisfactory, on precisely the same grounds as have been already indicated in the case of emphysema, and which there is therefore no occasion again to repeat here. As in the case of emphysema of the vesicles, it seems more consonant with reason to ascribe these dilatations, as is done by Dr Corrigan, to the expansive forces of inspiration acting upon the bronchi of atrophied lung. But it is difficult to

[1] Dublin Journal. May 1838.

understand, on this principle, the occasional partial character of the lesion—the expansion of one portion of a bronchial tube into a sacculated globular enlargement, while adjoining tubes and adjoining portions of the one affected, retain their natural size. In such cases it becomes necessary to suppose the existence of some more local affection, rendering the bronchial tube dilatable at the point in question.

To those who have studied this subject only in the light of Laennec's description, the following remarks will probably appear too bold and sweeping a generalization. They are, nevertheless, the result of much consideration, both of the descriptions of authors, and the facts observed in very numerous dissections, as well as in most of the public pathological collections of this country. The conclusion to which I have been led by this survey is, that almost all the so-called bronchial dilatations, and all of those presenting the abrupt sacculated character here alluded to, are in fact the result of *ulcerative excavations* of the lung communicating with the bronchi. That such ulcerations are not uncommon in bronchitis, especially in the case of children, has been already sufficiently indicated in the first part of this memoir, under the head of bronchial abscess. I have, in fact, incised the bronchi with great care in cases of recent bronchitis with bronchial abscess, and have found the small cavities so described to occupy precisely the same relations to the caliber of the tubes as the larger dilatations which are found in connection with chronic atrophy. The expansion of these small cavities, either by increase of ulceration, or by the act of inspiration, would clearly in these cases have led to an appearance closely resembling the so-called dilatations of the bronchi in everything except the fine, smooth, and consistent lining. On the other hand, the examination of a very great number of unquestionable instances of chronic pulmonary excavation from tubercle and other causes, has satisfied me that cavities originally formed by ulceration, may become lined by membranes exactly resembling those found in the "dilated bronchi" of Laennec. In no instance that I have seen, has this membrane exactly resembled the mucous membrane of a bronchus; even in the cases of so-called true bronchial dilatation, it is thin, dense, very smooth and glistening, and with comparatively few vessels; in fact, more resembling a serous than a mucous tissue. This description altogether concurs with those of Hasse [1] and others as applied to bronchial dilatation. It is, I think, almost conclusive upon this question, that in chronic cavities, evidently of tubercular origin, I have been able to trace quite satisfactorily the gradual assimilation of their lining to this type; and in several instances to observe cavities, which, but for the existence of others in the same lung in a different condition, would scarcely have been distinguishable from

[1] Pathological Anatomy—article, Bronchiectasis.

the so-called bronchial dilatations; they being lined by membrane perfectly smooth and glistening, and gradually passing into that of the undilated portion of the tubes leading into them. I have also observed that in such cases there is even the formation of an incipient epithelium upon this new membrane; or, at least, of numerous cells which, under the microscope, sufficiently resemble the columnar epithelium of various parts of the air-passages, differing, however, from that of others, and especially of the trachea and larger bronchi, in being of inferior size, and never, so far as I have observed, furnished with cilia. The opportunities of making such observations under the requisite conditions to ensure accuracy, are too rare to permit of my entering into this subject at greater length.

If these observations are admitted as bearing on this question, it will, I think, become probable that the usual origin of bronchial dilatations is in cavities formed in atrophied lung, in consequence of bronchitis or tubercle, and afterwards expanded beyond their original dimensions by the inspiratory force. The conditions that conduce to such expansion have been already considered under the head of emphysema, and its relation to excavations. It will be at once evident that the tendency to expansion of a cavity must be great in proportion to the flaccidity of its walls, and the absence of crepitant lung in its neighbourhood. It is owing to this circumstance, that bronchial dilatation and emphysema of the lungs have been found, to a certain extent, mutually exclusive. The cases most fitted for the development of such permanent excavations, are those in which the whole of one lung has been converted into a series of cavities, with no intervening crepitant tissue. Of this I have seen several examples. One such instance, figured by Cruveilhier,[1] forms an admirable illustration of Corrigan's cirrhosis, and of the real mode of its origin, viz., by ulceration. The whole upper lobe of the lung is converted into the condition of chronic cavities, lined by smooth membrane, and communicating freely with each other; while in the lower lobe are found recent excavations; and every intermediate condition between the two varieties can be readily traced. Of this case I have on one occasion seen an almost exact counterpart, in a boy, aged about 12 years, affected for a long time with hepatic and pulmonary disease, under the care of Dr Renton.

Pulmonary Concretions and Cicatrices.—The existence of cicatrices and puckerings in the pulmonary tissue, sometimes accompanied by distinct induration, with much thickening of the pleura in the neighbourhood, and sometimes by rounded whitish masses of atheromatous, chalky, or stony consistence, imbedded in the tissue

[1] Anatomie Pathologique, Livraison, 32. Pl. 5. Fig. 3. The case was considered to be of tuberculous origin, yet "the cicatrization (of the excavations) was perfect; the parietes presented the appearance of accidental mucous membranes."

of the lung, and surrounded by a fibrous cyst, has been long known to morbid anatomists, although more attentively studied of late years. Morgagni, summing up his own experience with that of his predecessors, signalised their existence in connection with asthma and other symptoms of disease of the respiratory organs.[1] Portal considered calculous concretions of so much importance, that he indicated, by means of them, a particular species of phthisis, the "phthisie calculeuse." He maintained the entire dissimilarity between the calculi of gouty and those of scrofulous origin, showing their frequent connection with phthisical symptoms, cough, hemoptysis, dyspnœa, &c.,[2] which, however, he did not consider as exclusive of the gouty diathesis, inasmuch as he has a "phthisie arthritique et rhumatismale." Portal also considered these calculi as formed in some instances by inspissation of the bronchial humours, as well as by dust inhaled from without, — an opinion which has received some recent confirmation in the special case of the stone-hewer's phthisis, but which, as a general deduction, is contradicted by the chemical nature of these bodies, the pulmonary calculi being now known to consist in a great part of phosphate of lime, and other calcareous and magnesian salts, evidently of organic origin. Laennec was the first distinctly to protest against the opinion that these pulmonary concretions were necessarily attended by symptoms; having, as he says, frequently found them "in subjects who had presented no sign of oppression or embarrassment in the respiratory organs." The description by Laennec of these bodies bears the marks of a very attentive observation of all their pathological relations. He notices their frequent occurrence with or without the accompaniment of other lesions, and remarks that they are occasionally found in the centre of tubercles, and very frequently along with pulmonary cicatrices, like those which are found in tuberculous individuals; from which circumstance he concludes, that, "in the greater number of instances, they are the result of a cured tubercular affection." He does not, however, deny that osseous and cretaceous concretions may be developed independently of tubercle, but regards this as occurring very rarely.[3]

Since the enunciation of the above opinion by Laennec, as to the source of these pulmonary lesions, it has been for the most part acquiesced in by pathological authorities; and, as usually happens when a doctrine gains general support, even the prudent reservations of its author have been in some danger of being consigned to oblivion. The accuracy of Laennec's observation as to the fre-

---

[1] De Sed. et Causis Morb. Epist. xv. 17, ad finem.

[2] Phthisie Pulmonaire. Edition of 1809. Vol. i. 478 et seq.; vol. ii. 321, 349. The connection of calculi with phthisis was also maintained by Bayle, as well as other still older authors. See Sauvages' Nosologia Methodica—article Phthisis.

[3] Auscultation Mediate; tome 2, chap. 4.

quency with which these lesions occur, is more than justified by the later researches of M.M. Rogée and Boudet of Paris, and Professor Bennett,[1] who have found that pulmonary contractions and puckerings, with or without concretions and thickenings of the pleura, occur in a very large proportion of the bodies subjected to examination in hospitals. In the cases of Dr Bennett, the proportion is about 40 per cent. of the whole; while M. Rogée and M. Boudet give respectively 51 and 86 per cent. in their different spheres of observation. It thus becomes a matter of considerable importance to determine whether the connection of these lesions with obsolete tubercle be subject to no exception; more especially as none of the observers above noticed have, in drawing inferences from the facts adduced by them, indicated any doubt upon this subject. I have thought it right, therefore, to make the following remarks, tending to limit the application of the doctrine of Laennec, which in being made to include all, or nearly all, pulmonary cicatrices and concretions, under the designation of healed or obsolete tubercle, appears to me to have been scarcely warranted by the facts of the case. That I may not be suspected, however, of an equally exclusive bias upon the other side, I may state that the healing of tubercles in this particular manner admits, in my opinion, not of the smallest doubt; and that to any one who has seen, on a sufficiently large scale, the progress of these lesions, as exemplified in the lungs of those dying of unquestionable tubercle, the conclusions of Laennec as to the frequent cure of tubercular lesions, especially in their early stages, must appear perfectly irrefragable. Nor is there anything in the doctrines of Carswell, Cruveilhier, or other subsequent writers, from which I feel called on to dissent, in so far as they illustrate the different modes in which this cure is accomplished. But, as regards the precise frequency with which the early stages of tubercle become obsolete, I believe that we are not yet in possession of accurate statistical results; and that neither the 40 per cent. of Dr Bennett, nor the 51 and 86 per cent. of the French observers, represent correctly the proportion of such cases in our hospital bills of mortality.

That simple bronchitis must be responsible for a certain number of pulmonary contractions and puckerings, will be at once evident to the reader of the preceding pages. Every instance of pulmonary atrophy, from whatever cause, which is abruptly defined and surrounded by normal or emphysematous tissue, will necessarily present the appearance of a cicatrix,—more especially if the pleura over it be, as often happens, thickened. It might be supposed that such cicatrices would in simple bronchitis occur chiefly or exclusively at the back part of the lungs; but this is by no means the case; for while partial atrophy occurs with extreme frequency at the back part of the lungs, it is rarely complete, and almost always in

[1] See the paper of Dr Bennett, in Ed. Med. and Surg. Journal, vol. lxiii., p. 406.

the diffused form; while the obvious pulmonary cicatrices arise from lobular atrophy, which occurs chiefly at the edges and upper parts of the lung. I know of no means by which a simple cicatrix, formed by bronchitis or broncho-pneumonia, surrounded (as such cicatrices often are) by a certain amount of induration and carbonaceous infiltration, could be distinguished from a tubercular lesion, unless the absence of tubercular traces in other organs, and presence of lobular atrophy along the whole edges of the lung with diffused incomplete atrophy behind, and the comparative exemption of the summit of the lung, be considered to indicate such a distinction. Even to one aware of all the characteristics of both forms of disease, I believe cases will constantly occur in which no distinct opinion can be formed; at least such is the result of my experience since my attention has been particularly drawn to this subject. Nor can I for a moment doubt, considering the frequency of bronchitis at all periods of life, that a proportion of pulmonary cicatrices, quite large enough to modify considerably the statistical results alluded to, has been included among the tubercular lesions without having a just claim to this designation. This is particularly the case with M. Boudet, whose enormously high proportion of 86 per cent. could only have been attained, as it appears to me, even among old subjects (and it is not asserted that his were exclusively such) by diligently seeking out every trace of pulmonary contraction, by whatever cause produced. By such a method the traces of extinct pulmonary disease may indeed be discovered in a very large proportion of cases; but certainly not of extinct tuberculous disease.

With regard to concretions, which were found by Dr Bennett in about 22 per cent. of the bodies opened by him, I have little doubt that they have a tuberculous origin in a large proportion of cases. But something in the way of reservation requires even here to be kept in view. The occurrence of such obsolete masses of exudation in connection with old-standing bronchitis is far from uncommon; and as Laennec, and after him many others, have clearly traced tuberculous matter through all its stages into that of complete calculous induration, so has it occurred to me repeatedly to see old bronchial abscesses, having evidently the characters and the usual distribution of bronchitic lesions, in every stage of conversion into these bodies. Even when the conversion was complete, it has appeared to me that a certain proportion of those I have met with might be reasonably referred to this source, from their prevalence at all the borders of the lung, or from their being surrounded rather by what I have called *simple lobular atrophy*, than by any considerable induration. But in this, as in the former case, the observer who looks to all the possibilities of the case is apt to find instances of a sufficiently equivocal and doubtful character in the present state of our knowledge.

Without being prepared to defend the following conclusions as absolute, and without at present entering at length into the grounds

for some of them, I may here endeavour to state briefly the inferences to which I have been led by the preceding and other researches in regard to such lesions of the lung as may be suspected to be connected with the extinction of tuberculous disease. 1. There can be no reasonable doubt that open excavations, one or many, completely cicatrised on their internal wall, and lined by a membrane possessing the appearance, and in some degree the minute structure, of an epithelial membrane, may be of tuberculous origin;[1] but such excavations (the *dilated bronchi* of Laennec) may also originate in simple bronchitis, broncho-pneumonia, simple isolated abscess (a very rare disease), multiple or metastatic abscess, syphilitic ulceration, gangrene of the lung,—which last lesion also may have several causes. Of these forms of healed excavation, the tuberculous is probably the most frequent; but there are as yet no good characters to distinguish it from the others, unless collateral lesions, sufficiently characteristic, be found in other organs, or unless the original lesion of the lung be, in other parts of the same or opposite organ, in an earlier stage of its development. 2. Concretions of inspissated pus, atheromatous matter, cretaceous or calculous matter, surrounded by atrophy, with or without induration, are in the great majority of cases the remains of obsolete tubercles, softened or miliary; but they may also arise from any other form of pulmonary ulceration or abscess, as above enumerated. 3. Pulmonary atrophy, simple or with induration, and carbonaceous deposit, accompanied or not by adhesions and thickening of the pleura, may arise from obsolete tubercles, from bronchitis, from broncho-pneumonia, and probably also, though more rarely, from simple pleuro-pneumonia; and pulmonary cicatrices arise frequently from all these sources except the last. The *relative* frequency of these lesions as leading to cicatrices is as yet undetermined. 4. The lesions above mentioned (1, 2, 3) *are probably tubercular*, if they occur exclusively or chiefly at the apices and back parts of the upper lobe of both lungs at once; or in the apex of one lung only, without trace of a lesion elsewhere; or generally diffused throughout both lungs, but chiefly in their upper lobes, and especially at their back part and apex; or in any case in company with characteristic traces of tubercular lesions in other organs. In reference to this last clause, it is doubtful whether cretaceous or other deposits in the bronchial glands can, in a case of such pulmonary disease, be considered as a characteristic indication of the tuberculous taint; but such deposits in other parts of the lymphatic system, especially in the mesenteric and cervical glands, or the traces of old deposit or ulceration in the mucous membrane of the small intestine, would necessarily determine the tuberculous nature of the affection, except in some cases, rare enough in this locality, but of which I have seen examples, in which typhoid or cancerous disease might throw doubt upon the diagnosis.

[1] See the text of Cruveilhier, loc. cit.

5. All these lesions *are probably non-tubercular* if they occur in one lung in a generally diffused form, without traces of tubercle, even obsolete, in the other lung; or in the lower lobes to the exclusion of the upper; or at the edges of the lung in both lobes and not at its apex; or at the root of the lungs only; being in all these cases unaccompanied by tubercles, or the traces of tubercles, elsewhere. 6. After employing all these characters of distinction, a certain number of cases of all the lesions in question will remain of indefinite or unknown origin.

In concluding this series of papers on bronchitis, I am well aware that many important practical relations of the various subjects discussed have been necessarily left almost untouched; but if I have succeeded in throwing any light on these relations, or in giving the practitioner a key to their apprehension in any degree simpler and of more extensive application than has hitherto been accessible, I am confident that the numerous minds at work in this country and elsewhere in the furtherance of practical medicine, will not allow these researches to remain destitute of the assistance which they are calculated to derive from more extensive opportunities of clinical research than I have been able to command. With this conviction, I leave the foregoing papers in the hands of the profession.

Part Second.

REVIEWS.

A Practical Treatise on Diseases of the Urinary and Generative Organs (in both Sexes). By WILLIAM ACTON. London. 1851.

ON turning over the leaves of this thick volume, to learn what sort of matter they contained, we were struck with the following passages:—

"But is it true, as asserted and believed, that the prostitute dies after a few years of following her calling? In a table, published elsewhere, it is shown, from the return of the registrar-general—that infallible table of the causes of death—that a very few women die of syphilis in the metropolis; only twelve females died in the course of the year 1845. . . . . What, then, becomes of the large number of women who resort to prostitution for a livelihood? I have every reason to believe that by far the majority soon cease to have promiscuous intercourse, and return to a more or less regular course of life. . . . The sufferance, annoyance, and want, attendant on the vocation, have the effect of driving all from the streets, except some few, who seem to thrive in proportion to their age. . . . . We must recollect that she has a healthy frame,

an excellent constitution, and is in the vigour of life, or, probably, would not be able to so abuse the gifts of nature. During the career she has run, she has obtained a knowledge of the world, perhaps beneath, more probably above, the situation in which she was born,—is it surprising, then, that she settles and is amalgamated with the poorer classes of society, or becomes a married woman, after first living in a state of concubinage with her husband? The better class of prostitutes become the wives of the mechanic, the clerk, and the petty tradesman; and as they are frequently barren, or have only a few children, there is reason to believe they live in a comparative state of affluence, unknown to many virtuous women burdened with families."—Pp. 18-20.

The clerks, mechanics, and tradesmen of London, are certainly much indebted to the author for explaining a circumstance so remarkable in their family history, which may, perhaps, be as new to them as it is to us. But still we feel inclined to suspect that the career of metropolitan prostitutes may not be quite so triumphant as is here alleged, and that the common belief as to their wretched fate may not be so remote from the truth as would appear from the tables of the Registrar-General. If ample courses of mercury be alternated with walking in the cold wet streets, and immoderate gin-drinking, the untimely end of these poor creatures may be entered under different titles, which will afford no trace of their peculiar avocation.

Proceeding in the expectation of meeting with other views, having at least the quality of novelty to recommend them, we were sorry to find that Mr Acton's publication was chiefly devoted to an exposition of the opinions and practice of M. Ricord of Paris. We say sorry, because we believe the system of treatment pursued by this gentleman, however beneficial to the pockets of practitioners, is eminently injurious to the constitutions of patients. We ventured to hope that the eyes of the profession had been so fully opened to the baneful effects of mercury, as to prevent any risk of a relapse into the error of supposing that it was the antidote of a poison, instead of being itself the malignant influence which gives rise to all the formidable effects attributed to syphilis. M. Ricord says there is only one primary sore, which requires, or produces effects requiring, the administration of mercury, and that even this sore, characterised by its induration, or in other words the Hunterian chancre, may be remedied by local means, through the use of caustic applied within three days of its existence. But if this period, which, however arbitrarily, has been fixed as the limit for effectual interference, should be allowed to pass, then the patient, during the remainder of his life, must be the slave of mercury:—

"During my late visit to M. Ricord's hospital, I asked the French professor 'How long must mercury be given after the disappearance of induration?' He acknowledged that six months at least may pass before leaving off the preparation, which he thinks ought to be kept up to nearly the same dose which has effected the cure for that prolonged time; 'and even then,' adds he, 'the patient must not be surprised at seeing the disease return.'"—P. 440.

Now, Mr Acton tells that, "of 112 patients he found in M. Ricord's wards at a late visit, 23 laboured under the indurated chancre." It may, hence, be imagined what an ample field is afforded to those who administer mercury on the ground of this particular character of the sore. But, as patients seldom remain always under the same practitioner for the remedy of venereal complaints, the precise condition of the primary affection must frequently be matter of uncertainty, and consequently constitute an additional claim for dosing with the poison.

Mr Acton says :—

"We were acquainted with three students, who had connection with the same *grisette* during one evening; one was affected with a phagedenic sore, the other was a long time recovering from an indurated chancre; the third had a simple excoriation, which was slighter than that which we had witnessed on the genital organs of the female, whom we had examined a few days after the debauch." . . . . "It is a curious fact, that a man may contract chancre at one time of the year, and it will not become indurated; he may recover, and in a few months contract disease again, when the sore will be characterised by induration." . . . . "Previous to the complete series of experiments which have established these facts, surgeons denied that a sore could be a chancre unless it was indurated; as we have, however, now reason to believe that the poison is one and the same, whether induration be present or not, it behoves us more accurately to study induration, in the hope that it will assist us in solving the difficulties surrounding this part of pathology."—Pp. 375, 423, 418.

From this it appears, that the character of induration cannot be owing either to peculiarity of the poison, or peculiarity of the patient's constitution, and also that, whatever it may depend upon, its absence cannot afford any positive proof of the poison which requires mercury not being present. Is it, then, to be tolerated that practice founded upon such principles should be considered in any respect different from the destructive delusion of the old mercurial school? We know how readily at the present time any folly in the mask of science finds favour with the public, and we also know that the simple treatment of venereal sores taught and practised in this part of the world, if adopted elsewhere, would reduce many practitioners from comfort to starvation; but, nevertheless, we cherish the hope of seeing the truth more generally diffused. It is, indeed, impossible to do this by talking or writing on the subject; but, as the managers of the Royal Infirmary of Edinburgh have nearly completed a great extension of the surgical department, which will double the number of beds, and provide accommodation for male patients labouring under venereal complaints, we trust that the ingenuous youths who resort from so many distant places to pursue their studies here, will carry with them such an abundant store of facts as may constitute a sound foundation for their own practice, and exert a salutary influence over that of their neighbours who have been educated in a mercurial atmosphere.

Mr Acton has transferred to his publication fifty of the most

objectionable pages from M. Lallemande's filthy book; but having lately had occasion to express our opinion of that work, we shall not inflict upon the reader any further remarks as to its injurious tendency,—except that, from what occasionally falls under our observation here, the good people of a certain metropolis are already reaping the fruits that might be expected from the scientific profession of arts suitable for the most unscrupulous empiricism.

Om Nödvändigheten af Vetenskaplig Kontroll öfver Gymnastiska Central-Institutet, med särskildt afseende paa den Medico-Gymnastiska behandlingen och undervisningen derstädes. Kritisk framställning af D. Stockholm, 1851.

On the necessity of a scientific control over the Central Gymnastic Institution, with special reference to Medico-Gymnastic practice and instruction. A critical exposition, by D. Stockholm, 1851. Pp. 103.

Perhaps few of our titled and wealthy supporters of charlatanry are aware that, issuing from the distant north, and in comparatively recent times, yet drawing its leading idea from the imaginative south, and from a remote antiquity, a new claimant for their intelligent patronage has placed itself in the field. When death, or the fear of death, seemed to approach too closely, there was doubtless a certain amount of luxury in surveying, before selecting, the different portions of the noble panoply of defence, which lay already before them to assure their safety. We can imagine a kind of enjoyment in the hesitation, with which they were enabled to pause between the two extremes of the illimitable doses of Morison and the infinitesimal ones of Hahnemann; and the further satisfaction they must have experienced in the consciousness, that, should either or both of these belie their hopes, there were still other opposites to which they might revert, and that they might transfer themselves to mesmerism, to be again transferred to hydropathy, with the soothing confidence that, in return for the "*Gieb's Gelt her*" of one or other of these infallibles, the desired succour was really lurking somewhere, as it was proffered everywhere. But now, when to these rivals is added *Kinesipathy*, the happiness of those who delight to prosecute their search for health through byepaths, however divergent, ought surely to be approaching its completion.

The new medical system of Kinesipathy, or Kinesitherapy,[1] de-

[1] The first evidently an absurd compound, from Κινέω, *moveo*, and Παθος, *morbus*; and the latter, more correct, from Κινέω, and Θέραπεία, *sanatio*.

rives its origin from Pehr Henrik Ling, a fencing-master, and teacher of gymnastics, in Stockholm, who died there, in 1839, at a moderately ripe age. Ling was unquestionably a man of considerable genius; and, as a poet, has acquired no small share of reputation and popularity among his countrymen. His principal work is an epic, on the subject of the Asar, or companions of Odin, and in this, according to the native critics, he has displayed the leading characteristics of his style, both in its merits and its defects. Wholly self-educated, he was frequently original, but generally crude, and lacked the art to give a graceful and consistent outline to his creations. The marvellous, degenerating into the grotesque, supplied the prevailing colour to his themes; and his original defect of cultivation subjected him to the farther danger of offending through mannerism and monotony. We have ourselves had some slight opportunity of judging directly of the style of this poem, which seemed to us to affect, in easy and harmonious verse, much of the simplicity of thought, without the simplicity of language and construction, of the ancient ballads of his country; while the proneness of the author to range in a world of fancy, unfettered by any of the usual rules of art, constituted a peculiarity which appeared in every respect consistent with the dreams which it was elsewhere his pleasure to graft upon his professional pursuits, and which a mind so constituted was sure to advocate with a zeal and enthusiasm, such as could not fail ultimately to attract observation from that considerable portion of the public which is ever found ready to accept novel and bold assertion in lieu of reason.

It was thus no common-place pretender, whose mental idiosyncrasies gave a bent to the pursuits of his vocation, and constrained him into a career of charlatanism. His scheme developed itself gradually. For many years his school resembled that which our own Roland has so successfully established in Edinburgh; in which fencing, athletic exercises, and feats tending to increase muscular vigour and agility, were practised, generally with a view to promote a robust, healthy, and symmetrical development of the frame, but more especially with an application to the kind of training requisite as preparatory to the efficient performance of military duty. In the year 1813 he attracted the notice of government, from whom he received an annual grant in aid of the maintenance of his establishment. Meanwhile he was engaged in superadding new adjuncts to his original system. Not confining himself to those sound and rational views which have been universally held, and which he himself originally entertained, regarding the wholesome effects of well-regulated exercise upon the general health and vigour, he began to regard his trained movements as therapeutic means, which could be successfully applied also to the special treatment of individual diseases of whatever description. With the natural timidity of a new speculator,

whose zeal had not yet outrun his reason, or rather, perhaps, in imitation of the ancient gymnasiarchs, he at first proffered these therapeutic movements as measures subsidiary to the aims of legitimate medicine, under whose sanction they were to be employed, and by whose rules they were to be guided. But, even if not mercenary, the poetic element had too much influence over the Swedish gymnast to permit him to continue long under such trammels. Other fancies came as he summoned them, and were nourished probably, at least in part, into convictions. It was now but a light step to assume the position of a wholly independent medical system; and the result has been that the establishment of Ling still subsists in Stockholm, under the superintendence of his immediate successor, and with the title of the Central Gymnastic Institution, and is still supported by an annual contribution from the Swedish Government, notwithstanding the monstrous and increasing effrontery into which its pretensions have lapsed; and through which it has not only drawn itself apart from legitimate medicine, but has even declared itself in a certain degree incompatible with it, while it has joined itself in a kindred alliance with homœopathy, in the person of Dr Liedbeck, the only homœopathic practitioner (let it be so far said in honour) within the Swedish dominions.

The course of study at this institution, though it extends only through a single half-year, embraces physiology, anatomy in its relations to the motor powers, descriptive anatomy, pedagogic and military gymnastics, and fencing; and if therapeutic gymnastics constitute no part of the regular course of instruction, they are at least a matter of daily practice. The regular pupils, at the close of their brief curriculum, undergo a public examination, and acquire a sort of right to devote themselves to the teaching and practice of gymnastics, either in the army or in the public schools throughout the kingdom. The present superintendent, Hr. Branting, has the reputation of being a skilful swordsman, as well as a dexterous athlete; but his acquaintance with the mysteries of tierce and quarte, the pole, the bar, or the giant's steps, would scarcely have brought him within the reach of our notice, had he not tampered with deeper mysteries, his unabashed attempts at which have rendered him one of the lesser notorieties of the day. Enjoying the support of the authorities, as well as the patronage of many of the leading members of society, the new medical doctrine of the gymnasiarch is confessedly attracting very general attention; through the exercise, doubtless, of those arts which are usually well understood by unscrupulous innovators, and by means of which the weaker portion of the world, ever of strongest faith, may be fawned or bullied into assent. Upwards of 300 patients were reported to have been treated at the institution in Stockholm, during the year from April 1848 to April 1849; and we have seen much larger numbers vaunted by those whose interest it is to extol the merits of the system. The authorities in St Petersburg

have already been induced to found a similar establishment; placing it, however, under the superintendence of the Medical College, a kind of surveillance which its partisans in Sweden have hitherto managed to elude. An attempt to introduce the system into France, to the credit of that country, has hitherto failed, having been apparently checked through a satirical critique by Malgaigne, which met it at the outset. London, of course, must anticipate an early visitation: for what system of imposture, or of extravagant pretensions, ever spared the open credulity and easy convictions of our liberal citizens? Already, indeed, a friendly correspondent in Stockholm informs us, two Swedish gymnasiarchs are on the eve of founding establishments in the metropolis, and we have heard of another which is proposed somewhere in the country.

Kinesipathy has as yet favoured the public with only a meagre body of literature: and Hr. Branting himself, though he appears to enter unhesitatingly on the treatment of the most serious diseases, is, nevertheless, unwary enough to confess, in his address delivered in 1850, that "medical gymnastics have not yet attained that development, through which they are in the position to assume a fully scientific form;" leaving us to infer, that only a very crude amount of knowledge entitles a gymnasiarch to tamper with human health and life. Ling published a few fragments in 1834, which have been since farther extended by Dr Liedbeck and Hr. Georgii, both connected with the institution. The annual addresses of Branting, at the public examinations of the pupils, convey a certain amount of information; while Dr C. U. Sondén, in 1840, has communicated some views upon the subject; and Dr H. E. Richter of Dresden, in 1845, published a sketch of the system. In the latter year, Branting stated that he was engaged in framing an exposition of its special doctrines and practice, which, however, has not yet appeared. In fact, though we have evidence enough of the grossness of the pretensions of the gymnasts, they have divulged little as to the peculiar methods which they employ, or the principles on which these are founded. We can gather, generally, that the therapeutic movements which they enjoin are either of an active, passive, or mixed character; that through these they pretend to be able to increase or diminish the vital action of any organ, and to promote or regulate the living energies throughout the whole frame, or in any of its parts; that this is effected by stimulating or retarding molecular motion, in the organs and tissues where those functions are to be fulfilled on which the maintenance of life depends; and that thus they can induce many effects analogous to those of ordinary medicinal agents. The movements may, in this way, be rendered either stimulant or sedative, tonic or relaxant, diaphoretic, purgative, or derivative, &c., and are employed as efficient substitutes for most of the usual remedies of the physician. But as to the strict relations between cause and effect, in the production of this wonderful agency, these they make no effort to develop,—con-

tenting themselves with the analogies to be derived from such remarkable discoveries, as that violent exertion causes palpitation, and fatigue sleep, or that more moderate exercise promotes the appetite, or gives energy to the functions of the skin, while certain forms of motion may induce vomiting. Branting offers a somewhat more specific rationale for his treatment of chlorosis, in which his aim is to increase the capacity of the chest and lungs, "that the blood-globules may be more highly animalized, so as to re-act upon the vaso-motor nerves, and through these arouse the normal innervation." Luckily for Hr. Branting, there are minds so constituted as to accept these loose assumptions as the choicest of mental nourishment, and who would turn from a severer logic with all the repugnance instinctively excited by an unnatural diet.

But what are the diseases in which the medical gymnastics have proved an efficient remedy? The catalogue is by no means a contemptible one. Hypochondriasis, epilepsy, tubercles in the brain, congestion of brain, asthma, neuralgia, pectoral complaints, tubercles in the lungs, hæmoptysis, purulent expectoration, laryngitis, diseases of the heart, rheumatic carditis, diseases of the liver, ascites, hydrothorax, gonorrhœa (!), and many others, not a few wearing the appearance of having been established by no very accurate diagnosis, have all been wrestled, concussed, or percussed into nothingness. To descend to more particular instances, a "pectoral complaint," of which we have no more exact definition, was successfully treated "through the contemporaneous employment of a modification of compression and concussion in the region of the axilla, combined with rotation of the arms." Hr. Georgii cites the case of a patient who, to all appearance, had tubercles in one of the hemispheres of the brain, and who was at first treated by "derivative movements," and afterwards by "circular percussions," lightly applied by the hand on the upper part of the head, and, of course, with extraordinary success. A young female, who had been afflicted for several years with a liver complaint, "complicated with ascites, hydrothorax, and anasarca," and with whom the remedy assumed the form of "concussion and compression under the ribs, on the right side," had, in the course of four months, the liver greatly diminished, the respiration rendered easy, and the chest and abdomen free from water; so that, Hr. Branting assures us, she could join with spirit in the dance,—not in itself, we conceive, an unpleasant description of gymnastic exercise. We have already noted that kinesipathy has made, in so far, a kind of compact with the equally rational system of homœopathy, and we may here add, that Branting has even approached the exceedingly notable discovery, as the result of most of his trials to determine the specific influence of the movements on healthy persons, that, considered generally, the series of movements which cures a disease will call forth the same, or closely analogous morbid symptoms, when employed upon a sound organism! After this sop to the admirers of

homœopathy, as a section of the "advanced spirits" of the day, we cannot wonder that phrenology should receive its tribute also. A young man, it seems, of a very imaginative disposition, had long suffered from sleeplessness. Punchings (*punkteringar?*) and slight circular percussions over the organ of ideality, were repeated for several days, without benefit. Recourse was then had to the same movements over the seat of the perceptive faculties, when there followed a cessation of the morbidly excited fancies, and sleep was recovered.

Such are the kind of facts and the doctrines which, says Branting, it "was reserved for Ling, a fencing-master, not a physician, to discover and to develop into a new science." We most cordially concede to him all the merit which is intended to be implied in this claim. But with regard to medical gymnastics, in the better sense of the term, no pretensions could be more utterly unfounded. As a part of physical education and of general hygienics, and as an adjuvant, with occasionally a special application, in the treatment of disease, no medical author of note, since the days of Hippocrates, has despised or neglected the subject, and a few even have claimed for it a more extended application. If it be true, however, that Herodicus, the master of Hippocrates, anticipated our fencing-master, by alleging for it an independent and specific influence in the cure of individual diseases, his immortal pupil rejected the groundless dogma; and it is perhaps in direct controversion of it, that he speaks of gymnastics and medicine as opposed to each other,—the one being for the healthy, and the other for the sick. Our own Fuller, in his once popular work (*Medicina Gymnastica*), which, in the year 1718, had reached its fifth edition, and which we recommend to such of our readers as love to see how boldly, and in what sturdy language, our predecessors could deal with a hypothesis, ascribes, to say the least, to certain parts of the subject all the remedial value they deserve. In short, from the time of the *pentathlon*, down to those of the modern *turnkunst*, the uses of gymnastics have never received less than their due consideration, while not unfrequently an inconsiderate zeal has lauded them to excess.

But it is a characteristic of a weak or of an irregularly trained mind, that, if it once seizes upon a substantial idea, it allows it to become the bias and the master-movement of its existence. We believe it to be utterly beyond all human ingenuity, to contrive any system of either speculation or doctrine, which shall not contain somewhere, at least some portion of either positive or relative truth. This portion, the original substantial idea of the speculator, is paraded everywhere as his standard of faith, in other matters with which it has no necessary or reasonable connection. And thus, upon the real truths relating to medical gymnastics, has Ling contrived to rear his ridiculous fabric of error and unreason. We have no intention of insulting the sagacity of our readers by enter-

ing upon any critical examination of the facts which have been cited in proof of his doctrines, and of which we have previously quoted a few examples, simply because they carry with them their own refutation in the preposterousness of their details. We set aside the question of fraud, and that worst of frauds which preys upon the public wealth through the medium of a still greater treasure, knowing that where the question of scientific truth is once decided, the moral question cannot long remain in doubt. As to the able pamphlet, the title of which we have placed at the head of these remarks, and which, though anonymous, is understood to be the production of Baron Gust. von Düben, if we have any fault with it, it is, that it treats the whole subject and its promoters with too much respect. The main object of the writer is to have the Gymnastic Institution brought under proper medical superintendence, so that its claims upon the public confidence may be duly tested. His own views are thoroughly orthodox; but we should have been better satisfied if he had directed the controversy less upon matters irrelevant to the main question, and where the victory might be doubtful, and worthless to either party when obtained, in as far as the true merits of the new theory and practice are concerned. Indeed, thus to argue on minor points, and perhaps to allow the establishment of subordinate principles, just in themselves, but not just in their ultimate applications, is rather, sometimes, to give the appearance of the victory to the weaker side, at least in the eyes of the very wide class of careless or incompetent judges.

We may have already occupied too much space with this topic; but we have a kind of pleasure in seeing these drunken helots of science reeling before the world, as a warning and an example. The feud which such heresiarchs urge against medicine is not that which can lead, in as far as they are concerned, to a unity of faith. Even if they could conquer what cannot be conquered—an eclectic science—their inevitable course would be to turn their weapons against each other: homœopath against mesmerist, hydropath against hygeist, or kinesipath or isopath against all or any. But these systems, writhe and wrestle as they may, can have no other fate than to fall successively, as scarcely discernible ruins at the base of the pyramid of science; and the little portion of truth which it may be possible to develop from amid their audacious absurdities, will become the ultimate property of legitimate medicine, whose goodly fabric, wearing the signs of time but not of decay, like a mighty crystal is ever gathering to itself all pure and precious particles from the mass that is floating around it.

The First Step in Chemistry. By ROBERT GALLOWAY, F.C.S., Lecturer on Chemistry, Putney College. London, Churchill.

DURING the last few years there has been no lack of excellent introductory works on chemistry, all more or less adapted for elementary instruction. Mr Galloway, however, in the little work before us, has treated his subject with some degree of originality, and has produced an introduction to chemistry well suited to serve the purposes of self-tuition, for which it is principally intended. It is not a *popular* work (using that term in its now commonly-received acceptation), but it is very elementary, and entirely confined to the general properties of the elements, and the laws and forms of combination. These subjects are clearly and concisely discussed, and to each chapter is appended a series of practical exercises, which form a very excellent feature in the work, and must prove a valuable means of habituating the student to a careful consideration of the nature and phenomena of the different chemical changes.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE—PROFESSOR BENNETT.

CARBONACEOUS LUNGS.[1]

Thomas Wilson, æt. 38, collier, from the Oakley Mines near Dunfermline, was admitted into the Clinical Ward 26th July 1851. He states that he has been employed about coal mines for the last twenty-four years, during the principal part of which he has been occupied in blasting rock for the sinking of pits, using large quantities of gunpowder daily. He is much exposed to cold and wet in this occupation, working almost constantly with wet feet, and frequently with the whole of his clothes drenched. During the laying open of a new surface for the working of coal, he has often worked in an atmosphere barely capable of supporting the combustion of the miners' lamps; he has observed this particularly when working iron-stone. Notwithstanding, it was only about seven months ago that he began to be troubled with short dry cough, and difficulty of breathing. He continued to work till about four months ago, when the cough became more severe; he had much dyspnœa, and frequent profuse perspiration, especially at night. Two weeks after this,—that is, about fourteen weeks ago,—he was laid up from work; and about the same time, while under treatment, he began to spit much, and observed that the expectoration was of a deep black colour. The change was so sudden as to alarm him considerably.

[1] Case reported by Mr William Calder, clinical clerk.

On admission, the chest is of large dimensions and well formed. On percussion, there is slight comparative dulness on the left side of the chest over the upper half in front; and there is also appreciable dulness over the same extent on the same side posteriorly. There is complete resonance on the right side anteriorly and posteriorly.

On auscultation, for some inches below the clavicle on the left side, the inspiration is rough and harsh, and there is prolongation of the expiration. Lower down on the same side, there is also fine moist râle, and the vocal resonance is somewhat increased. On the right side there is loud pealing vocal resonance both anteriorly and posteriorly; it is particularly loud in front immediately below the clavicle, but there is nothing abnormal to be detected in the respiratory sounds. The sputum is in considerable quantity, adheres tenaciously to the bottom of the spit-box, and is of an intense black colour. On being placed in water, a small portion of a lighter colour swims on the surface, but the greater part sinks immediately. On being allowed to stand for a little, the supernatant water becomes nearly clear, the dense black mass remaining at the bottom of the dish. Acetic, sulphuric, and nitric acids, and also aq. potassæ, though boiled with the sputum, do not in the least affect its colour.

On placing a small portion of the sputum below the microscope, many of the epithelial cells are seen loaded with the black carbonaceous matter. In some of them, the nuclei are evident, the matter being deposited between them and the cell walls; in others, the nuclei are quite concealed. In other parts of the field, all appearance of cells is lost, and nothing but apparently homogeneous black masses are seen. Some of the cells may also be seen ruptured, and the black contents poured out.

The expansion of the chest is equal on both sides. Breathing is equal and unembarrassed while he is sitting or lying in bed, but on using much exertion, it becomes short and difficult. There is some cough, but it is not very troublesome. States, that at the commencement of his illness, he could not lie on his left side; but at present he can lie in any position without uneasiness.

The other systems are quite normal. Has good appetite, no sour or unpleasant taste in his mouth. Bowels are regular at present, and are generally so; has never had diarrhœa. Does not sweat at night, or when he is not employed at his work. Pulse during examination was 88, soft and full; during his stay in the house, has averaged about 76. He was ordered an expectorant mixture; and during the last fortnight he was in the house, he had three tablespoonfuls of cod-liver oil daily. He went out by his own desire in August.

Commentary.—The physical signs existing in this case leave us in little doubt that there was considerable condensation at the apex of both lungs. This was indicated on one side by considerable dulness on percussion, and on the other by a pealing vocal resonance. It is true, the resonance in this place was unaccompanied by any change in the percussion note, a circumstance that may be caused by the existence of slight emphysema counterbalancing the increased limits of the pulmonary tissue. The history of the man's case, the nature of his employment, and the black sputum, at once indicated to us that this condensation was owing to the accumulation of carbonaceous matter in the lungs, a disease which is peculiarly apt to occur in coal-miners, the moulders in iron and copper, and a few other trades. From the circumstance of its being very common among the colliers in the neighbourhood of Edinburgh, the disease has been thoroughly studied here. In the first case that was recorded by Dr James Gregory, the black matter was analysed by Dr Christison, and shown by him to consist of pure carbon or lamp black, a result confirmed by every subsequent writer. The papers by Dr William Thomson, inserted in the XX. and XXI. Vols. of the Medico-Chirurgical Society of London, contain a mass of important information concerning this disease; and in a series of communications by the late Dr Mackellar, inserted in the "Monthly Journal" for 1845,

you will find numerous facts and opinions regarding it, the result of long and extensive practice among the colliers of Haddingtonshire.

On examining the lungs of those who die labouring under this disease, they are found to present a deep black colour, which is most intense towards their apices. On section, the pulmonary tissue yields on pressure a copious black inky juice, which stains all surrounding objects, attaches itself to the inequalities on the skin of the hands, and insinuates itself under the nails like fine black paint. At the apices of the lungs, the pulmonary tissue is generally more or less condensed, and the black matter thicker, and often of the consistence of putty. In advanced cases, ulcerations and cavities exist, exactly resembling in all their characters those of phthisis ; with the exception, that no tubercle can be discovered, and that the whole is of an intense black colour. Occasionally black indurated and gritty masses can be felt and dug out, which in some rare cases, present a slight crystallized structure like coal or crystallized carbon. The morbid anatomy of the disease will be best understood by carefully examining the preparations I now show you from the University Museum.

On placing some of the black fluid squeezed from the lung under a microscope, and examining it with a power of 250 diameters linear, it is seen to consist of a multitude of black molecules of extreme minuteness floating in a liquid. Here and there may be observed cells, similar to those previously described as existing in the sputum, more or less loaded with similar molecules of black matter, on which the strongest chemical re-agents produce no effect whatever.

When we endeavour to investigate the pathology of this disorder, we are met with a host of difficulties. Does it originate from the inhalation of carbonaceous particles floating in the atmosphere, or is it the result of a secretion of carbon in the pulmonary system itself?

The principal fact on which the first opinion rests, is the frequency of the disease in colliers, and in moulders of iron and copper. Yet it is curious, that whilst it is common among the workmen of some coal pits, it is unknown among those employed in others. For instance, it exists to a great extent among the colliers of Fife, Haddingtonshire, and Midlothian, but is unknown at Newcastle, Paisley, or Alloa, although the dust and powder is as finely levigated in one place as in another.

There is another curious fact connected with the disease in coal miners, viz., that those engaged in working at what is called the *coal* wall, and in sifting or transporting the coal, are little affected ; whilst those who work at the *stone* wall are peculiarly liable. This working at the stone wall, consists in blasting the rock by means of gunpowder. Hence the prevalence of black deposits in the lungs has been by some not so much attributed to the dust of coal, as to the smoke of the explosions, or of the lamps carried by the miners, confined as it is in narrow passages, where little ventilation exists.

Even this explanation has difficulties ; for it has been proved, that in some mines, where blasting is carried on very extensively, and where there is much coal dust, it does not exist at all ; and hence it has been supposed that there are some kinds of stone which disengage gases, or the working among which is much more deleterious than others. Thus Mr Philp of Aberdour, who has had great experience in this disease, in a communication he made to Dr Thomson, says, "In working at stone work, that is, in sinking pits and driving mines of communication, the workmen are exposed, in an imminent degree, to the influence of the impure air ; for, besides working in a confined space, and in a *cul-de-sac*, where the ventilation is very imperfect, there is also a considerable exudation of the carbonic acid gas from the fresh cut surfaces of the minerals. In this impure air they continue to work for many hours daily, for some months, their operations being frequently carried on several yards in advance of where their lamps will burn." In another place, he says, "Those pits and mines which have been noted for the impurity of the air, have given origin to the greatest number of cases."

Again, Mr Steele of Craighall says, he considers the disease to be caused by running mines in stone, and working in impure air. He says again,—"A principal reason is, that the stone contains some poisonous matter, which is probably of a metallic nature, as the workmen complain of it exciting a styptic and metallic taste in the mouth. A mine was carried across the strata in the Niddry estate, the finishing of which required a number of years. Six or eight of the miners employed in it died; several were obliged to leave it, and only one of those who commenced it was able to work in it throughout, and lived to see it completed. There was a particular stone in this mine, which was repeatedly met with, and to which the miners gave the name of arsenic, which was found highly pestiferous. Its exact nature I am not acquainted with. In a stone mine, run some years ago in the Newbattle field, a great many men died,—the average length of time each of the miners employed in it lived being about two years. The mortality was ascribed to the nature of the stone."

The facts connected with this peculiar black colouring of the lung, leave it therefore undecided whether it be owing to the inhalation of coal dust, the smoke of gunpowder and oil lamps, or carbonic acid gas, entering the lung. Is the powder deposited directly and primarily in the minute tissue of the organ, or is the carbon deposited secondarily, and by a process of secretion, from the blood?

Before we can answer these questions, there are two other series of cases, which it is necessary to take into consideration. 1*st*, Persons have been known to have the black spit, and black infiltrated appearance of the lungs, with caverns, without ever having been in a coal mine, or exposed in any way to gunpowder smoke. Several such cases are given in the paper of Dr W. Thomson. One of these is by Dr Browne, now superintendent of the Dumfries Lunatic Asylum; another, by the late Dr Moir of Musselburgh. The subject of this last case was never exposed to noxious vapours, unless it might be *carbonic acid*, —as he was the proprietor of an extensive brewery, and in the daily habit of inspecting the tun-rooms, the large vats in which overflow with that deleterious gas. Another case was observed by the same gentleman, in the person of a coachman, where, after death, the lungs were found to contain collections of an inky or tarry fluid, which stained the fingers, as if with soot or charcoal. A fourth and similar case, in a woman, is related by Dr Veitch. In none of these cases were the individuals exposed to the fumes of smoke, or coal dust. I have myself seen several such cases; in one especially, a cyst in the bronchial gland existed, about the size of the human eye-ball, filled with a black inky fluid.

2d. M. Guillot of Paris has brought forward another series of cases in old persons.[1] He is physician to the hospital for aged people, and says there is scarcely an individual of advanced life who does not present the carbonaceous deposit in the lung, generally most marked at the apex. The carbon often exists in crude masses, obliterating the vessels and bronchial tubes. I have also seen many cases of this kind, and there can be no doubt that the longer we live, the more carbon is deposited in the lungs. In infants, as is well known, these organs are of a light pink colour, and they become more gray and of a deep blue or black tint as life advances.

What are we to conclude from these cases? The black matter in all of them, when chemically examined, is undoubtedly free carbon, and is distinguished from the similar occasional results of chronic pneumonia, and changes of the blood by the action of chemical agents. Dr Pearson[2] thinks that we are always insensibly breathing an air more or less loaded with smoke, the results of combustion, and other impurities; and that this of course accumulates the older we get. If this opinion were true, it should be more common in persons who inhabit densely inhabited and manufacturing towns, as London, Liverpool,

---

[1] Archives Générales de Médecine, vol. 67. [2] Philosophical Transactions, 1813.

Manchester, or Glasgow. Yet we do not find this to be the case. The air in Paris is remarkably pure, the fires of manufactories are few, and yet there, according to Guillot, it is common.

On the other hand, should we conclude that it is formed internally by a chemical process, in all cases? If so, must we ascribe it rather to the inhalation of carbonic acid gas, than to carbon in a minutely divided state, inspired from without? In the present state of our knowledge, we cannot answer these questions with any exactitude.

Another point of great interest is the apparent antagonism of carbonaceous deposits with tubercle.

We frequently find the black lungs of colliers to contain small or large ulcerated cavities, with bands crossing them; but we rarely find tubercle. In one or two cases chronic tubercle has been found; but it does not appear that the caverns filled with black matter in the miner are owing to tubercle. Such is the analogy existing between these cavities, and the symptoms they occasion, however, that the disease has been named "black phthisis" by several persons. Some have supposed that tubercular phthisis is first set up, and that the carbonaceous deposit is then insinuated among the textures and ulcerations. By others, and especially by the late Dr Mackellar, it is maintained that there is never tubercle in these cases, and that ulceration is entirely owing to the accumulation of masses of carbon which are expectorated. I have carefully examined several lungs in various stages of the disease, but have never been able to satisfy myself of the existence of tubercle. The difficulty of the investigation, however, is very great, for it is probable that, did a tubercular exudation exist, it would be so impregnated with the carbonaceous matter, as not to be recognisable even under high powers. On the other hand, the bronchial tubes are frequently loaded with purulent matter of the usual appearance, but which, instead of presenting pus-corpuscles, is loaded with particles closely resembling those found in softened tubercle.

Guillot has alluded to the well known fact, that in old persons who present traces of tubercle, the cretaceous concretions are always surrounded by black matter. He thinks that the carbonaceous deposit compresses the capillaries which immediately surround the tubercle, prevents its further exudation, and so induces cure. But this is by no means invariable. Still it is an undoubted fact, that the black matter, like tubercle, always exists in greatest proportion at the apex, and the rarity of recent tubercle in combination with it, leads us to suppose that it exerts a peculiar influence on tubercular deposits. I have also seen black matter surrounding tubercle of the peritoneum, but in this situation it has not been free carbon, for long immersion in spirit has caused the disappearance of all colour.

So far as my observations have yet carried me, no difference is to be determined structurally between the pigment of melanosis and that of the disease under consideration. Some cells spit up by the miner, containing black granules, exactly resemble those in melanic cancer. On the other hand, there is a marked difference in the chemical constitution; as in the one, the black pigment, whatever be its nature, is fugitive, and removeable by chemical acids; in the other, fixed, and unchanged even under the blow-pipe. Hence all black deposits may be divided into two classes; to distinguish which we may call one class by the name it has so long borne, viz., "*melanotic*," and the other, from its chemical composition, "*carbonaceous*."

With regard to the treatment of individual cases of black phthisis, there is little to be said. The disease, once established, does not admit of cure by art; and it is a curious fact, that a man may work in a coal mine for many years without the black spit; but when it comes on, even should he leave the mine immediately, he will continue to expectorate carbonaceous matter ever afterwards. Of this the case before us furnishes us with an example, and the circumstance is one which furnishes a strong argument in favour of those who contend for the disease being dependent upon a secretion rather than a mechani-

cal deposition of carbon. A palliative treatment, guided by the nature of the symptoms and strength of the constitution, is all that is admissible. The only means of preventing the disease seems to consist in ventilating the mines where colliers work, or adopting means of carrying off the fumes to which the moulders of iron and copper are exposed. Such prophylactic measures are equally called for, whatever theory of the nature of the disorder shall ultimately be proved to be correct.

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

ON THE CHEMICAL CONDITION OF THE BLOOD IN VARIOUS DISEASES. BY PROFESSOR ANDREA COZZI.

The following are the principal results at which the author has arrived, from experiments recorded in the *Gazzetta Medica Toscana*.—[We have not seen the details.]

In inflammations which develope themselves in individuals long resident in marshy places, and affected with engorgements [*fisconie*] of the abdominal viscera, the fibrine is never increased; and it remains rather low in amount in inflammations of the liver and of the other viscera, when they are accompanied by an adynamic state, or a bilious condition and jaundice, as occurred during the continuance of that epidemic constitution under which in Tuscany cholera prevailed, and in the Florentine territory a certain frequency of nervoso-putrid fevers, often accompanied by petechial eruptions.

In pneumonic and pleuritic cases associated with a bilious condition, there was observed, sometimes in the clot and always in the serum, tinges of yellow, from the colouring matter of the bile, in rather considerable quantity.

In some pregnant women affected with pneumonia, either the fibrine or the colouring matter was observed in considerable proportion, and the albumen rather scanty,—not constantly, however. In other cases there was found to be a low proportion of the most of the protein compounds of the blood.

The clot of blood drawn in inflammations, when its lower, middle, and upper portions were examined, in cases where the buffy coat existed, yielded in the upper portion a larger proportion of fibrine, whilst in the other two portions the amount was not diminished; in the two lower portions, on the other hand, there was more colouring matter [hæmatosin], and in the upper portion either little or none at all.

In blood drawn in inflammatory fevers the fibrine did not run high, whilst the proportion of globules increased in these affections, and in fevers of a putrescent type.[1] The fibrine was increased when the fevers were complicated with inflammation or simple congestion of some of the viscera. The physical characters [of

[1] The author's phrase, "*diatese disolutiva*," is not easily expressed by any single English word. It means apparently a morbid tenuity or fluidity—a "dissolved state"—of the blood. We have used the old expression putrescent, as applied, on an erroneous theory, to such states of disease as purpura.

the blood] proved that intermittent fevers would be properly placed among diseases of a "*processo disolutivo.*"

In the blood of individuals in the Maremma, debilitated by endemic fevers or inflammations, who had considerable engorgement of the liver or spleen, there was found much cholestrine, in which also the bile of those who died of such fevers was very rich.

In rheumatic (?) tetanus [*tetano reumatico* in the original, probably a misprint for *traumatico*] the blood presented results identical with that drawn in inflammations of some intensity.

In lead colic, the lead combines with the albumen of the serum, in preference to the other materials of the body, forming with it a true chemical compound.

In hydrophobia, the blood of two bleedings, and also that collected after death, showed the presence of cyanide of sodium.

In albuminuria, when no complications existed, there were observed a notable diminution of albumen and hæmatosin, and a constant presence of urea; which last was also detected in the effusions into the ventricles of the brain, the pleura, the pericardium, and the abdomen. But when with slight degree of albuminuria there was associated eccentric hypertrophy of the heart, the albumen was but little diminished, the globules were lessened, and there was increase of the fibrine.

In the blood of a new-born child, there was shown to be a superabundance of the watery part, the colouring matter was scanty, the fibrine as if wanting, and the albumen exuberant.

Whatever be the modification which disease makes in the blood, it is observed that when there is a deficiency of one of its elements, the others, or one of the others, by increasing, compensate for the deficiency.

The dominant morbific constitution of the atmosphere seems to modify the blood in such a manner as to impede the further progress of its animalisation.—*Gazzetta Medica Italiana Federativa*, 9 Giugnio 1851.

ON THE PRODUCTION OF SUGAR IN THE LIVER OF MAN AND ANIMALS. BY CLAUDE BERNARD, PROFESSEUR SUPPLEANT AU COLLEGE DE FRANCE, &c., &c.

(*Translated and contributed by Dr W. R. Sanders.*)

Although it has long been known that, under certain conditions, sugar may be found in the blood and other animal fluids, yet, hitherto, the presence of saccharine matters has always been considered as accidental, and dependant exclusively on the nature of the food. In the present note, I shall demonstrate by the result of my experiments:—

1. That the presence of sugar in the animal organism is a constant fact, and is indispensable for the regular accomplishment of the phenomena of nutrition.

2. I shall prove that the presence of sugar in the animal body is not dependant on the kind of food, but that sugar is formed in the liver by a special function of that organ.

3. I shall, finally, point out the principal characters of the production of sugar in the liver, showing that it is in immediate dependance on the nervous system.

1st, Of the presence of sugar in the organism. *During the period of digestion, the blood which issues from the liver by the hepatic veins,*[1] *invariably contains sugar, both in man and animals, whatever the nature of their food may be.*

The liver in most animals, and particularly in mammifera, is placed intermediately between the abdominal and the general circulation, so that the blood of the ventral vena portæ, returning from the spleen and intestines, must pass through the tissue of the liver before arriving at the heart. Now, without attending for the present to the source of the sugar, I first establish the general facts:—1st, That it is by the hepatic veins, and them alone, that the sugar

[1] "Veines sus-hépatiques," in French.

is conveyed into the general circulation; and 2d, That, when the hepatic veins carry sugar, the tissue of the liver is also saturated with it in a high proportion. No other organ of the body is in the same condition, so that the constant presence of the saccharine principle is distinctive of the tissue of the liver during digestion. These facts have been proved by a very great number of direct experiments, and confirmed in a variety of animals belonging to nearly every order of the zoological series. Without entering on details, I shall enumerate the species on which my investigations have been made.

On man, in the state of health, I have three times had the opportunity of ascertaining the presence of sugar in the liver: first on the body of an executed criminal; next in an individual killed accidentally by a gun-shot; and, lastly, in a case of sudden death.

In the class mammalia:—*Quadrumana*—the cynocephalous ape. *Carnivora*—dog, cat, hedgehog, mole. *Rodentia*—squirrel, guinea-pig, rabbit, hare, black rat, and brown rat. *Ruminantia*—goat, sheep, ox. *Pachydermata*—horse, ass, pig.

Birds. *Rapaces*—hawk, owl, strix, and ulula. *Passeres*—swallow, sparrow, raven, lark. *Gallinaceæ*—pigeon, turkey, cock. *Grallæ*—snipe. *Palmipedes*—goose, duck.

Reptiles. *Chelonian*—land-tortoise. *Saurian*—green lizard, and grey lizard. *Ophidian*—anguis fragilis, coluber, and common viper. *Batrachian*—common frog, grenouille rousse, red frog, brown toad, aquatic salamander, or triton.

Fishes.—Osseous. *Acanthopterygian*—common perch, labrax, thunny. *Abdominal Malacopterygian*—common carp, barbel, leuciscus, common trout. *Subbranchial Malacopterygian*—cod, turbot. *Apodous Malacopterygian*—common eel, conger-eel.

Chondropterygian or Cartilaginous Fishes. *Sturiones*—sturgeon. *Selacian*—sea-dog.

Mollusca. *Gasteropodous—pulmonary*—the red slug, grey slug, common snail, fresh-water snail. *Pectinibranchiata*—paludina vivipara. *Acephalous M.* or *Lamellibranchiata*—the edible oyster, pecten, mussel, anodont, and unio.

Among the Articulata, I have detected sugar in the liver of some *decapadons crustacea*, as the crawfish and lobster; but in this branch of the animal kingdom the organs of nutrition undergo such profound modifications that the determination of the liver would lead to unnecessary discussion.

The numerous researches cited above have all been made on animals in full health, and during digestion, or shortly after it. They are, I think, sufficient to prove that the presence of sugar in the liver is a general fact, both in man, and in all animals evidently provided with that organ.

In proportion as the act of digestion draws to a close, the quantity of sugar poured by the hepatic veins into the general circulation gradually diminishes; and, at the same time, the tissue of the liver eliminates by degrees all the saccharine matter which it contained. But in warm-blooded animals which are in good health, and in the usual conditions of supply of food, there is never complete absence of sugar from the liver, because the digestion of another meal commences before the quantity of saccharine material already formed is exhausted. If, however, we subject animals to forced abstinence, the sugar after a time completely disappears, and the liver exhibits no more trace of it than any other organ of the body.

Accordingly, as it is a fact that in all animals during digestion the hepatic tissue, and the blood which issues from it, constantly contain sugar, so it is equally true inversely, that in all animals subjected to abstinence prolonged sufficiently, the liver and hepatic blood are entirely deprived of saccharine matter, which, however, immediately re-appears as soon as digestion and nutrition resume their activity.

The duration of abstinence required for the complete elimination of sugar from the liver presents many variations according to species, age, health, &c.

I shall merely state, that in birds the disappearance is very rapid, occurring at the end of two or three days; while, in dogs, it is complete only at the end of seven or eight days of starvation. In cold-blooded animals a much longer time is required.

We shall afterwards observe, that in cases where the function of digestion is disturbed or disordered, one of the first results is the disappearance of sugar from the liver and from the blood of the hepatic veins. Hence, the presence of saccharine matter there must be regarded in animals in their ordinary condition as the indication of the normal performance of digestion.

2d. *Sugar is produced in the liver independently of the nature of the food.*

The experiments above-cited might serve to show that the sugar is formed in animals without the intervention of saccharine or amylacious principles in the food, since the presence of sugar was detected in carnivora, as well as in omnivora and herbivora in the animal series. However, as the fact of the production of sugar in the liver is still new, and has most important bearings on physiology, I shall support the above proposition on the evidence of special and direct experiments.

To demonstrate that the saccharine matter originates in the liver, and is not introduced with the food, animals such as dogs, cats, or even rabbits, must be subjected to a diet exclusively animal, and containing no substance which can by the process of digestion give rise to saccharine principles in the alimentary canal. Thus, I have fed dogs during three, four, five, and even eight months exclusively on flesh, and on examination at the end of that period, I have constantly found that, while the intestines and blood of the vena portæ at its entrance into the liver, contained no sugar, the blood of the hepatic veins was always abundantly charged with it. But an experiment less prolonged proves the production of sugar in the liver. In fact, as before stated, the dog's liver may be completely deprived of sugar by an abstinence of seven or eight days' duration. If, at the end of that period, the animal be fed on flesh only, the sugar will nevertheless reappear in the liver as soon as the process of digestion determines increased activity in the circulation of the organ. When, therefore, in animals fed exclusively upon flesh, it is constantly ascertained that the blood brought to the liver by the vena portæ contains no sugar, and that the blood which leaves the organ by the hepatic veins is always charged with it, we must admit, that the blood acquires the saccharine principle in passing through the hepatic texture, or, in other words, that the liver is endowed with a peculiar function, in virtue of which sugar is produced.

The liver, therefore, performs two functions at the same time, namely, the secretion of bile and the production of sugar; and the latter function commences even before birth, for I have detected sugar in the liver of the young of mammalia and birds at different periods of fœtal life. It is remarkable that while the bile, like other intestinal secretions, is poured out into the alimentary canal, the sugar, on the contrary, mixed with the portal blood returned from the intestines and spleen, is carried out into the general circulation, and disappears in contributing to the phenomena of nutrition. This separation of the bile and sugar, however, occurs only in vertebrata; for in molusca, I have found the biliary fluid highly charged with saccharine matter.

The sugar produced in the liver, presents the chemical characters of glucose. Along with M. Barreswill, I have ascertained the following properties:—

1st, The saccharine principle of the liver ferments when put in contact with yeast, and yields alcohol and carbonic acid.

2d, Its solution is rendered brown by the caustic alkalies, and reduces the tartrate of copper dissolved in potass.[1]

I ought to add, that the hepatic sugar undergoes spontaneous destruction in

---

[1] The latter test is the one commonly used by M. Bernard to detect the presence of sugar.—W. S.

contact with blood and animal textures, much more rapidly than ordinary glucose,—a circumstance indicating that to operate in favourable conditions, the search for sugar in the liver should be made on animals recently dead.

3d, Influence of the nervous system on the formation of sugar in the liver.

The formation of sugar in the liver is a function placed under the immediate influence of the nervous system.

In vertebrata, the liver receives two kinds of nerves supplied from the pneumogastric and the solar plexus. In this, as in other functions, it is difficult to determine the kind of participation which the nervous system has in the chemical acts of nutrition. It is, however, incontestible that some of the phenomena of nutrition cannot be produced external to the living individual, and are connected in an immediate manner with the integrity of the nervous system, so that we can extinguish, exalt, or disturb these chemical phenomena simply by modifying the nervous organs which influence them. In particular, those functions, generally periodical, designated in physiology as secretions, are so placed; and I shall show that the production of sugar in the liver belongs to the same category.

Thus, for example, whatever be the kind of food, we can cause the complete disappearance, in a few hours, of the saccharine matter of the liver in dogs or rabbits by the section of the pneumogastric nerves in the middle region of the neck. The same result occurs whenever, by any means, a violent commotion of the nervous system is produced.

In the whole extent of the nervous system, I have found only one limited spot of the medulla oblongata, where a lesion occasions the opposite effect. Thus, when in dogs or rabbits, we succeed in pricking the medulla oblongata with a sharp-pointed instrument within the narrow space, limited below by the origin of the pneumogastric, and above by the emergence of the acoustic nerves, we ascertain, after a short lapse of time, that the saccharine principle has been formed so abundantly as to spread throughout all the organism. The blood and other liquids of the body are surcharged with it; the urine eliminates the excess; the animal is diabetic.

Thus we can cause the excess or disappearance of sugar solely by modifying the phenomena of innervation.

In conclusion, from the results presented in this paper, I think I have proved beyond doubt the production of sugar in the liver of man and animals, and have established the existence of a function which, up to the present time, had remained entirely unknown.

SURGERY.

INCOMPLETE ANCHYLOSIS OF THE ELBOW-JOINT. REPORTED BY M. DUCLOS.

Louis Daincourt, a joiner, aged 24, of good constitution, fell under M. Malgaigne's care on the 6th April 1851. Two years before he had fallen from the second storey of a house to the ground; but as he at the same time became deprived of consciousness, he can give no precise information as to the manner of his fall. However, on returning to his senses, he felt very sharp pain in the left elbow, which immediately became enormously swollen; he in consequence was admitted to the hospital of Amiens. For the first eight or ten days the joint continued to swell, and was so intensely painful that he had not an instant's rest. Antiphlogistic treatment was employed: cataplasms, fomentations, repose; and, finally, after two or three months of hospital treatment, the elbow returned perfectly to its normal dimensions; but there remained considerable articular stiffness, and the joint was in a state of complete extension, which neither patient nor surgeon could try to remove without inducing most excruciating torture. He was, consequently, kept in the hospital, the joint was covered with cataplasms, and baths

of various kinds were employed. At last, after two years of this treatment, M. Josse determined to send him to the Hôpital St Louis, to try if the *vapour douche* would have more influence on the stiff joint.

On admission there, the left elbow was neither swollen nor painful, it preserved its natural form, but was fixed completely in the extended position, and there was such absolute inability to bend it even in the slightest degree, that the limb hung from the body as stiff as a stick, causing considerable annoyance to the patient, and preventing him from following his occupation as a joiner.

Had we here to do with a true anchylosis, in which case the joint was irrevocably stiff? or was it a false anchylosis, in which case, although the re-establishment of the motion of the elbow could hardly be expected when the remote period from which the injury dated was considered, there might yet be hopes of bringing the arm into a state of semi-flexion? M. Malgaigne, in determining this point in diagnosis, availed himself of a sign which he regards as pathognomonic,—if, in endeavouring to move the joint, the surgeon causes pain, even without producing apparent motion, *there is no osseous union*, and there are hopes of changing the position of the parts without fracture, or other disagreeable consequence. This was the case with our patient; bending the elbow across the knee produced acute pain; the use of forced flexion was, therefore, postponed till the morrow.

The patient being placed in a reclining posture, and under the influence of chloroform, M. Malgaigne, standing at his left side, seized the affected arm with both his hands, a little above the wrist, while an assistant kept the arm motionless; the surgeon then bent the fore-arm slowly, and without jerk, till it formed with the arm an angle slightly exceeding 90 degrees. This manœuvre required considerable force, and was completed without crepitation or fracture; the patient gave no sign of pain. The forced flexion was maintained, first, by a triangular handkerchief, fixing the elbow to the trunk, and then by a bandage attached by a running knot to the wrist and tied behind the neck, so as to keep the hand in front of, and on a level with, the right shoulder. The patient on awaking manifested excessive joy at seeing his elbow bent. Emollient cataplasms frequently renewed were applied to the part.

Throughout the day there was felt some pain about the elbow, but next day this had ceased. The bandage was kept applied till the 21st of April; at this time the patient felt no pain; the joint was bent to a right angle, but was incapable of the slightest spontaneous movement. Without expecting much improvement in this respect, M. Malgaigne urged the patient to exercise the joint, and, in fact, some slight movements of flexion and extension were regained, and excited hopes of further progress, when the want of beds and arrival of some severe surgical cases occasioned his dismissal. He has not since been heard of.

Note by M. Malgaigne.—When I admitted this patient into my wards, I certainly had not in view the restoration of the motion of his elbow; not that I regard such restoration as impossible; but when articular rigidity has existed for two years, in order to overcome it assiduous attention is necessary, and a period of treatment far exceeding in duration what an hospital can afford. But an immense service could be rendered to this youth, by restoring to him the use of the arm and hand, by bringing the member to a state of flexion. It is impossible to imagine a result obtained more rapidly. Thanks to chloroform, there was no pain in the first instance, and I am inclined to believe that employment of this agent was not without its influence upon the almost absolute absence of subsequent unpleasant symptoms.—*Revue Médico-Chirurgicale de Paris*, June 1851, p. 371.

LITHIASIS IN RUSSIA.

At a recent meeting of the Swedish Medical Society, Dr Grähs communicated the following notice regarding the prevalence of lithiasis in Russia.

"Lithiasis, and especially stone in the bladder, is a very common disease in Russia, and particularly in Moscow. It is not there, as with us, chiefly an affection which prevails among those advanced in years, and of the so-called better

classes, or among children of the lower ranks, but it occurs in every station, and at all ages, especially among the middle classes, the peasants, and the commonalty. Of the causes which conduce to this prevalence, much has been both said and written. Many believe that the affection arises from the quantity of quass which is consumed. (Quass is a liquor which, in its properties, has some resemblance to French cider, and is prepared from barley, black bread, spices, &c., being used as generally as our small-beer.) In all probability, however, the cause is to be looked for in the qualities of the water, and in peculiarities of climate.

"The most ordinary operative treatment is by lithotomy. In former times this operation was practised in Russia, as in other countries, by persons who rendered it an exclusive occupation, and whose art descended as a heritage in certain families, among which that of Benedictoff was particularly distinguished. At a more recent period, especially since the commencement of the present century, the duty was assumed by the regular faculty, and the method which was almost exclusively adopted, and is still employed by them, was the lateral section after the manner of Frère Come, with the *lithotome caché*. Amongst the surgeons now living in Moscow, who have performed the greatest number of operations of lithotomy, are Alphonsky, of the Foundling Hospital, who has had the good fortune to have operated forty-seven times in a single year, with the death of only a single patient, and Professor Pohl, of the Catherine's Hospital, who has operated on not fewer than 1100 occasions. Assow, a pupil of Alphonsky, published in 1841 a dissertation on this subject, in which he has collated 3000 operations for the stone, performed since 1807, partly in Moscow and partly in Smolensk. Of these there occurred (from 1808 to 1837) 1471 in the Mary's Hospital at Moscow alone, regarding which a separate report has been published in Russian, which the reporter now presents to the society."—*Förhandlingar vid Svenska Läkare-Sällskapets Sammankomster*, 1849–1850, published in *Hygiea, Med. och Pharm. Maanads-skrift*, May 1851.

LIGATURE OF LEFT SUBCLAVIAN ARTERY. BY MR CRAWFORD.

In the month of February last, Mr Fendal, a midshipman of the H.E.I.C.'s brigantine *Tigris*, then stationed at Bassadore, in the Persian Gulf, sustained a frightful laceration of the left shoulder, by the accidental explosion of his gun, while he was in the act of drawing it towards him by the muzzle. It unfortunately happened at the time that there was no surgeon attached to the vessel; however, the hospital assistant succeeded, by application of tourniquets and bandages, in stopping the flow of blood. Mr Crawford, the assistant surgeon in charge of the steam-frigate *Auckland*, was immediately sent for, from the Arabian coast. . . . For the first fortnight matters were progressing favourably, and his exertions were encouraged by the improving state of the patient, when unhappily secondary hæmorrhage of the most violent character set in. The first bleeding took place about two o'clock in the morning, but luckily Mr Crawford slept in the house, and being upon the spot, adopted such measures as soon stopped the flow of blood. In a few days the patient was again going on well, when exactly one week from the first bleeding, a second, much more profuse, and threatening death in a few seconds, occurred at midnight. The gush of blood was temporarily arrested, but Mr Crawford found that it proceeded from an injury to the axillary artery near the collar bone, and that the only hope of preserving the life of the patient was to perform the operation of tying the subclavian artery as it runs under that bone. Although without assistance, Mr Crawford proceeded at once to the operation, and succeeded in a few minutes in achieving his object. By the administration of suitable restoratives, and a steady course of judicious treatment, the patient gradually revived, and everything connected with the operation succeeded so favourably, that after the expiration of five or six weeks his health and strength were almost re-established. Shortly after this accident the *Tigris* was ordered upon some pressing duty, so

that Mr Crawford was left in charge of his patient, without the society of any other European at the station, and during this interval he was forced to undertake another rather severe operation upon the bones of the wounded arm, by which he hoped to preserve that member; and now not only is the sufferer restored to strength again, but the wound is nearly well, and there is much hope that the arm will still be a very useful one. We believe that this is the first case of the kind that has happened in India; and when we consider that the operation was performed at midnight, the patient in a most inconvenient position, upon a bed from which he dared not be moved, and without the aid or advice of a professional brother, we can have little hesitation in pronouncing the result to be most creditable to the operator.—*Abridged from Bombay Journal, and London Med. Gazette.*

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

HERNIA OF THE OVARY MISTAKEN FOR AN ENCYSTED TUMOUR—EXTIRPATION—DEATH. BY M. EVERSANT.

A girl, eleven years of age, came into the hospital to be under the care of M. Eversant, having a tumour in the left labium. Her parents had observed it when the girl was a year old, but it had never caused any trouble till eighteen months previously, when it began to be painful, and made the patient walk lame.

The tumour was very moveable, more moveable than a cyst. It was easily pushed from the inguinal ring to the lowest part of the labium—like a testicle in the scrotum. It was of the size of a small nut, painful on pressure, and in one point fluctuation could be perceived.

It was diagnosed to be an encysted tumour, and it was resolved to extirpate it. An incision was made in the centre of the labium, and the dissection carefully continued layer by layer. A cord similar to the spermatic cord, and containing part of the fallopian tube, was arrived at and tied. It was then cut across below the ligature, and the ovary removed along with the cord attached.

The girl shortly took peritonitis, and died on the third day after the operation. —*Gazette des Hôpitaux*, 24th May 1851.

RUPTURE OF THE UTERUS TWICE IN THE SAME SUBJECT—THE LAST FATAL. BY DR HARTT.

Dr H. was sent for in May 1846, to a female who had been in labour three days. She was exhausted, with a pulse of 108. On examination, he found the shoulder presented; that there was a laceration of the neck of the uterus, through which the head, and the head only, of the child had passed. He immediately seized the feet, and delivered with little difficulty. He gave the patient an anodyne, and waited beside her four hours, expecting that she would die. At the end of that time, however, she seemed to be better, and he began to cherish some faint hopes of her recovery. She did recover; and about eighteen months afterwards she was delivered of a living child upon a raft in the river, without assistance of any kind.

On the 13th October 1849, he received a summons to visit her again on a similar occasion. When he arrived she had been in labour four days. She was very weak; pulse 118 and feeble; breathing very hurried. The account given by the attendant was, that the hand presented, and her pains were severe, and that twenty-four hours after they began, a snap was distinctly heard by her friends around the couch; that the hand receded, and from that moment the labour had been suspended. Suspecting the true state of the case, Dr H. instantly examined, and found a large opening precisely in the seat of the former rupture,

and passing on the hand, felt the child in the cavity of the abdomen. The patient was so far exhausted that he was unwilling to interfere. He described both to herself and her friends her alarming position, and stated that, owing to the length of time which had elapsed since the accident occurred, he feared that no operation could prove successful. They all, however, expressed great anxiety that an effort should be made, and at their repeated and earnest solicitations, he proceeded to deliver her *via naturale*. The operation was performed with great gentleness, and particular care was taken to avoid unnecessary injury to the bowels. He remained with the patient several hours, administering anodynes and restoratives, but in vain. She gradually sunk, and died thirty-six hours after delivery. The interesting feature in the case of this patient was, her complete recovery from the first rupture, and the perfect union of the parts, a union sufficient to sustain the violence of the unassisted delivery of a large and vigorous child.—*New York Journal of Medicine*, Nov. 1850, and *Brit. Amer. Med. Journal*, April 1851.

CONGENITAL VARIOLA IN TWINS. BY JAMES AYER, M.D., BOSTON.

I was called in haste to Mrs P., and found her lying on the bed, in great pain. On examination, the head of a small fœtus was found born. The uterine contractions were active, and full delivery was effected in a moment, attended by a feeble cry. The pains continued, a bag of fluid was felt protruding, and soon a second fœtus was expelled dead. Two separate placentæ were afterwards removed, and the patient made comfortable.

The infants were found of the size and development of six months. The living one had a dozen or more of pustules on the face, head, and breast; one or two were noticed on the abdomen, but none on the limbs. Three or four were good-sized, plump and well-defined pustules of small-pox. The remainder were not so full, but evidently of the same character. This one survived its birth two hours.

The dead child had no offensive odour; the abdomen was dark purple, and the cuticle quite loose. Its whole body, especially the abdomen, was marked with depressions, similar to those of variola in infants, after death. No elevations or pustules were noticed; these marks only remained.

Three weeks before the abortion, the mother, I was informed, had broken out with varioloid, after the usual premonitory symptoms, and had just recovered when I saw her. The disease was so mild that a physician was not called. She could not trace her miscarriage to any over-exertion, or any cause, except the attack of varioloid. Whether the mother infected the two at the same period, and the death of one caused the expulsion of both; or one had the disease first, and the second received it from him, are questions of some interest, but difficult, from the evidence, to decide.—*Boston Med. and Surg. Jour., and Brit. Amer. Med and Phys. Journal.*

FATAL INVERSION OF THE UTERUS. BY DR E. SMITH.

Dr E. Smith related that he had been called at 11 P.M., May 5th, to a patient who had been attended in her second confinement by a midwife; she was thirty-five years of age, of a healthy but not robust habit of body.

A tumour ten inches in diameter was found to protrude from between the labia. In this tumour, which was an inverted uterus, the placenta was still found adherent. There had been no extraordinary hemorrhage.

The pulse could only be discovered at long intervals; however, consciousness remained perfect. Assisted by Mr Lucas, Dr E. Smith removed the placenta, and then returned the uterus to its normal site, without involving the loss of much blood. Brandy was administered to the extent of half a pint in the course of three hours, by half ounce doses; but nevertheless, about 3 A.M., the patient died, without any prominent symptom.

A post-mortem examination proved the body to be perfectly healthy in its vital

organs. The vascular system was bloodless; the cavities of the heart being empty.

The uterus was found in its proper position; the cavity measured five inches in the transverse, and eight inches in the longitudinal direction. The substance of the uterus was flabby, and the thickness of the walls at the fundus half an inch.

Dr Smith believed that the patient had died from the loss of blood, and from the shock induced from the violent inversion of the uterus. There was no direct evidence to prove that the midwife had used improper force in endeavouring to remove the placenta in the first instance.—*Med. Gazette*, June 6, 1851.

CASE OF INVERSION OF THE VAGINA COMING ON DURING LABOUR. BY DR LAMBERT.

The patient was a labouring woman. She had had two children in easy confinements. During the last six months of her third pregnancy she suffered from prolapsus of the vagina whenever she was working at out-of-door labour. The swelling thus produced attracted her attention, but did not alarm her, as it disappeared when she lay down in bed.

When labour came on, the tumour again appeared between the limbs. The midwife in attendance finding the labour tedious, and ignorant of the nature of the case, recommended the woman to make the most of her pains, and ordered her a vapour bath. The only apparent effect of this advice was the increase of the tumour to double its former size. When Dr Lambert arrived, he found it projecting from the vulva of the size of the two fists, of a bluish-red colour, round, wrinkled, and of considerable consistence. At its lower extremity was an opening though which the finger could be introduced to the os uteri.

Dr L. recommended rest in the horizontal posture, with the pelvis a little elevated, cold applications to be made to the tumour, and slight pressure applied to replace it during the intervals between the pains. After considerable delay the tumour was reduced, and the woman delivered with the forceps. She made a good recovery, and the swelling has never returned.—*Revue Med. Chir.*, April 1851.

RETRO-UTERINE SANGUINEOUS TUMOURS.

The following is the substance of M. Huguier's speech at the conclusion of an interesting discussion on this subject in the Surgical Society of Paris:—

M. Huguier observed, that these extravasations are not so entirely new to science as might be thought. An example was to be found in the writings of Ruysch, who considered it to be a deviation of menstruation. Another instance, with a similar explanation, was to be found in the "*Recueil d'observation des erudits de Leipsick for* 1693." Many other collections of cases mentioned retention of blood in the uterus, tubes, and peritoneal sac. Recently, Denœux, in his excellent monograph, "*Sur les troubles de la vulve*," cited cases which, according to M. Huguier, should be regarded as tumours of the same character as those under discussion. Lastly, observations of the same kind are to be found in the writings of Dugès, Boivin, Bernous, Piegdaniel, Beau, Nelaton, Pioge, and Vignes. All the facts known with regard to these tumours permit of their being divided into two classes,—the *first*, those in which the effusion takes place in the cellular tissue below the peritoneal *cul-de-sac*, between the rectum and uterus, whence it may spread into the broad ligaments, and arrive at the fallopian tubes, remaining all the time extra-peritoneal. These tumours are owing to venous fulness of the parts; to periodical menstrual congestion; to venereal excesses; to frequent pregnancies; and to varicose vessels. Their exciting causes are violent efforts, as in labour and coitus.

The *second* class includes two species, which M. Huguier distinguishes according to their etiology. The first, which he names a false peri-uterine hæmatocele, is caused by intra-uterine pregnancy, in which the ovum continues to be developed for six weeks or two months, and then dies: there results a cyst, which goes on

enlarging, and presents the characters of a true hæmatocele. M. Huguier related the particulars of two cases of tumours situated behind the uterus, in the midst of the contents of which he had met with the debris of the fœtus.

The second species, which M. Huguier termed true hæmatocele, is most frequently formed by retention of the menstrual blood in the cavity of the uterus; the blood having distended the uterus, escapes by the tubes into the peritoneum, being lodged in the retro-uterine peritoneal *cul-de-sac*. This species presents several varieties. The tumour may be formed of the distended tubes lodged behind the uterus. It may be caused by an accumulation of blood in the peritoneal sac from rupture of an ovarian vein. Lastly, it may depend upon sanguineous exudation.

The diagnosis of peri-uterine tumours M. Huguier stated to be comparatively easy. Thus: extra-peritoneal tumours are situated low down; they rapidly acquire a large size; the tumours of the *cul-de-sac* are formed more slowly; they excite partial peritonitis, from which result adhesions: the extra-peritoneal tumour is more resistant and harder, and does not exhibit the violet-colour pointed out by M. Nelaton as belonging to those of the tumours of the *cul-de-sac*.

In both forms the uterus is displaced and pushed towards the pubis; in the intra-peritoneal the uterus is at the same time raised.

The course of the extra-peritoneal tumour is gradual and steadily progressive; that of the other form is irregular, and increases at the menstrual periods. When death takes place, in the former, phlegmonous inflammation is found; in the latter species, peritonitis is constantly met with.—*Med. Gaz.*, June 13, 1851, and *Union Médicale*, May 31, 1851.

DOUBLE OVARIAN DROPSY CURED BY OPERATION. DR PEASLEE.

The subject of this operation was a young lady, æt. 25. Dr P. proceeded in the usual way to remove the tumour by first carefully dissecting through the abdominal parietes. On arriving at the fascia transversalis, it proved to be a very thin layer, and some doubts were entertained as to the dense white membrane which presented itself after its division. Careful dissection was therefore continued, and the membrane was found to be the sac. In the course of this last dissection, a vein was opened, and 10 ounces of blood lost. Not above an ounce was lost in dividing the abdominal parietes. The vein in the wall of the sac was tied, and the operation continued.

At this conjuncture, fifteen minutes were consumed in guarding against the effects of violent vomiting which came on. After the retching had ceased, the hand was introduced and the sac found to present no adhesions of any moment. The contents of the sac, amounting to twenty-two pounds of fluid, were now evacuated into a bucket. The pedicle was now reached, tied, and divided.

On making a careful examination of all the parts and organs brought into view, preparatory to closing the incision, a sac of the size of a pullet's egg was discovered on the right ovary, and the whole organ was diseased. Accordingly, a double ligature was passed through the broad ligament, and the ovary removed. The incision was now carefully closed, and the ligatures drawn out through the wound at the nearest point.

The woman made a good recovery. The catamenial discharge appeared seventy-two hours after both ovaries had been removed, and lasted for three days.—*Amer. Jour. Med. Science*, April 1851.

QUININE AS A PREVENTIVE IN PUERPERAL FEVER.

Dr M. Retzius, in reporting that the General Lying-in Institution at Stockholm had been temporarily closed, owing to the prevalence of puerperal fever, stated that the disease had begun in the month of September, and that in November and December it had somewhat abated, but that in spring it had again in-

creased, and become so virulent, that it was necessary to apply to the authorities to have the wards closed for a couple of months to secure their disinfection. Of the patients who had been admitted since the beginning of April, nearly all had suffered from the contagion. The mortality, which was at first frightful, although less severe than in many foreign institutions, amounted to 33 per cent. Latterly, however, it had somewhat subsided; an advantage which Dr R. was inclined to ascribe to the prophylactic measure which had been adopted, of administering daily to each, from as early a period as possible, a scruple of the sulphate of quinine.

[Some consideration should be attached here to that which seems nearly a universal rule in all epidemics, that the earliest cases exhibit always the greatest amount of virulence and fatality.—TR.]—*Hygiea, Medicinsk och Pharm. Maanads-skrift*, May 1851.

SCARLATINAL VAGINITIS. BY DR CORMACK.

"During the last epidemic (1848-9) of scarlatina," says Dr Cormack, "I had, under favourable circumstances, considerable, though not a very large, experience of the disease; and of all my cases I have preserved a record. Vaginitis I frequently met with, and I regarded it in no way as an unexpected or rare occurrence; but, on the contrary, as a not unlooked-for extension of the exanthematous inflammation of the skin, analogous in its nature to what is often met with in the mucous linings of the nose, ear, air passages, and intestinal canal. In the epidemic of 1848-9 I had under my charge twenty-three female patients, all of whom were cleanly, well nursed, and in a respectable social position. In twelve of the whole number there was well-marked vaginitis; and so impressed was I with the importance of averting or preventing this affection, that in every female patient I directed, from the very first, careful ablutions of the parts to be performed at least twice in twenty-four hours. Of the twenty-three female patients, two only were above 14 years, and these were respectively 26 and 28, and both married. Now, both of these patients had *acute* vaginitis, much more severe than any of the children. In one lady, for forty-eight hours the discharge was so abundant as to require the nurse to change the towels at least every hour, and it was of so acrid a nature as to excoriate the thighs and anus, notwithstanding every precaution being taken to protect these parts. The other had it more mildly; but the vaginitis was also in her a source of great suffering and discomfort. The first lady aborted: the second was not in the family way. The first was the most dreadful case of scarlatina which I have ever seen issue in perfect recovery: the second was one of moderate severity.

"Speaking, as I now do, from a very limited number of facts, I have no right to give an absolute opinion as to the frequency or rarity of scarlatinal vaginitis; but I would venture to suggest that the non-observance of this affection by the practitioner is no proof of its absence; for patients suffering from scarlet fever are often too ill to make complaints; and in other cases the affection is managed by the nurse, without her thinking it necessary to trouble the doctor. The question must be decided by future experience of a large number of cases, carefully observed with a special view to its elucidation.

"The whole treatment required is generally frequent ablution with tepid water, and, if pain be complained of, the use of opiate fomentations. The children, with one exception, required very little special treatment; but one of strumous habit, continued to have a profuse muco-purulent discharge long after convalescence was in all other respects complete. Chalybeate medicines, cod-liver oil, and astringent lotions, at last effected a cure. The two adults were both treated by injections of nitrate of silver, and the keeping apart the labia by a piece of lint soaked in the same. I believe in severe cases the nitrate of silver will be found the best local remedy; but the injury which it occasions to the linen of the patient, and of her bed, naturally occasions a prejudice against its use."—*Medical Gazette*, August 2, 1850.

DELIVERY OF FIVE LIVING CHILDREN. BY DR SERLO.

The following rare fact is reported by Dr Serlo, in No. 50 of the Medizinische Zeitung (for December 1850).

A woman, 34 years of age, who had already been safely delivered five times, became pregnant for the sixth time. The first period of pregnancy passed well; but in the latter period weakness, agitation, and sleeplessness supervened. The abdomen acquired an extreme development, and in the last months so much so, that the woman could no longer walk, and was obliged to keep her bed during the last seven weeks. Dr Serlo visited this woman on the approach of her confinement, and found her abdomen enormously distended in every sense, very much raised, hard, and strongly projecting on the right side. She could only feel very feeble fœtal movements, and was very weak; the pulse was small and quick, the legs and thighs œdematous. On examination, the os was found to be half-way opened; the membranes flabby, but no part of the child could be felt.

Labour came on about thirty hours after this examination. The pains, at first very quick, caused a rupture of the membranes; a little of the liquor flowed off, and then the contractions completely ceased. The head of the child presented itself lower in the pelvis; on applying the forceps, they easily brought down an infant to the outlet, which did not commence crying till the cord, which was rolled around its neck, was cut. When M. Serlo wished to proceed to complete the delivery, he found, on introducing his hand into the vagina, a second bag, and as there were no contractions, he ruptured it, and easily extracted a second infant, weaker than the first. In the same manner he found a bag which contained a third infant, almost as strong as the first. This was extracted by the feet; it was very lively. After M. Serlo had cut the cord and removed the placenta, a fourth bag presented itself, which was open, and contained a child weaker than the third, and which was extracted by the feet. A fifth presented itself, and was extracted in the same manner.

After this last expulsion, the womb was found to be empty and very distended; it did not contract till the end of five or six minutes.

The five children, living and crying, were put in a bath, measured and weighed:

The length of the first was	15	in. (Fr.),	it weighed	$3\frac{1}{2}$	lbs.
„	„	second	„	$12\frac{1}{2}$	„	„	„	$2\frac{1}{4}$	„
„	„	third	„	13	„	„	„	$3\frac{1}{8}$	„
„	„	fourth	„	14	„	„	„	3	„
„	„	fifth	„	11	„	„	„	3	„

At the end of three hours, the second infant died: the fourth survived it twelve hours: the third seventeen hours: and the fifth twenty-five hours. The first, which had been extracted by the forceps, lived nine days.—*L'Union Médicale, March* 1851

MATERIA MEDICA.

METHOD OF COATING RETORTS, MATRASSES, CAPSULES, ETC. BY M. MOHR.

Distillation by naked heat, such as by a charcoal furnace, offers so many advantages over the water bath or sand bath, that in order to enable chemists to have recourse more frequently and more securely to the former mode of heating, M. Mohr applied himself to the discovery of a solid coating, capable of protecting all sorts of fragile vessels which it may be wished to expose to the action of fire. Hitherto chemists have been constantly exposed to the risk of serious losses by the breaking of such vessels. The plan of covering with a layer of copper by galvanism is too troublesome and expensive. All the coatings and lutes hitherto in use are easily detached from glass, and none of them can bear washing with water. The author has succeeded in obtaining a coating which does not possess

any of those inconveniences; its application does not involve the least difficulty, it adheres strongly to glass and porcelain, and may be perfectly well washed with water.

Take equal parts of ordinary brick and litharge in very fine powder, and, after thorough mixture, add enough of linseed oil to bring it by triturition to the consistence of a thick and tenacious paste. Apply it to the retort by means of a rather stiff brush, and then with a sieve dust it over abundantly with coarse sand. It hardens rapidly when put into a drying stove, so that it becomes difficult to detach it from the glass even with a knife. The glass, thus protected against the too intense action of fire, runs no risk from resting too long on an iron triangle, or from contact with the flame of the furnace.

The same material, omitting the sand, makes an excellent cement for repairing mortars of porcelain, serpentine, or similar materials. The brick is reduced to a very fine powder, and there is substituted for the litharge white lead slightly calcined, and thus deprived of its carbonic acid. After having added to these the linseed oil, they are mixed by strong triturition, so as to form a paste as smooth to the touch as possible. It is applied quite liquid by the finger to the two faces of the fracture, then the two pieces are pressed strongly together, and the mortar left at rest for several days. It ought to be previously ascertained if the two fragments which are to be luted, will stand in this position without being tied or supported. After four or five days the mortar is put in a warm place, and the cement allowed to harden completely. Large porcelain mortars, which, after having been joined by various cements, speedily gave way again, with this luting unite perfectly, and answer for their usual purposes. They sound as if they had never been broken. The closer the application of the broken surfaces, and the thinner the layer of luting, the stronger is the union. In luting household porcelain articles, white lead, gypsum, or chalk, may be substituted for the brick dust, so that the cement is white, and the fractures are scarcely perceptible.

Another and cheaper coating than the above is made by slaking lime with water, so as to make a soft paste, and adding about an equal volume of white bole [*lithomarge*, the so-called French chalk used by tailors], and bringing it with water to the consistence of a thick paste. It is applied like the other with a brush, and when one coat is dry another can be laid over it. The lithomarge constitutes essentially the adhesive material; the lime only serves to prevent it from coming off in washing.—*Archiv. du Pharm.* in *Journal de Pharm.*, July 1851.

IODINE RENDERED SOLUBLE IN WATER. BY M. DEBAUGNE OF MONS.

After many attempts to render soluble in water, without the use of iodide of potassium, the small quantities of iodine, in the form of tincture, which are prescribed in draughts, the author was led to discover, that the addition of an ounce of syrup of orange-peel to a four or six ounce mixture, renders five or six grains of iodine perfectly soluble in this quantity of fluid. In investigating as to which of the constituents of the orange-peel had this effect on the iodine, he came to the conclusion that it was due to the tannin. In order to determine the truth of this supposition, he made several experiments, and had recourse to the addition of a few grains of tannin to water, containing ten, twelve, or even fifteen grains of iodine, precipitated from the tincture when added to it. After a few moments' agitation, the solution became complete, and thus proved that it was by the tannin that the iodine had been rendered soluble.

Being unable to explain why this vegetable acid should thus favour the solution of iodine in watery vehicles, an effect not produced even by the most powerful mineral acids, he contents himself with establishing this fact, which appears hitherto to have been unknown. He recommends to practitioners to have recourse to syrup of orange-peel in draughts containing tincture of iodine, and the addition of a few grains of tannic acid in the preparation of iodine injections.—*Journ. de Pharmacie*, July 1851.

ON THE NITRITE OF THE OXIDE OF ÆTHYLE. BY DR A. STRECKER.

The composition of the so-called nitric æther has been established by the analysis of Dumas and Boullay, who arrived at the empirical formula $C^4\ H^5\ NO^4$. Chemists differ in opinion as to the rational composition, the majority having adopted the view that the so-called nitric æther belongs to the compound æthers, and must consequently be considered as a combination of anhydrous nitrous acid with oxide of æthyle, $C^4\ H^5\ O,\ NO^3$, or as the hydrate of nitrous acid, in which 1 equivalent of hydrogen is replaced by 1 equivalent of æthyle, whilst other chemists do not reckon it in this class of compounds. Of the most recent views, I shall notice only that of Gerhardt, who considers the so-called nitric æther as the nitro-compound of the hydrocarbon $C^4\ H^6$ (acetone), and writes its formula accordingly, $C^4 \begin{matrix} H^5 \\ NO^4 \end{matrix}$.

The most decisive character of an æther is its separation into alcohol and an acid by the action of alkalies. Now, according to Liebig's statement, this nitric æther is resolved, on treatment with hydrate of potash, into alcohol and nitrite of potash. No difference of opinion could therefore exist as to the constitution of this body, if this mode of decomposition, which was only proved by qualitative reactions, had not been brought into doubt ; I have, therefore, been induced to make some experiments on the decomposition of nitric æther by potash.

The nitric æther used in the following experiments was disengaged according to the method of E. Kopp, by mixing alcohol, nitric acid, and copper shavings together, freeing it from any admixture of nitric oxide or nitrous acid, by passing it through a lukewarm solution of protosulphate of iron in water, and condensing it in a freezing mixture. The slightly-yellowish mixture thus obtained was decomposed in closed vessels with fragments of hydrate of potash ; at first it turned quite brown, indicating the presence of aldehyde in the nitric æther prepared according to the above method ; and a crystalline salt separated, which disengaged copious red fumes upon the addition of sulphuric acid. The separation of this crystalline salt continued for several days, that which fell last being least coloured. After standing for several days, the liquid was mixed with water, any still undecomposed nitric æther expelled by a gentle heat, and the residual liquid submitted to distillation,—the product exhibited the properties of dilute alcohol.

The so-called nitric æther was decomposed far more readily by an alcoholic solution of hydrate of potash, than by pieces of hydrate of potash ; in this case the liquid also turned brown, and the salt which first separated was of a dark colour. The yellowish-brown liquid decanted from the latter gradually deposited, on being set aside, more crystals, which, after pressure between blotting-paper, proved to be pure nitrite of potash. Its aqueous solution was precipitated with nitrate of silver, the precipitate dissolved in boiling water, whence it crystallised on cooling in slender needles. 0·2036 grammes of these crystals dried in the air left, on calcination, 0·1428 grammes, or 70·1 per cent. of silver. The nitrate of silver, $AgO,\ NO^3$, contains, according to theory, 70·1 per cent. of silver.

There can, therefore, no longer be any doubt that the so-called nitric æther yields, on decomposition with alkalies, alcohol and alkaline nitrite, and consequently belongs to the class of compound æthers. It is certainly decomposed with some difficulty by hydrate of potash at the ordinary temperature, but, as is well known, the æthers differ very much with respect to their facility of decomposition. The affinity of the nitrous acid for the oxide of æthyle is exceedingly great, as the easy formation of the nitrite of the oxide of æthyle shows.—*Chemical Gazette*, from Liebig's *Annalen*, March 1851.

Part Fifth.

MEDICAL NEWS.

OBSERVATIONS ON THE TEAS OF COMMERCE.

BY R. WARINGTON, F.C.S.

In my previous communication to the Society on this subject, in February 1844 (Memoirs and Proceedings of the Chemical Society, ii. 73), I endeavoured to show that there exist two distinct kinds of green tea, known in commerce as *glazed* and *unglazed;* that the former is coloured by the Chinese with a mixture of Prussian blue and gypsum, to which a yellow vegetable colouring matter is sometimes added, while the latter are merely dusted with a small quantity of gypsum; that in the specimen of the so-called Canton gunpowder, this glazing or facing is carried to the maximum. I also mentioned, that I had never met with a sample of green tea in which the blue tint was given by means of indigo. Since the publication of that paper, I have been in communication with several parties of great experience in this subject, from whom I have received much additional information, which, with several experimental points of interest that have come under my own immediate observation, will form the subject of the present paper.

The first point to which I wish to call the attention of the Society is, the question of the blue colouring matter used by the Chinese for colouring the green teas being Prussian blue, because some doubts have been thrown on this subject from various quarters. Mr Bruce thus states (Report on the Manufacture of Teas, &c., by C. A. Bruce, Aug. 16, 1839):—"The Chinese call the former (the indigo) *Youngtin*, the latter (the sulphate of lime) *Acco*." Now I am favoured with the opinion of Mr J. Reeves on this point, whose knowledge and experience render him most competent to decide in such a case; he believes that indigo is *never* employed for colouring used on tea, that the term Youngtin, as used by Mr Bruce, should be Yong-teen, *foreign blue*, the name given by the Chinese to Prussian blue, in contradistinction to Too-teen, *native blue or indigo;* this, I think, is very conclusive evidence, and shows that Mr Bruce's statement was erroneous.

In another quarter a surmise has also been published on this same point. Mr Fortune, in his entertaining work (Three Years' Wanderings in the Northern Provinces of China, by Robert Fortune) on China, says, speaking of the ingredients used in dying the northern green teas for the foreign market, p. 201:—"There is a vegetable dye, obtained from *Isatis Indigotica*, much used in the northern districts, and called Tein-ching, and it is not unlikely that it may be the substance which is employed;" again, at p. 307,—"I am very much inclined to believe that this (the Tein-ching) is the dye used to colour the green teas which are manufactured in the north of China, for the English and American markets." This question, however, I think is now satisfactorily settled, and the experimental evidence I had adduced of the material being Prussian blue of a darker or paler tint, placed beyond a doubt by a positive demonstration; for Mr Fortune has forwarded, from the north of China, for the Industrial Exhibition, specimens of these materials, which from their appearance, there can be no hesitation in stating, are fibrous gypsum (calcined), turmeric root, and Prussian blue; the latter of a bright pale tint, most likely from admixture with alumina or porcelain-clay, which admixture may account for the alumina and silica found as stated in my previous paper, and the presence of which was then attributed possibly to the employment of kaolin or agalmatolite.

Mr J. R. Reeves, in a letter to my friend, Mr Thompson, dated July 1, 1844, commenting on my paper, says:—"Mr Warington's experiments have led him to correct results as to the substances used, which I know to be Prussian blue, gypsum (fibrous), and turmeric; the second being sulphate of lime; and the last, the 'yellow, or orange-coloured vegetable substance,' which Mr W. does not otherwise name. That the colouring is not intended as an adulteration, I feel quite sure. It is given to suit the capricious taste of the foreign buyers, *who judge of an article used as a drink by the eye instead of the palate.* You well know how little the London dealers, even now, like the yellowish appearance of uncoloured green tea. The Americans, a few years since, carried the dislike even further than the English, and therefore the Chinese merchant had scarcely a chance of selling his tea unless he gave it a 'face' that would suit *their fancy.* The small quantity of the colouring matter used, must preclude the idea of adulteration as a matter of profit." Mr J. Reeves states, "that in the East India Company's time, gypsum and Prussian blue were sometimes used upon hyson teas, Tien Hing using the first on his pale, bright hyson; Lum Hing, the latter on his dark, bright leaf; but these were only in minute quantities just sufficient to produce an uniform face."

It is still a question of interest, which I before alluded to, whether the gypsum in its *calcined* state is not used for the absorption of the last portions of moisture, and allowing the tea the better to withstand the damp of the sea voyage. Through the kindness of Dr Royle, I have received, since my last communication, a sample of green tea from the Kemaon district, in the Himalayas, which is quite free from any facing, as are also the green teas of Java, a large number of which I have had the opportunity of examining, and which are exceedingly clean and genuine in their appearance and characters.

On Black and Green Teas.

Although the preparation of green and black tea from the respective plants, the *Thea Viridis* and the *Thea Bohea*, has been warmly advocated by many botanists, yet it is now, I believe, pretty generally admitted by all parties, that both green and black teas can be and are made, indiscriminately from the same parcel of leaves, taken from the same species of plant. It is also well known to all persons, that the infusions from these teas have marked differences of colour and of flavour, and that the effects produced on some constitutions by green tea, such as nervous irritability, sleeplessness, &c., are very distinct from those of black tea. Their characteristic physical differences are too well known to require any comment, but they have peculiar chemical properties to which we shall have occasion to allude more particularly presently, and which have always been attributed by chemists to the effect of high heat in the process of manufacture.

The question presents itself then—from whence do these distinguishing peculiarities arise, and to what are they to be attributed? From observations made in other directions, in the course of the routine work of the establishment to which I am attached, I had formed in my own mind certain conclusions on this subject. I allude to the exsiccation of medicinal herbs; these are for the most part nitrogenous plants, as the *Atropa belladonna*, the *Hyoscyamus niger*, the *Conium maculatum*, and others. The plants are brought to us by the growers or collectors from the country, tied up in bundles, and when they arrive fresh and cool they dry of a good *bright green* colour; but, on the contrary, it is found that if they are delayed in their transit, or remain in a confined state for too long a period, they become heated, from a species of spontaneous fermentation, and when loosened and spread open emit vapours, and are sensibly warm to the hand; when such plants are dried, the whole of the *green colour* is found to have been destroyed, and a *red-brown* and sometimes a *blackish-brown result* is obtained. I had also noticed that a clear infusion of such leaves evaporated carefully to dryness was not *all* redissolved by *water*, but left a quantity of *brown oxidised extractive matter*, to which the denomination *Apotheme* has been applied by some

chemists; a similar result is obtained by the evaporation of an infusion of black tea. The same action takes place by the exposure of the infusions of many vegetable substances to the oxidising influence of the atmosphere; they become darkened on the surface, and this gradually spreads through the solution, and on evaporation the same *oxidised extractive matter* will remain insoluble in water. Again, I had found that the green teas, when wetted and re-dried, with exposure to the air, were nearly as dark in colour as the ordinary black teas. From these observations, therefore, I was induced to believe that the peculiar characters and chemical differences which distinguish black tea from green, were to be attributed to a species of heating or fermentation, accompanied with oxidation by exposure to the air, and not to its being submitted to a higher temperature in the process of drying, as had been generally concluded. My opinion was partly confirmed by ascertaining from parties conversant with the Chinese manufacture, that the leaves for the black teas were always allowed to remain exposed to the air in mass, for some time before they were roasted. Mr Ball, in his valuable work (An Account of the Cultivation and Manufacture of Tea in China, by Samuel Ball, Esq.) on the manufacture of tea, has described in detail the whole routine of these interesting processes, fully confirming my preconceived opinions, and of which I cannot do better than give you a summary. Some of the facts, I believe, had been published in Batavia in 1844, by Mr Jacobson (Handboek v. d. Kult. en Fabrik v. Thee), in the Dutch language. In the preface to his work, Mr Ball says:—"It will be seen by dates incidentally adverted to, that the facts and most of the materials of this work, were established and collected thirty years ago."—"These facts, as well as other materials, were derived from conversation with growers and manipulators from the tea districts; from written documents furnished by Chinese; from published works in the same language diligently sought out; and also from correspondence with a Spanish missionary long resident in the province of Fokim. These were all put into their present form full twenty years ago, and were read to one or two friends during my residence in China."—"They were not, however, so arranged, with any view to immediate publication."—"They were thus disposed, as the best mode of recording and keeping together the facts and materials I had collected."—"But it was not till the year 1844, when I received Mr Jacobson's Handbook on the cultivation of tea in Java, that I found my own views so far confirmed, and my information such as to justify me in bringing my labours to a close.

The processes peculiar to the preparation of black tea, are styled Leang-Ching, To-Ching, and Oc-Ching, and these all consist in carefully watched and regulated processes of *spontaneous heating* or *slow fermentation* of the leaves until a certain degree of fragrance is developed. The leaves are said to *wither* and *give*, and become soft and placid. The utmost care, practical skill, and experience is required in the properly conducting these operations, and as soon as the proper point is arrived at, the leaves are to be immediately removed to the Kuo or roasting-pan. After being roasted and rolled two or three times, they are then to be dried, and this is effected in the Poey-long, which consists of a cylinder of basket-work, open at both ends, and covered on the outside with paper; it is about $2\frac{1}{2}$ feet in height, and $1\frac{1}{2}$ in diameter, which diameter is diminished in the centre like an ordinary dice-box to one foot and a quarter. This stands over and round a small charcoal fire, and is supplied with cross-bars about fourteen inches above the fire, on which an open sieve containing the tea is placed; and a small aperture about an inch and a half in diameter is made in the centre of the tea with the hand, so that an ascending current of air and the products of the combustion pass through and over the tea contained in the sieve. A circular, flat bamboo tray is placed partially over the mouth of this cylinder, and most probably serves to regulate the rapidity of the ascending current, prevent the admission of the cold air to the leaves, and at the same time allow a sufficient outlet for the generated watery vapours and the products of combustion. At the commencement of this operation, the moist leaves are still green and retain their vegetable appearance; after the drying has continued about half an hour, the leaves are

turned, and again submitted to the heat for another half-hour; they are then taken out, rubbed and twisted, and after sifting away the small dust, again returned to the sieve and drying tube. This operation of sifting is very necessary, to remove any of the small tea or dust which might otherwise fall through the meshes of the sieve on to the fire, and the products of their combustion would deteriorate and spoil the flavour of the tea. The leaves have now begun to assume their black colour; the fire is diminished or deadened by ashes; and the operation of rolling, twisting, and sifting, is repeated once or twice until they have become quite black in colour, well twisted, and perfectly dry and crisp. They are then picked, winnowed, and placed in large quantities over a very slow fire for about two hours, the cylinder being closed.

Now, that this black colour is not owing to the fire is evident; for in cases mentioned by Mr Ball, where the leaves have been dried in the sun, the same colour is obtained; and on the other side, if roasted first, without the process of fermentation or *withering*, and then finished in the Poey-long, a kind of green tea is produced.

In the operations for the manufacture of green tea, on the contrary, the freshly-picked leaves are roasted in the Kuo at once, without delay, at a high temperature; rolled and roasted again and again, assisted sometimes with a fanning operation to drive off the moisture; and always with brisk agitation until the drying is completed.

The marked differences in the mode of manufacture of black and green tea, will, I consider, after what has been stated, fully account for all the variation of physical and chemical properties to which I have before alluded.

Adulteration and Sophistication of Teas.

Since writing my former paper, several teas have come under my notice which must be classed under this head. The first I shall mention is a sophistication which has been carried on in this country to some extent, and consists in giving the appearance of green tea to an imported black tea. The material used as the bodies for this process of manufacture is a tea called scented caper; it is a small, closely-rolled black tea, about the size of small *gunpowder*, and when coloured, is vended under this latter denomination, the difference in price between the scented caper and this fictitious gunpowder being about 1s. per pound, a margin sufficient to induce the fraud. This manufacture has been carried on, I understand, at Manchester, and was kept as secret as possible; and it was only after considerable trouble that some of my friends succeeded in obtaining two different specimens for me, that could be fully depended on, as originating in this manufactory. It appears that it is generally mixed with other tea, so as to deceive the parties testing it. How this manufactory was conducted I am not prepared to say; but some preparation of *copper* must have been employed, as the presence of that metal is readily detected in the specimens I received. I believe, however, that this sophistication has ceased.

I have now to call your attention to another adulteration of the most flagrant kind. Two samples of tea, a black and a green, were lately put into my hands by a merchant for examination, the results of which he has allowed me to make public. The black tea was styled *scented caper;* the green, *gunpowder;* and I understand they are usually imported into this country in small chests called catty packages. The appearance of these teas is remarkable; they are *apparently* exceedingly closely rolled, and very heavy; the reasons for which will be clearly demonstrated. They possess a very fragrant odour. The black tea is in compact granules, like shot of varying size, and presenting a fine glossy lustre of a *very black* hue. The green is also granular and compact, and presents a bright pale-bluish aspect, with a shade of green, and so highly glazed and faced, that the facing rises in clouds of dust when it is agitated or poured from one vessel to another; it even coats the vessels or paper on which it may be poured. On examining these samples, in the manner described in my former paper, to remove this facing, I was struck by the tenacity with which it adhered to the surface,

and which I had never remarked in any previous sample, requiring to be soaked for some time in the water before it could be detached; with this precaution, however, the greater part of the facing material was removed. It proved, in the case of the sample of green tea, to be a pale Prussian blue, a yellow vegetable colour, which we now know to be turmeric, and a very large proportion of sulphate of lime. The facing from the sample of black tea was *perfectly black* in colour, and on examination was found to consist of earthy graphite or black lead. It was observed, that during the prolonged soaking operation, to which these teas had been submitted, there was no tendency exhibited in either case to unroll or expand, for a reason which will be presently obvious. One of the samples was therefore treated with hot water, without, however, any portion of a leaf being rendered apparent. It increased in size slightly, was disintegrated, and then it was found that a large quantity of sand and dirt had subsided; this was separated by decantation, and collected; it was found to amount to 1·5 grains from 10 grains of the sample, or 15 in the 100 parts. It was evident, however, that much of the lighter particles must necessarily have been lost in the process of decantation; a weighed quantity of the sample was therefore carefully calcined, until the ash was quite white, and the whole of the carbonaceous matter burnt off, it yielded a result equivalent to 37·5 on the 100 parts. During this operation, also, no expansion or uncurling of the leaf, as is generally to be observed when heat is applied to a genuine tea, was seen; in fact, it was quite evident that there was *no leaf to uncurl*, the whole of the *tea* being in the form of dust. The question next presented itself as to how these materials had been held together, and this was readily solved; for, on examining the infusion resulting from the original soaking of the sample, abundant evidence of gum was exhibited.

The sample of green tea was of a precisely similar kind to the black; it yielded 4·55 grains of ash, &c., from 10 grains of the specimen, or 45·5 per cent. A specimen of Java gunpowder yielded 5 per cent. of ash; so that we have in this sample 40·5 per cent. of dirt and sand over and above the weight of ash yielded by the incineration of a genuine tea.

Thus we have then in these samples a mixture of tea-dust with dirt and sand, agglutinated into a mass with a gummy matter, most probably manufactured from rice-flour, then formed into granules of the desired size, and lastly dried and coloured, according to the kind required by the manufacturer, either with black lead, if for black tea; or with Prussian blue, gypsum, or turmeric, if intended for green.

Since examining these two samples, I have obtained through a friend another specimen of green tea, having a very different appearance; that is, better manufactured, or rather, I should say, more likely to deceive the consumer, from its being made to imitate an *unglazed tea*. It is of a yellowish-green colour, scented and granulated as the former samples, and not much dusted; it yielded 34 per cent. of ash, sand, and dirt.

On inquiry, I have learned that about 750,000 lbs. weight of these teas have been imported into this country within the last eighteen months, their introduction being quite of modern origin; and I understand that attempts have been made to get them passed through the Customs as *manufactured goods*, and not as teas; a title which they certainly richly merit, although it must be evident, from a moment's consideration, that the revenue would doubtless be defrauded, inasmuch as the consumer would have to buy them as teas from the dealer. It is to be feared, however, that a market for them is found elsewhere. The Chinese, it appears, will not sell them except as teas, and have the candour to specify them as *lie* teas; and if they are mixed with other teas of low quality, the Chinese merchant gives a certificate, stating the proportion of the *lie tea* present with the genuine leaf. This manufacture and mixing is evidently practised to meet the price of the English merchant. In the case of the above samples, the black is called by the Chinese, *lie flower caper;* the green, *lie gunpowder;* the average value is from 8d. to 1s. per lb. The brokers have adopted the curious term *gum and dust*, as applied to these lie teas or their mixtures, a cognomen

which at first I had some difficulty in understanding, from the rapid manner in which the two first words were run together.

I may subjoin the results obtained from the careful incineration of a variety of teas, as they may be interesting, for the purpose of comparison, and illustrate the point I have mentioned as to these spurious teas being mixed with genuine ones.

Gunpowder tea, made in Java, gave	5·0 grains of ash in the 100 parts;
Gunpowder, during the East India Company's Charter,	5·0
Kemaon hyson,	6·5
Assam hyson,	6·0
Lie gunpowder, No. 1,	45·5
... No. 2,	34·0
Scented caper,	5·5
Lie flower caper,	37·5
Mixtures containing these lie teas, No. 1,	22·5
... ... No. 2,	11·0

—*Quarterly Journal of the Chemical Society*, July 1, 1851.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING IV.—*June* 28 1851.—Professor BENNETT, P., in the Chair.

REPORTS.

MECHANISM OF THE PELVIC ARTICULATIONS.

1. *Dr W. T. Gairdner* read the following report by Mr Barlow and himself, on the views submitted to the Society by Mr Zaglas, on the subject of the mechanism of the pelvic articulations:

The prevalent idea in regard to the connection of the sacrum with the ossa innominata is, that it is a wedge placed between these bones all but immoveably, and receiving the pressure from above downwards of the spinal column, while from below upwards, and laterally, it is acted on by the counter-pressure of the limbs, communicated through the curve of the ossa innominata. The balance of forces resulting from this arrangement is often stated in a general way; but we are not aware of any attempts to determine more minutely the statical conditions under which the equilibrium of the body is maintained, still less to take scientific account of the motion allowed by the pelvic ligaments, and especially by the sacro-iliac synchondrosis. It appears, however, manifest that the elastic ligaments which exist in this situation cannot be without use in the economy ; and while we do not commit ourselves in all points to an acceptance of the present views of Mr Zaglas, we have no hesitation in stating that these views appear to be a decided advance in physiology, and, for the most part, to correspond well with facts already known, both physiological and pathological.

The extent to which the motions of the sacro-iliac synchondrosis merit consideration, is shown by a very simple experiment of Mr Zaglas, which we have witnessed. In a subject, of which the gluteal regions had been dissected, and the sacro-sciatic ligaments exposed, the lower limbs were immoveably fixed throughout their whole length to two upright parallel bars. On then moving the body backwards from the upright position as far as possible, it was easily remarked that the sacro-sciatic ligaments became perfectly lax, while on bending it forwards they became gradually tighter; and when the body was allowed to fall forward by its own weight, assumed the greatest possible degree of tension. This experiment appears to demonstrate not only that there is considerable motion between the bones above named, but that the motion is of such a character as to place the sacrum in the mechanical position of a lever, of which the fulcrum is some intermediate point in the long axis of the bone, probably (as it appears

to Mr Zaglas) about the second sacral vertebra. When the body is in the normal position (erect), the antero-superior end of this lever is very much shortened, or, indeed probably, is abolished, the centre of gravity of the superincumbent weight being brought directly over the fulcrum; but in any inclination, either backwards or forwards, of the body, the antero-superior end of the sacral lever receives a corresponding bias, and the postero-inferior end an opposite bias. Hence it arises that the sacral attachment of the sacro-sciatic ligaments moves in a direction opposite to that of the promontory of the same bone, the ligaments being relaxed (or their attachment to the sacrum thrown forwards) when the promontory moves backwards in connection with the spine, and being tightened when the spine and the promontory are bent forwards.

When we look at the articulating surface entering into the sacro-iliac synchondrosis, we find the mechanical conditions on which this leverage depends. Mr Zaglas has pointed out to our satisfaction that there are on the os innominatum four distinct surfaces of articulation, inclined at certain tolerably constant angles to one another. Without entering too minutely into detail, we may mention that the two antero-inferior of these (or those nearest the symphysis pubis[1]) correspond to what is commonly called the auricular surface of the sacrum, and are inclined to each other at an angle which looks outwards, and forms a ridge inwards. The two posterior-superior surfaces (or those nearest the spine), which are not so closely connected with the corresponding ones of the sacrum (being separated by a considerable thickness of elastic substance), are also inclined to one another at a similar angle; and there is accordingly a crooked ridge running between the four surfaces, in a direction from the spine to the symphysis. Taking the four surfaces, however, in another relation, it may be seen that the two posterior-superior surfaces are separated from the two anterior-inferior (which constitute the auricular surface) by a *groove* (running nearly parallel with the axis of the sacrum), and are inclined to them at an angle which looks inwards. The consequence of this arrangement is, that while some motion, as will presently be seen, is permitted, any tendency to displacement of the sacrum is entirely obviated, so long as the innominate bones are retained in a due degree of proximity by the pressure of the acetabula, and by their ligaments. In particular, it is obvious that any tendency of the sacrum to be displaced towards the symphysis pubis is met by the resistance of the innominate auricular surface, the anterior portion of which faces upwards and inwards, and, when the body is thrown forwards, receives and supports the whole superincumbent pressure. In this position the anterior part of the auricular surface of the innominate bone acts as a fulcrum to the sacral lever, and all parts of the sacrum beyond it accordingly receive a bias upwards and backwards corresponding with that of the promontory of the bone downwards and forwards. This bias is resisted, first, by the sacro-sciatic ligaments, which, as before-mentioned, become tense in such a position; secondly, by the ilio-lumbar ligaments, which, passing from the posterior iliac spines to the first sacral vertebra, tend to suspend the anterior and superior end of the sacral lever, and limit its motion forwards; and, lastly, by the posterior-superior articulating surface of the ilium, which, inclining downwards and inwards, roofs over the lower end of the sacral bone, and receives its upward pressure through the elastic intervening substance.

Such are what appear to be the most clearly demonstrable and important of Mr Zaglas' views. In the course of our examination into them, we have become satisfied of their general correctness, and of their important relation to several

---

[1] It is right to mention that we depart (in conformity with the usual ideas of the inclination of the pelvis) from Mr Zaglas' nomenclature of these surfaces. He believes the obliquity of the pelvis in the erect position to be greater than that indicated either by Naegele or Weber; considering, in fact, the axis of the first three vertebræ of the sacrum to be, when the body is erect, nearly or quite horizontal. We do not express any opinion on this view, which was not necessarily brought under our consideration in connection with the present subject.

points both of physiology and pathology. Mr Zaglas has, for instance, directed our attention to the fact that in the pelvis deformed by mollities ossium, or rickets, besides the well-known projection forwards of the promontory of the sacrum, the axis of the bone assumes a very acute curve, the portion of it between its first vertebra and the attachment of the sacro-sciatic ligaments being thrown in a marked degree backwards and upwards, while the remaining portion is kept in its original position by the sacro-sciatic ligaments—a condition, of course, clearly referable to the mechanism above indicated. We have observed also, that in some deformed pelves the crest of the ilium presents at its posterior third a very peculiar curve, caused by the promontory of the sacrum bending forward and downward the part to which it is more immediately attached, while the posterior spines of the ilium are raised and pushed backward by the middle vertebræ of the sacrum. Again, a most important physiological deduction from Mr Zaglas' views appears to us to be the following:—The erect position, or that in which the spine is as far bent back as possible, has a tendency to open the brim and contract the outlet of the pelvis, by throwing back the promontory, and forward the coccygeal end of the sacrum; while the bent position of the body has a still greater tendency to enlarge the outlet, and, to a slighter extent, to contract the brim. The application of these facts to the physiology of parturition and of defecation need not be more than noticed.

In conclusion, we would strongly recommend Mr Zaglas to pursue his inquiries in the direction of comparative anatomy, and particularly to observe the modifications of form of the pelvis and its articulations in quadrupeds, whose permanently prone position does not permit the spine to be bent forwards, nor, consequently, the leverage of the sacrum to be thrown into action in this direction.

COMMUNICATIONS.

1. *Dr Bennett* showed a specimen of urine from a patient in the clinical wards. It contained urates in a form very similar to that described by Dr Gairdner at the first meeting of the Society; and a number of scale-shaped bodies, which appeared to the naked eye to float in the urine, were made up of small rounded deposits of urate of ammonia, imbedded in some intervening substance. The case was one of scarlatina. (See Dr Bennett's clinical lecture for August, "Monthly Journal of Medical Science," p. 158.)

Mr Drummond was appointed to report on the deposit.

2. *Dr Bennett* also showed a specimen of albuminous urine, containing tube-casts and epithelium of the kind common in kidney disease. It was from a patient affected with pneumonia, in whom the urine had become albuminous in the stage of resolution. Dr Bennett considered the tube-casts thrown off in these cases to be derived from the fibrin which was in the act of being absorbed into the blood from the lung.

3. *Mr Sanderson* showed the result of an experiment which he had now several times made on the brain of the pigeon. In this case he had five days ago punctured the cerebellum with a needle, and presented to the Society the parts involved in the operation, which showed limited softening, with some hemorrhagic discolouration of the nervous substance. Under the microscope, Mr Sanderson demonstrated several aggregations of blood-globules, to the number of three or four, in each of which a delicate membranous structure was found to surround the whole contained number of globules. (See Mr Sanderson's paper, in this No. of "Monthly Journal of Medical Science.")

Dr W. T. Gairdner was appointed to report on this observation.

4. *Mr Cobbold* exhibited several sections of fossil bone taken from the Red Crag deposit of Suffolk, in which the corpuscles and canaliculi were unusually well-marked, and which presented all the characters of mammiferous bone.

CYSTIC TUMOUR OF THE EYELID.

Mr Sanderson described and exhibited the structure of a small cystic tumour removed from the eyelid by Mr Walker. It consisted externally of a fibrous cyst, with numerous septa, containing oval yellowish bodies about the fortieth of a millimetre in their longest diameter, mingled with numerous irregularly-shaped lamellar bodies, varying greatly in size.

Dr Bennett was of opinion that this was a form of epithelial growth, which he had described in his work on cancerous and cancroid growths as occasionally occurring in ducts and follicles. In this case he believed occlusion of a follicle had occurred, while its contents had increased and caused it to swell—thus proving a cystic tumour. Epithelium was then formed in large quantities, which accumulated frequently in concentric laminæ, round adipose, and other cells, and in course of time, by means of compression and disintegration, presented the various forms now shown under the microscope.

A long discussion now ensued between Mr Sanderson, Dr Gairdner, and Dr Bennett, as to the nature and mode of development of the various structural elements contained in these growths. Further researches, it was agreed, were necessary, and Dr Bennett undertook the inquiry, and engaged to report the results at a subsequent meeting.

INAUGURAL ADDRESS

TO THE CANDIDATES WHO RECEIVED THE DEGREE OF DOCTOR OF MEDICINE AT THE UNIVERSITY OF EDINBURGH, ON 1ST AUGUST 1851.

BY DR CHRISTISON.

Gentlemen,—I beg to congratulate you on the part of the Medical Faculty and Senatus, as I also sincerely do for myself, that you are at length not only Cives, but likewise Doctors of this University, and no longer the pupils of your Professors, but their professional brethren. And, according to custom in this University, and in the belief that you are in a fit frame of mind, after having thus far ascended the ladder of preferment, I have now, as Promotor of the Medical Faculty, to discharge a duty entrusted to me by the Senate, of bestowing on you some parting advice, before you quit these College walls for the last time, to enter on the rough and thorny path of the real business of life.

This is not a seasonable occasion, nor am I perhaps the most suitable person, to enforce on you those general views of moral and religious conduct, which are the most essential of all conditions for securing true happiness even in this world, and without which few have been known to arrive at distinction in a learned profession. As I understand the task allotted to me here, I have rather to point out to you how to turn to good account the knowledge you have been gaining from your teachers here and elsewhere,—and how, with your foot on the vantage-ground you have this day attained, you may go onwards until you work out for yourselves an honourable settlement in life, pleasure in the farther pursuit of medical learning, and, it may be, even a place among the chosen few whom history records as improvers or discoverers in medicine. It is true, I can say little that is absolutely new on such a theme. And yet part of what I have to say may be new to some among you; while at all events, what is not new will be so true, that it cannot be the worse for rehearsal in your present circumstances.

During the last few years which you have spent in studying the several branches of medicine and allied science, you have amassed more knowledge than you will probably ever accumulate again in the same time. You have learned more; because you had every thing to learn. No one can overrate the importance of the period of your life that has just gone by. No one can over-value the fruits of a well-spent youth. And, therefore, I sincerely hope you can all look back upon that stage of your existence with satisfaction.

But this is not enough. You are entering on a period of even greater conse-

quence still,—the turning-point, indeed, or crisis of your fortunes in the present world. For it is the use you are to make of the next few years, that will mainly fix the course of your future professional life, and determine its issue in success and happiness, or in misery and disappointment.

Above all things, then, beware at the outset of falling into the mistake of supposing, because you are now to be suddenly left to your own guidance, and freed from the discipline of students, that you are also to be freed from their duties,—that your studies in short may cease with your graduation. For, on the contrary, instead of being ended, they are in some measure only begun. No one expects that by cultivating the medical sciences systematically at a University, you shall become all at once experienced physicians. You have thus merely secured for yourselves a solid foundation, on which you must now proceed to raise the superstructure of practical knowledge and practical skill. Some of you indeed, aided by more years of experience than the rest, or by peculiar opportunities, have already made no little way in this work. But these, and all of you, have yet much to overtake before you can acquire that familiarity with practice, which will entitle you to entertain a reasonable hope of winning public confidence as professional men.

Do not imagine, however, that there is to be imposed on you any fresh or irksome task. The habits of application which I trust you have all acquired in studying for the honour you have this day received, must have given you some taste of the pleasures of labour, no less than of its fruits. These habits, once learned, if nurtured with ordinary care, will grow with your years; and as they grow, so too will the pleasure which attends their exercise, and the fruits which they will yield in the shape of public esteem and professional prosperity. Cherish then, as graduates, the zeal and diligence you have shown as students: Only do this, and with a well-considered plan of study, there can be no doubt of your professional success in due time,—or at least, that you will have done every thing that lies with yourselves for attaining it.

Much however, indeed every thing, will depend on your studies during the next few years being conducted on a well-considered plan. Desultory study will avail you little. It is impossible for me to lay down such a plan. This must vary with your means; with your opportunities; and most of all, with your destination,—whether that shall be the public services or civil life, the city or the country, private practice or a public station, the duties of general practitioner, or of physician, or of physician and teacher together, or of cultivator of the fundamental sciences of medicine irrespective of practice at all, and lastly, whether there is that within you which fires you with the hope, that you may be destined one day to extend the boundaries of medicine as discoverers.

But at the same time, there are some conditions which all may attend to with advantage, and more especially, that most numerous class among you whose destination is medical practice, whether public or private.

In the first place then, keep fast hold of the knowledge you have already acquired. You may very possibly have heard some lightly repeating a stale sneer, that the first thing a young graduate has to do is to forget much of what he has already learned. But, except the doubtful literary merit of a startling antithesis, there is nothing to praise in this vulgar proposition. It is untrue. If your seniors will speak the truth, they will tell you how often they miss the branches of knowledge acquired in youth, which they have subsequently allowed to escape them; and how little cause they have had to regret any superfluity in their education. Listen not to those who assure you that you may now let go your acquaintance with the fundamental branches of medicine, or even collateral science and literature. You can never entirely forget them, even though you would. The most negligent can never efface the liberalising impression made on your minds by the mere exercise of studying them. They will often furnish in after-life ideas which seem new, merely because you have forgotten whence they have been suggested, but which are not the less on that account the fruit of an extended education. You will moreover encounter not a few occasions, when you may turn to down-

right practical use the knowledge you may have acquired of collateral science. The possession of such knowledge will frequently qualify you for subsidiary duties and offices, which generally fall to the lot of medical men. Nor ought I to omit mentioning, that the farther cultivation of one or more of the fundamental or collateral sciences, or of some branch of allied literature, is one of the most appropriate, pleasing, and profitable relaxations for the medical practitioner, as a relief from the fatigues or monotony of a life of otherwise uninterrupted professional labour. Some may even thus usefully acquire a literary or scientific name, which they perhaps could not attain on a strictly professional field, and nevertheless without surrendering their professional character or prospects.

You are not to suppose then, as many imagine, and some have taught, that there is an incompatibility between success in medical practice, and the successful cultivation of science or of literature. It is true that this has not proved a fortunate conjunction for all. It becomes so only under certain conditions; the chief of these being, that you must take care not to allow your hours of relaxation to encroach on your hours of professional duty and study, and that you must make no display of these your diversions, while engaged in the exercise of your profession.

But secondly, whatever may be the advantage of not forgetting your acquirements in the fundamental and collateral sciences of medicine, do not lose sight for one moment of the imperative necessity of cultivating, and especially during the years which immediately follow graduation, the practice of medicine as the main occupation of your lives. That is, let all your plans for the future be primarily founded on your chief pursuit being directed to Pathology and Therapeutics, and in their widest signification, as comprising all which directly relates to the knowledge of disease and the knowledge of remedies.

I feel that I need say nothing to you in commendation of the former of these branches. Pathology, comprehending in its strict signification the "Ratio Morborum," or theory merely of disease, is usually now understood as implying something much more important, namely, the knowledge of both the nature and the phenomena of disease,—in short the whole of medical practice except its ultimate end, the cure of disease; and some will not except even that altogether from its definition.

In the course of your studies you have heard and seen so much of what is interesting, clear, and practically productive in the present advanced state of pathology, that you can require no encouragement from me to follow it out, and no advice how to do so. But the branch of Therapeutics is differently circumstanced.

I must here claim your indulgence for one moment. I have observed that my colleague, who in annual succession fills the honourable post which is now mine, seldom fails to take advantage of the opportunity to imbue his hearers with the importance of the subject he himself professes. But I am not content with pleading usage as the source of my right to follow that example. I have a higher claim than this. For there can be no physician without physic; no medicine without therapeutics. Pathology is a mere introduction to it. The only true end of medical knowledge is the cure of diseases. But there can be no cure without remedies. Neither can remedies be of any use unless you know them, and how to give them, and what effects to look for from them in health, and in disease, and in the thousand varying circumstances of disease, inherent as well as extraneous. This knowledge summed up constitutes the materia medica; and the branch of it which specially treats of the actions and uses of remedies, and to which my present remarks mainly apply, is called, as you well know, Therapeutics.

It may perhaps appear at first view surprising, that such a subject should need to be pressed on your attention at the present time. But I appeal to yourselves, whether it has hitherto received in the course of your studies that share of your consideration which its relative importance among the practical branches of medicine undoubtedly demands for it; or whether your ideas on the actions

and uses of remedies are as precise as they ought to be, and as they are on other subjects. You know this is not the case. And why? Simply on account of the difficulty of Therapeutics; and because, of all the medical sciences, it is the most unsettled and unsatisfactory in its present state, and the least advanced in its progress.

It is the most difficult of all to investigate satisfactorily, because, besides requiring the most deliberate and frequent observation of individual facts, it is surrounded by the most numerous and most deceitful sources of fallacy. A legal author on the Law of Evidence, who has grasped more comprehensively than any other the relations of evidence to medical science, thus speaks of therapeutic inquiries,—"In reasoning as to the probable effect of particular remedies on the human body, the condition and circumstances of the latter are so various in different cases, and the number of concomitants which are to be considered in addition to the more obvious facts and symptoms, is so great, that the utmost exertion of human sagacity, founded upon the largest induction of particulars which any one mind is capable of embracing and retaining, can do no more than approximate to that real evidence, of which the case seems by its proper nature to be susceptible (Glassford's Principles of Evidence, 1820, p. 135)."

But it is not the difficulty merely of therapeutic study, which deters many from undertaking it. There is no want of minds so constituted, that mere difficulty would prove to them any thing but an intimidation. It is also the unsatisfactory condition of the subject, and the failure of previous inquiries to elucidate it, together with the seductions of simpler and more promising pursuits, which have deterred many from embarking in the special cultivation of this branch of medical knowledge. Compared with pathology, for example, how few, during the last forty years, have been the steps made in advance by therapeutics, and how trifling its discoveries? In that period the face of pathology has been entirely renewed. It has absorbed the attention of almost every great practical inquirer in Europe. New instruments of research have been applied to it. Percussion, the stethoscope, the microscope, and chemical analysis have made us almost wonder in these days how medical men only fifty years ago could practise their profession to any good purpose. In therapeutics, on the other hand, we have had no new instruments to aid inquiry, no galaxy of talent concentrated on its cultivation, no great discoveries, even few great improvements, commensurate at least with its vast importance, or the improvements made in other medical sciences. There has been no want indeed of new remedies of empirical origin, some of which, introduced during the last forty years, have stood the test of time and experiment, and promise to be enduring benefits to mankind. But these are nothing to the incalculable mass of rubbish, the offspring of delusion or of imposture, which have been proposed year after year, only to be forgotten after a brief season of unreasonable popularity. And as for the theory of therapeutics, embracing a generalisation of the actions of remedies, an accurate description of the physiological effects of each, a just idea of their therapeutic influences, and a knowledge of the conditions for employing them,—we are little in advance of our predecessors thirty years ago. What is still worse: there is no other branch of medicine in which quackery has reigned so rampant. One adventurer after another has realised an enormous fortune with his Universal Remedy. Unscrupulous members of the medical profession, improving on such simple knavery, come forth annually with a new fashion of cure, so that the dupes of this fine-drawn empiricism are as certain of a new remedy with every fresh season of fashion, as of a new dress or a new novel. Some ascending a step higher still in refinement, attach a theory to their contrivances, and thus affect to clothe their art in the dignity of science. And these too find their crowds to believe or tolerate them; for let a man once succeed in raising the watch-cry of science on his side, and no fiction is too wild for a certain class of society. It is not many years since a sect arose in this country, who, convinced by a dreaming physician that all diseases originate in animal food and impure drink, were prevailed on to live for years upon vegetables and distilled water. More recently,

multitudes of all ranks in London were persuaded that the most multifarious chronic diseases depended all alike on a peccant acid humour, which the author of this theory drew out of their backs and joints by irritating applications, and presented in substance to credulous believers; a physician too was found, who believed, or affected to believe him, and conformed to his practice; and many continued to resort to him even in his prison, to which he was sentenced for manslaughter, occasioned by the strong corrosives he applied. In your own time an ignorant peasant, stretching to the uttermost the doctrine of the curative influence of cold water, has been the means of great establishments being founded, where all who resort to them are promiscuously cured by an overflow of water, outside and inside; and the *auri sacra fames* has tempted men, who bear the honoured title you have now earned, to preside over these Neptunian mysteries. Wildest of all, and widest in its epidemic range, because most cunningly disguised in the borrowed trappings of science, a practice has lately arisen, which demands belief in the assumption, that all curable diseases may be cured by remedies so inconceivably minute, that no sense, no balance, no microscope, no chemical analysis, no human means whatever, can discover the slightest trace of them in what is administered,—that you can never have any surety of their presence, or any well-grounded assurance of their absence,—that they defy the finite faculties of man to form even a distinct conception of them, either with the aid of figures and the eye, or with the help of a vivid imagination. Nevertheless this creed, in no respect perhaps more repugnant to common sense than that it involves the disregard and annihilation of the entire therapeutic experience of twenty centuries, has taken root in every country of Europe, has obtained a footing in the palace as well as the cottage, and has in the end poisoned the faith even of a few votaries of medicine.

But, nevertheless, do not allow even such lamentable events to bewilder your judgment, or stagger your common sense. Nothing in the history of the human mind is to our frail human faculties more astounding, though more undeniable, than its frequent wanderings, even in the highest and most cultivated intellects. One of the first and most practical of engineers, to whom the world owes a great invention, believed that our earth, instead of slowly wasting, as all may see, under constant meteorological influence, is actually growing by constant deposition from the atmosphere. And the greatest of English lexicographers, believed in the second sight, and in the reality of ghosts, but had no faith in the great earthquake of Lisbon.

You will then ask how you are to study therapeutics in face of all these obstacles, arising out of the inherent difficulties of the subject, and the pitfalls of delusion and error which surround it. I take it for granted that mere difficulty will not deter you in face of an evident necessity, and under the invitation of seeing at your feet a rarely trodden path, which is certain to lead, sooner or later, to important discoveries; and if necessity and temptation together will not carry you along it, I am afraid I can add nothing farther to encourage you in the brief time allotted me for these observations. But I may perhaps aid some of you to avoid, in your inquiries after truth, the snares of imposture and bye-ways of delusion, into which in these days, perhaps more than at any prior period, the young physician is apt to be betrayed. Do not, however, overrate my capabilities in this respect. Your time will not allow me, though I were able otherwise, to conduct you step by step from the true faith to a sober experimentalism, from experimentalism to heresy, and through heresy to quackery, depicting each in well-defined and recognisable figures. It is impossible in therapeutics, any more than in mixed science at large, to draw a distinct definition of truth and of error in the abstract. In the mixed sciences, all definitions, unless of simple objects, are unstable and dangerous. But, if we cannot safely define those which are complex, we can at least represent them all by their external characters, and so clearly that all men of common sense may recognise them. And I apprehend this may be done well enough in therapeutics, so as to enable you to distinguish what is false, or probably false, from what is true, or probably true. Nor in-

deed will it be necessary here to go beyond extrinsic characters to attain our purpose.

First, then, it is undeniable that all important discoveries in science at large, are preceded by a period of incubation, as it were, during which the world is gradually prepared to receive them. There appears a fitness in the very time of discovery. Hence hints of the truth precede its development; often, indeed, a near approximation has evidently led to it. Hence, too, a brief interval of hesitation on the part of the public is quickly followed by universal belief. Hence, moreover, arise disputed claims of invention—a circumstance observed even of most minor discoveries. Tried by this standard, all great delusions in therapeutics are found wanting. There has been no shadow cast before the coming event, no antecedent approximation, no universal adoption, no contending claimant.

In the second place, all true discoveries have made way first among the votaries of the science most immediately concerned with them, and have become matters of faith and interest with the public at large only in the second instance. So has it been with therapeutics. I do not mean that the rule applies always to the discovery of special remedies. Even here indeed it applies generally, yet not always; for the discovery of a new remedy has been sometimes the result of mere accident or opportunity, which may both unfold the fact, and long confine it, within extra-professional boundaries. But the rule applies without exception to all therapeutic discoveries essentially involving doctrinal considerations. The antivariolous property of vaccination, the prevention and cure of scurvy, the antiphlogistic action of calomel and opium, the non-mercurial treatment of the syphilitic disease, the contro-stimulant uses of tartar-emetic, the utility of anæsthetics in surgery, the substitution of the vegetable alkaloids for the crude drugs that yield them, the modern treatment of the several calculous diatheses, the cure of delirium tremens by stimulants and opium, the arrestment of phthisis by climatic changes; these discoveries, after being presented to the medical profession, were deliberately investigated and widely adopted there, without any appeal to the prejudices or imagination of the general public. They were not displayed so as to catch first the imagination of the unprofessional, to spread there like a pestilence, and then to be thrust upon an unwilling profession. There are certain never-failing and well-known hotbeds in this country for the propagation of quackery, whether in the shape of nostrums or dogmata. None of the therapeutic improvements I have mentioned, no discovery indeed that has outlived a few years of fashion, has spread in the first instance in these quarters. But there it is that are rooted the most popular delusions of the present day, which physicians are taunted to investigate as possible truths and questions of genuine science.

Should these characters be not enough to enable you to distinguish the true from the false in therapeutic innovation, there is yet a third criterion remaining. No upright physician ever attained great success in practice on a sudden or at an early age. For the success of such a one, experience is a necessary condition, and for experience time. No physician ever found himself immediately raised from obscurity into great employment by either inventing or adopting a novelty in therapeutics, which has subsequently stood the test of inquiry and of time. When, therefore, you may chance to see a professional brother swimming swiftly into fame and fashion on the current of some new popular dogma, when you hear of him boasting that he had scarcely settled down, a young physician, or an unknown man, in a strange place, when he found himself in full favour and affluence, I trust you may not even envy him for his prosperity; but at any rate he cannot be any worthy example to you for the means of attaining it.

Much more might yet be said upon this subject; and there are many other matters which might equally claim your attention at the present moment. But time presses; and I have already detained the senate and this assembly of our friends and yours too long. One word more, then, and let us part.

In the *Sponsio Academica*, by which you have this day bound yourselves—the only oath which is now ever administered within these academic walls—you have pledged yourselves to the discharge of sundry serious obligations. Among these you have declared that, to the last day of your lives, you will nourish the duty of gratitude towards the University in which you have been bred—*in omni grati animi officio erga Academiam Edinburgensem ad extremum vita halitum perseveraturos.* There is no way in which you can more fitly exhibit this gratitude than by striving to render yourselves worthy of the fame which the University has acquired, and to take your place beside the many good and wise physicians who have issued from it in past times, and beside those of the present day whom you will find scattered over the whole civilised globe, into whatever region your steps may be turned.

We, your teachers, have taken no *Sponsio* in return. In fact, it is somewhat singular that we have been allowed to discharge all our duties to you in the University without the obligation of any oath at all. But depend upon it nevertheless, that this University will be always proud to claim her sons who distinguish themselves by their professional attainments; and that we who now represent her, although we must in a moment part with you, and can never again see you all assembled before us, will always be delighted still to do whatever we can to make you wise, and good, and prosperous, and happy.

LIST OF GRADUATES

WHO OBTAINED THE DIPLOMA OF DOCTOR IN MEDICINE, IN THE UNIVERSITY OF EDINBURGH, ON THE 1ST AUGUST 1851.

**** Those who have obtained Prizes for their Dissertations.*
*** Those selected for Competition for the Dissertation Prizes.*
** Those Commended for their Dissertations.*

Abercrombie, James, Cape of Good Hope. On the Diseases that accompany and follow Idiopathic Fever.
*Balfour, Thomas Alexander Goldie, Scotland. On Alcohol as an Etiological Agent.
*Black, James, England. On the Clinical Investigation of Disease.
*Bone, Theodore Gordon, Barbadoes. On the Diseases of Troops in British Guiana.
5*Buckle, Robert Thomas, England. On the Balance of Food.
*Carrington, Benjamin, England. On Entophytes found in Man.
Christison, David, Scotland. On Emphysema of the Lungs.
***Cobbold, Spencer, England. On the Canal of Petit.
Cowper, James Alexander, Scotland. On Menstruation.
10*Cuningham, James Macnabb, Cape of Good Hope. On the Morbid Conditions of the Aorta.
Cutts, William Henry, England. On Dropsy.
Eastwood, Joseph William, England. On the Natural Food of Man.
*Fairweather, James, Scotland. On Temperature.
**Fisher, Alexander, Scotland. On the Influence of Climate.
15 Fowler, Robert, England. On Syphilis Infantum.
Govan, George Moncrieff, East Indies. On the Structure of the Liver.
Guthrie, John, Scotland. On Dysentery.
*Heddle, Matthew Forster, Orkney. On the Ores of Metals.
Hoile, David Ogilvy, Scotland. On the Urinary Secretion.
20 Howatson, William Collingwood, Scotland. On Sympathy.
*Hughes, John Robert, Wales. On the Anatomy, Physiology, and Pathology of the Arteries.
**Keith, David, Scotland. On Stricture of the Urethra.
Knowles, George Stevenson, Scotland. On Chloroform.

*Liebermann, Edward, Berlin. On the Muscular Action of the Pregnant Uterus.
**Maclagan, James M'Grigor, Scotland. On Colchicum Autumnale.
*Meiklejohn, John William Sinclair, Scotland. On the Organs of Locomotion in the different Classes of Animals.
Messer, John Cockburn, Scotland. On Lactation.
***Millington, William, England. On the Pathology of Pus in the Blood.
Moreton, William Robert, Ireland. On the Management and Nutrition of Early Infancy.
***Murchison, Charles, Jamaica. Contributions to the Pathology of Tumours.
Nicolson, Thomas, England. On Diabetes.
Payne, Henry, England. On Phthisis Laryngea.
Pearse, William Henry, England. Practical Illustrations of Medicine.
Reed, Henry Wilson, England. On Apoplexy.
Roden, Serjeant Samuel, England. On the Treatment of External Aneurism by Compression.
Roome, Henry, East Indies. On Dropsy.
***Sanderson, John Scott, England. On the Metamorphoses of the Coloured Corpuscles in extravasated Blood.
*Scott, George, Naples. On Respiration.
*Simpson, Edward, England. On Hysteria.
Stewart, George Easson, Scotland. On Reproduction.
Whitchurch, Reuben, England. On Scarlatina.
Wilkinson, William, Scotland. On the influence of Light and other imponderable agents on Animal and Vegetable Life.
Williams, Thomas Wynne, Wales. On the Inflammation of Bone.
**Wright, Henry Goode, England. On Consecutive Abscess.
*Wyld, George, Scotland. On the Liver, the Hydrogenator in Animals.

RESOLUTIONS OF THE MEDICAL FACULTY OF THE UNIVERSITY OF EDINBURGH RELATIVE TO THE CHAIR OF GENERAL PATHOLOGY.

We are requested, by authority, to publish the following resolutions, which were passed unanimously on the 11th of August last by the Medical Faculty of the University of Edinburgh:—

"The Medical Faculty of the University of Edinburgh consider it their duty at the present time to make the following statement as to the position of the Chair of General Pathology.

"In 1844, on learning that Dr Henderson, then acting as one of the Professors of Clinical Medicine, had introduced the practice of homœopathy into the University wards of the Royal Infirmary, the Faculty remonstrated with him in the most friendly spirit against this proceeding, and warned him of the serious consequences to himself and the University. As their remonstrances and warnings made no impression, they were under the painful necessity of intimating to him, that they could no longer place confidence in him as a clinical teacher.

"This resolution was communicated to the Managers of the Royal Infirmary. The managers had at the same time taken up the question of their own accord.

"When matters had proceeded thus far, Dr Henderson rendered all farther proceedings unnecessary by resigning his office as one of the Professors of Clinical Medicine.

"In the case of the Chair of General Pathology, the Faculty were very differently situated; as they had over that Chair no jurisdiction of any kind available in the circumstances. They were therefore long averse to stir a question which they could not themselves settle. But they entertained hopes, either that Dr Henderson would ere long awake to the evil of the principles he had assumed, or would see their incompatibility with the system of medical instruction pursued in this University.

"But lately, the sentiments of the medical profession in regard to homœopathic practice have been expressed in the most decided terms by unanimous resolutions of the Royal Colleges of Physicians and Surgeons of Edinburgh. The Medical Faculty are not ashamed to confess, that until now they have been withheld, at the sacrifice of incurring much obloquy, from publicly expressing their own sentiments, by regard for a colleague. But although they have hitherto refrained from declaring, they have always entertained, and continue to entertain, the strongest disapprobation of the delusions of homœopathy; and they feel they would ill discharge their duty to the University, the medical profession, and the public, if they were deterred any longer by private considerations from making this opinion known, or from declaring that they will counteract by every possible means the risk arising to the University from the present state of the Chair of General Pathology."

In order to prevent all cavil or equivocation on the part of the homœopathic friends of the Professor of General Pathology, to whom we presume from past experience the phrase "unanimously" must prove peculiarly unpalatable, we beg to add, that these resolutions were passed at a large meeting of the Faculty, without a dissentient voice; that we have no reason to doubt that the absent members, one of whom was confined by ill health, and the others were at a distance from Edinburgh, heartily concur in them; but that if any of these gentlemen shall tender a dissent to the Faculty, or a denial to ourselves, we shall believe they differ from their colleagues,—but not until then.

RESOLUTIONS OF PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

At the nineteenth anniversary meeting of this numerous and influential body, held at Brighton, on 14th August 1851, and attended by upwards of 300 members from all parts of England, the following admirable resolutions, relative to the most popular medical delusion of the day, were unanimously adopted. Although these resolutions appear in our advertising pages, we have great pleasure in according them a more permanent record in the body of our Journal. The step just taken by the Provincial Association entitles them to the thanks of the Medical Profession, and will, we trust, prove a heavy blow and great discouragement to quackery:—

"1. That it is the opinion of this Association, that homœopathy, as propounded by Hahnemann and practised by his followers, is so utterly opposed to science and common sense, as well as so completely at variance with the experience of the medical profession, that it ought to be in no way or degree practised or countenanced by any regularly-educated medical practitioner.

"2. That homœopathic practitioners, through the press, the platform, and the pulpit, have endeavoured to heap contempt upon the practice of medicine and surgery, as followed by members of this Association and by the profession at large.

"3. That, for these reasons, it is derogatory to the honour of members of this Association to hold any kind of professional intercourse with homœopathic practitioners.

"4. That there are three classes of practitioners who ought not to be members of this Association, viz.,—1st, Real homœopathic practitioners; 2d, Those who practise homœopathy in combination with other systems of treatment; and 3d, Those who, under various pretences, meet in consultation, or hold professional intercourse, with those who practise homœopathy.

"5. That a committee of seven be appointed to frame laws in accordance with these resolutions, to be submitted to the next annual meeting of the Association.

"6. That the thanks of the Association are eminently due, and are hereby

given, to the Presidents and Fellows of the Royal Colleges of Physicians and Surgeons of Edinburgh, for their determined stand against homœopathic delusions and impostures.

"7. That the thanks of the Association are also due, and are hereby given, to the Universities of Edinburgh and St. Andrews for their resolution to refuse their diplomas to practitioners of homœopathy; but the Association feels imperatively called on to express its disapproval of any school of medicine which retains among its teachers any one who holds homœopathic opinions.

"That these resolutions be printed, and transmitted to all the medical licensing bodies and medical schools in the United Kingdom; and that they likewise be inserted in the *Times* newspaper, the *Morning Post*, the *North British Advertiser*, *Saunders's News Letter*, all the British and Irish medical periodicals, and in such other journals as the Council may sanction upon the recommendation of the Branch Associations."

MR LIZARS AND THE "MONTHLY JOURNAL."

We have been called upon by Mr Lizars for explanation and retractation of certain passages in an article of our Medical News of last month, which he designates as calumnious. In so far as retractation is concerned, we have nothing to say; but in explanation, we are most happy to remove any misapprehension which may have led to the idea that the expressions to which we have given publicity were of a calumnious nature. We should indeed feel deep regret if the pages of this Journal were ever justly chargeable with the admission of matter so objectionable, and we trust that the following statement will satisfy our readers that there is no ground for such charge on the present occasion.

Mr Syme having introduced into the practice of surgery what he believed to be an improvement in the treatment of obstinate stricture of the urethra, was, without any provocation, assailed by Mr Lizars with a succession of statements calculated to affect injuriously both his practice and character,—inasmuch as they flatly denied the truth of his statements as to matters of fact, and attributed the most disastrous effects to his operations, instead of the benefit which he had alleged to have resulted from them. The duty of Mr Syme as a teacher of surgery required him either to answer these charges, or to explain his reason for not doing so, and he chose the latter course. Twice before he had felt it necessary to prosecute Mr Lizars for defamatory statements respecting his operations. On the first of these occasions, the verdict of a jury was given against Mr Lizars, with costs and damages; and on the second, Mr Lizars rendered further proceedings unnecessary, by addressing to Mr Syme a letter which admitted that there was no foundation for the injurious statements in question, and paying the expenses which had been incurred in taking steps for obtaining redress. Upon these grounds, Mr Syme conceived that he was exonerated from replying to the new series of charges brought against him by Mr Lizars, and published an intimation to this effect. Since then, the Editor of the "Lancet," regarding the newspaper advertisements of Mr Lizars as opposed to the feeling, and inconsistent with the usage, of the profession, publicly called upon Mr Syme to abstain from noticing any statements of one who had chosen to place himself in such a position. More lately, the "Medico-Chirurgical Review" has declared that it will not in future notice any statements proceeding from Mr Lizars, or his coadjutor Dr Muller. When, therefore, the "London Medical Gazette" thought proper to acquaint its readers that a "fierce paper war" had arisen between Mr Syme and Mr Lizars, Mr Syme conceived it necessary not only to deny the imputation, but to state his reason for declining any such controversy.

The medical profession is a very numerous body, composed of materials the most heterogeneous, and influenced by motives the most conflicting. If therefore all the members were left to pursue at their own discretion the

line of conduct which inclination or expediency might in each instance suggest, it is plain that confusion and disorganisation would speedily ensue. But as there are no enacted laws for the moral government of the medical profession, and no judge to apply such laws even if they existed, the only control to which we are subject is that of professional opinion. According to this rule, certain lines of conduct are considered more or less reprehensible; and in a corresponding degree, pursued at the expense of forfeiting the respect and courtesy which are due from one member towards another. For instance, if a practitioner chooses to advertise in the newspapers that his own treatment of a particular disease is beneficial, and that of another practitioner hurtful, he cannot expect to derive the advantage contemplated from this proceeding, and at the same time be treated with fraternal regard and deference by his brethren, who have not so transgressed the bounds of professional decorum. Any member, who feels himself bound by a stricter principle of conduct, is quite entitled to decline intercourse, either of a public or private nature, with another who thus conducts himself, if he thinks proper to do so. But still the only ground of procedure is opinion; and if it should appear to be not sufficiently well founded, no harm will accrue to the party concerned. Now, this is precisely the state of matters between Mr Lizars and Mr Syme. The former pursued an unusual course in regard to his advertisements, and Mr Syme expressed the "opinion" that he had placed himself in a position which excused Mr Syme from treating his statements with the deference to which they would otherwise have been entitled. If Mr Syme did Mr Lizars injustice in forming or expressing this opinion, Mr Lizars may look with confidence to the opinion of his profession for redress.

The second passage of which Mr Lizars complains, is that denying the truth of his and Dr Müller's allegations in respect to the disastrous effects attributed by them to Mr Syme's operations for the remedy of stricture. In this passage there is no charge of wilful misstatement of the truth, but simply a declaration that the injurious statements, whencesoever derived, are not consistent with fact; and we have yet to learn that the subject of a free country is not at liberty to deny the truth of statements directly impugning his professional character and moral rectitude. We are quite sure that the intention of Mr Syme was not to asperse the character of Mr Lizars, but merely to vindicate his own good faith and accuracy.

CHANGE OF OUR LONDON PUBLISHER.

The readers of the "London Medical Times," must for many months past have observed the tone of criticism with which it has treated the Conductors of the "Monthly Journal," and, on turning to our pages, may have been puzzled to discover why we neglected to acknowledge or reciprocate it. Our excuse is simply, that by participating in such incivilities, we should have disgusted our readers, and lowered the character of our Journal; and we have been vain enough to suppose that such efforts to asperse our motives required no refutation on our part, but would signally defeat themselves in the end, in consequence of the very unscrupulous malignity which seemed to direct them.

But the question frequently suggested itself,—How far it was right for our London publisher to sanction systematic attempts to damage and abuse us, carried on by a journal of which he is *proprietor*, and not *publisher* merely? The "Medical Times," be it observed, is conducted by an anonymous Editor, under the protection of Mr Churchill, who admits his own responsibility for the literary doings of this subordinate. It was tolerably clear that Mr Churchill was acting improperly in suffering anything to appear in the "Medical Times" which he had the means of proving to be false; and it was certain that a portion of the medical public would accept as highly credible any statements regarding the "Monthly Journal" and its Conductors, emanating from Princes Street, Soho, where our own London agency was conducted, and where the aforesaid editor is understood to do his work.

An offensive article was published in the "Medical Times," on the 10th May, in which the "existence" of the "Monthly Journal" was characterised as "sickly," and the "obscurity of its pages" alluded to, while the average hebdomadal amount of abuse was distributed amongst its Conductors. A letter was addressed to Mr Churchill, by the Editor of the Edinburgh Journal, demanding an apology for an unfounded attack upon the commercial interests of the Journal, and upon the characters of the gentlemen associated in its direction. An assurance was also sought, that similar cause of offence should not be permitted to recur; the alternative being the immediate transference of our London agency to other hands.

A correspondence followed, in which Mr Churchill admitted his *legal responsibility* for what had appeared in the "Medical Times,"—conveyed the assurance of his anxious desire "that truth, regulated by courtesy, should be observed by all the gentlemen with whom he" was "connected,"—corrected the erroneous statements as to the "sickly existence" of the "Monthly Journal,"—testified that "its sale" had "greatly increased during the last twelve months," and was "still progressing,"—"in fact, that it" had "gained a successful position in medical literature;"—but protested against any attempt "to fix upon him," as a *publisher*, "the animus of articles published through his agency."

Mr Churchill had no reason to complain of any such attempt. He was charged, as the London publisher of *this* Journal, with compromising its commercial interests and the characters of its Conductors, by permitting them to be assailed in an unscrupulous manner by another periodical, anonymously conducted, of which he was *proprietor* and *master*, and for the contents of which he acknowledges himself, in a letter now before us, as legally responsible.

The character of the attacks complained of is illustrated by the following ungracious apology, or rather confession of error, by the "Medical Times" of June 7th, p. 622:—

"*The 'Edinburgh Monthly Journal.'*

"In our impression of the 10th of May, we stated that the *Edinburgh Monthly Journal* 'was drawing on a sickly existence.' It has since been suggested to us, that the sale of that periodical had improved. We have, therefore, felt it our duty to make inquiries on the subject; and we learn from the London publisher, that the sale of the *Edinburgh Monthly Journal* has considerably increased during the last twelvemonth."

A scandalous and personal attack, which, shortly after this retractation, was made by Mr Churchill's periodical upon one of the conductors of the Edinburgh Journal, induced us to place our agency in the hands of Messrs Simpkin and Marshall, and thus to remove Mr Churchill from the embarrassing and equivocal position which he voluntarily occupied.

We should not have deemed it necessary to trouble our readers with this long explanation, had it not been forced upon us by the interference of the "Medical Times" of the 9th and 16th August 1851. But, after this statement, the cause of any future scurrility on the part of the "Medical Times" will be understood, as well as the reason of the silence with which we shall treat it.

ROYAL INFIRMARY CLERKSHIPS.

An advertisement in our present Number contains an abstract of resolutions adopted by the Managers of the Infirmary in regard to the qualifications and election of clerks to the hospital. Those acquainted with the old regulations will fully appreciate the value of the changes now introduced with the full concurrence of the entire medical and surgical staff of the institution.

The present attempt to raise the qualifications of candidates for these import-

ant offices, will, we feel assured, be productive of the best results. The physicians and surgeons will of course endeavour to ascertain that each gentleman whom they recommend for election, and for whose conduct they engage to become responsible, is truly competent and trustworthy; and there will be an increase of the confidence between the clerks and their superior officers, which is so essential for their mutual comfort, and for the welfare of the sick committed to their care. The liberal terms now offered by the managers, enable a certain number of well-qualified students to obtain the office of medical or surgical clerk, and to live in the house free of any fee whatever; they afford to the diligent an opportunity of professional improvement such as can hardly be found in any other hospital; and will, in our opinion, add to the efficiency of the clinical school.

As vacancies in the clerkships, both medical and surgical, will occur before the commencement of the winter session, we beg to point out to gentlemen who have recently completed their medical studies, the extreme value of such appointments as a preparation for the emergencies of independent practice, and the expediency of enrolling their names, without delay, in the list of candidates.

MEDICAL DIRECTORY FOR SCOTLAND.

The Editor of the "London and Provincial Medical Directory" has announced the intention of publishing, on the 1st of January next, a Medical Directory for Scotland; and, in pursuance of this object, circulars have been transmitted extensively to medical practitioners, requesting them to supply information, and fill up blank schedules for the preparation of a correct list. We cannot too warmly recommend this undertaking to the aid and support of our readers and the profession at large. The English Directory has been admirably compiled, and supplies an extraordinary fund of useful information respecting the position and literary labours of practitioners of all classes in England. The evident utility of such a publication for a variety of purposes has created a great anxiety to see a similar work undertaken for the medical profession in Scotland. We have no doubt that the experience acquired by the editor in his previous volume will enable him to do ample justice to the new one. Those practitioners who have not yet been applied to by him ought immediately to obtain a schedule by addressing "The Editor of the Medical Directory for Scotland, 4, Adam Street, Adelphi, London," and return it filled up without delay,—otherwise their names will not appear in the list. It is easy to see that, with all his anxiety to leave no regular practitioner unapplied to, many omissions may occur on the part of the editor.

PUBLICATIONS RECEIVED.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Fasciculus IX. London: Churchill. 1851.

Letter to the Right Honourable the Earl of Shaftesbury, containing suggestions as to the Expediency of Submitting certain Branches of Trade and Manufactures to Government Medical Inspection. By J. B. Harrison, M.R.C.S., &c. Manchester, 1851.

Note sur les Matières Albuminoides. Par Melsens. (Bulletin de l'Académie Royale Belgique.) Bruxelles, 1851.

Twenty-First Annual Report of the Belfast District Asylum for the Insane. Belfast, 1851.

Letter to R. K. Greville, LL.D., being an Answer to certain Statements contained in a Pamphlet entitled "Singular Specimens of the Edinburgh Practice of Criticism, by John Joseph Griffin." By John Hutton Balfour, M.D., Professor of Medicine and Botany in the University of Edinburgh. Edinburgh: A. & C. Black, 1851.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Why is a true Corpus Luteum formed (normally) only when Pregnancy occurs?* By ALEXANDER HARVEY, M.D., Physician to the Aberdeen Royal Infirmary, and Lecturer on the Practice of Medicine in the University and King's College of Aberdeen.

BEFORE proceeding to consider the answer to the question placed at the head of this paper, the five propositions following may be premised :—

1st, That the assumption on which the question proceeds is a valid one,—at least to this extent, that (in the natural state) it is only when pregnancy occurs that the corpus luteum attains the full size, and passes through the whole series of changes described by Dr Montgomery and others as characteristic of the *true*, in contradistinction to the *false*, corpus luteum.

2d, That at each monthly period in the human female, a mature ovum makes its escape from the ovary; and that its becoming impregnated or remaining unimpregnated depends simply, speaking generally, on its meeting, or failing to meet, with the seminal fluid at the proper time or place.

3d, That, whether the ovum be thus impregnated or not, the *elements* of a true corpus luteum exist in the ruptured ovisac of *every* such ovum,—viz., a deposit of organised and still farther organisable matter.

4th, That this matter having, on the escape of the ovum, already fulfilled its destined office, its organisable power is *feeble*, and its tendency towards *atrophy and decay* greater than that towards *farther growth and development;* and, this being so, that its development into a perfect ("true") corpus luteum is *contingent* on certain accessary circumstances *extrinsic* to itself.

5th, That during certain states of the system of which pregnancy and lactation are notable examples, there is, along with suppression

of the menses, a suspension of the maturation of ova, and of the escape of mature ova from the ovaries,—these processes, however, being resumed, concurrently with the re-appearance of the menses, on the subsidence of those states of the system.[1]

Of these propositions the first, at least in the qualified form in which it is advanced, will not now be disputed by any one; and the second and third will be allowed by every one acquainted with the present state of our knowledge on the subject, to be at least in the highest degree probable. The fourth, on which the answer to the question proposed more immediately turns, seems to be a fair inference from proposition third, and from the known history of the matter there referred to; and the fifth, which bears on certain objections that may be urged to the sufficiency of the answer to be given, also appears to be a legitimate inference from known and admitted facts.

It may next be observed, that, while much has been written on the distinctive characters of *true* and *false* corpora lutea, and on the nature of these bodies generally, no writer (so far as the author can discover), with the single exception of Dr Carpenter, and that only in his "Manual" of Physiology, and there very briefly,—no writer has ever proposed, much less discussed, the question which forms the subject of this paper. Müller, at least, and Wagner, Carpenter in his "Principles of Human Physiology," Kirkes, Alison, Allen Thomson, Mayo, Bostock, and Dunglison, make no reference to it; and even Dr Montgomery and Dr Baly, who have written largely (and the former very expressly) on the subject of the corpus luteum, pass it over in silence. That the question should not have presented itself to the minds of certain of these physiologists need not be matter of surprise; but it does seem to the author somewhat surprising that it should not have suggested itself to those in more recent times, who acknowledge as physiological truths the second and third of the propositions advanced above. And yet it will perhaps be admitted on a little reflection, not only that it is a question naturally arising out of the propositions just referred to, taken in connection with the known history of the matter spoken of

---

[1] If this proposition be correct, a more definite, and probably a more accurate, idea may be formed both of the pathology of amenorrhœa and of the therapeutics of emmenagogues. Excluding certain special and rare cases of that disease, and having regard to the ordinary and much more frequent cases, it may be maintained that amenorrhœa is essentially dependent on morbid *constitutional* states, which prevent the maturation and discharge of ova; and that emmenagogues owe their efficacy, not to any specific action they may exert on the uterus or ovaries, but to their correcting those constitutional states,—on the subsidence of which states, the maturation of ova, and, concurrently with this, the process of menstruation, are *naturally* or *spontaneously* re-established.

in proposition third, but that a satisfactory answer to it would probably throw light on other and important but controverted questions relative to the corpus luteum, or at least serve to give greater precision to our estimate of the value of that body as an evidence of pregnancy.

Now, assuming that the propositions which have been laid down are all of them correct expressions of matters of fact, there seems but one way of accounting for a "genuine" or "true" corpus luteum being formed when pregnancy occurs, and not being formed when this does not happen; and the true account of the matter appears to be this:—

(1.) When a mature ovum is impregnated, provision is forthwith made for its being retained in the uterus; and both the ovum and the uterus becoming the seat of peculiarly active vital processes—which are to extend over many months—an active, sustained, and prolonged *determination of blood* takes place towards the uterus and its appendages. The ruptured *ovisac* participates in that determination and in the tendency to growth given by it; the *organisable* deposit which it contains is thereby placed in circumstances favourable to the exercise of its own inherent power of growth; and thus circumstanced it grows, and is developed into the full-sized, ripe, or "true" corpus luteum.

It is no objection to this view, that the body in question may attain its full size within the second or third month of pregnancy, then remain stationary for a time, and after that begin to atrophy (all which it seems to do) notwithstanding that the external conditions of its growth continue the while in full activity and unchanged—may, perhaps, increase with the advance of pregnancy. It seems sufficient to observe, that, indispensable as these conditions are to its development, it may have and probably has (in common with almost every organisable substance) its *own* law of growth, and that this *law* determines the extent of its development, and the several changes, whether of increase or decrease, which it passes through. And thus, although the circumstances favouring its continued growth obtain, no further increase goes on after it has reached a certain size. It may be remarked, however, as showing that these are still not without their influence on its condition (and, indeed, it may be added, as indirectly but very strongly supporting the view here taken of the cause of formation of the "true" corpus luteum), that it is not until *after* delivery, when all the vital processes connected with the (associated) ovum are completed, and the extraordinary determination of blood thereby created in all parts of the generative organs ceases, that the corpus luteum begins *rapidly to disappear;* that when inflammation attacks the uterus during the *post-partum* state, and the determination thereto is *renewed*, the size and *vascularity* of the corpus luteum *are preternaturally augmented* (see Montgomery, "Signs and Symptoms of

Pregnancy," pp. 225, 227, and Plate XI., Figs. 5 and 6); and further, and very particularly, that on the occasion of an *abortion*, when the vital processes are cut short, and the requisite determination ceases much earlier than usual, the decay of the corpus luteum is proportionally *early* (Montgomery, op. cit., p. 227). In proof and illustration of this last observation, it may be stated, that, supposing two women to conceive on the first day of January, and to die on the first day of November thereafter, but the one to have *aborted* on the 1st of April, and the other to have been *delivered* on the 1st of October,—there would be found in the ovary of the former slight traces only, if any, of the corpus luteum; in the ovary of the latter, very manifest traces of it.

(2.) On the other hand, when the ovum is not impregnated, it becomes blighted, and is thrown off from the generative organs. No cause of preternatural determination of blood to the uterus existing, no such determination takes place; and the organisable deposit within the ruptured ovisac, having no great inherent power of growth (Prop. 4), and not being placed in circumstances favourable to the exercise of this, either fails to grow at all, or grows only (although very rarely) to the extent characteristic of a "false" corpus luteum.

It might be supposed, indeed, in this case, that the periodic monthly excitement of the ovary would attract sufficient supplies of blood to enable that deposit to grow. This excitement, however, is essentially *transient*, continuing only for few days at each time—a period much too short for the deposit to make any tolerable progress towards development, if, indeed, its power to do so be not *lost* in the *interval* that elapses between the escape of the ovum and the next monthly renewal of the excitement.

The answer now given to the question proposed agrees with the statement made by Dr Carpenter as to the cause of the difference in the changes which the deposit in question undergoes, according as the ovum is or is not impregnated. "The escape of the ovule from the ovarium involves processes which are essentially the same, whether it be impregnated or not; but the subsequent changes differ in the two cases, so that the corpus luteum which accompanies the pregnant state is a much more highly organised body than that which is found in the ovary of the unimpregnated female. *This difference may be due, in part, to the absence, in the latter case, of that special determination of blood to the genital organs which takes place in the former.*"—("Manual of Physiology," p. 462.) Dr Carpenter would perhaps be willing to cancel the qualification "*in part*," or at least to substitute for it the word *probably*,—the expression "in part" implying that *another*, although *unknown*, cause, and *because* unknown possibly (on the principle of "omne ignotum pro

magnifico") a more *influential* cause, may be concerned in the production of the difference.[1]

On the principles now stated, it might be expected (seeing that a mature ovum is thrown off every month) that *any* condition of the uterus and its appendages, *besides* pregnancy, which can produce and keep up an active determination of blood to these parts, should, equally with pregnancy, be attended with the formation of a true corpus luteum. It may be remarked, however, that it is but rarely that the conditions requisite for such an occurrence can reasonably be expected to obtain. For, in the first place, *most* states of the uterus and its appendages tending to create such a determination are probably attended, even from the outset, with a *suspension* of the maturation of ova and of the menstrual secretion (Prop. 5); and, in the second place, *few* morbid conditions of these parts are of so active a character, or accompanied by so vigorous, and sustained, and complete a determination of blood, as are the processes attendant on pregnancy. Therefore, while the presence of a true corpus luteum in the ovary of a virgin, who had died of such disease of the uterus or ovarium as had been accompanied during life with decided and prolonged determination towards these organs, would be in keeping with the view here given of the formation of that body, its *absence* could not be regarded as a sufficient objection to the soundness of that view; and accordingly, while the reason will appear for the introduction of the word "normally" into the question now discussed, the occasions are probably very few in which our estimate of the value of the presence of a true corpus luteum, in relation to the question of pregnancy, could be at all affected. It is clear that the circumstances under which a well-developed or true corpus luteum could be formed, independently of pregnancy, must be such as to produce a condition of the uterus and its appendages, and of the circulation through them, corresponding more or less exactly to that attendant on pregnancy; and it may be confidently asserted, that such is not the ordinary history of any known disease of these parts,—*e. g.*, of ovarian disease, malignant disease, fibrous or other tumours, &c.,—while, as already observed, most diseases of these organs involve an *early arrest* of the menstrual discharge and of the maturation of ova.

Although the formation of a true corpus luteum, as consequent on the pregnant state, has here been referred to the *determination of blood* attendant on that state, it would perhaps be physiologically

---

[1] This substantial coincidence of sentiment is the more satisfactory to the author, that his own opinion was formed, and expressed in his lectures on physiology, before the publication of Dr Carpenter's Manual.

more accurate to regard it as an ulterior or incidental result of the *increased vital action* going on in the uterus and its contents, and by which the determination in question is itself maintained. That the ovaries, although they have no part to play in that vital action, should be indirectly influenced by it, and participate in the excitement existing in their immediate vicinity, would be no ways more remarkable than the fact of the mammæ then doing so,—organs remotely situate from the uterus, and having during the pregnant state nothing to do with the processes then in operation. Supposing this to be the more accurate explanation of the phenomenon, it may suffice to have thus briefly adverted to it.

ABERDEEN, September 1, 1851.

ARTICLE II. — *Case of Confluent Small-Pox occurring after a Third Vaccination; with Remarks.* By ALEXANDER P. STEWART, M.D., Assistant Physician of the Middlesex Hospital.

(*Read before the Royal Medical and Chirurgical Society, 25th February* 1851.)

ON the 19th of October 1850, Mr Henry John Brown, of Wilmington Square, requested me to visit a gentleman, who, notwithstanding his having been three times vaccinated, seemed certainly to be labouring under an attack of confluent small-pox. Having called in the course of the afternoon, I obtained the following history of the case:—

Mr P——, aged nearly 25 years, had enjoyed good health from childhood, had lived regularly, and was of moderately stout habit. Having been five years apprenticed to a gentleman in extensive general practice in a watering-place on the south-west coast of England, he came to town on the 13th of October, to begin his medical studies in the school attached to one of the Metropolitan Hospitals. He was first vaccinated, when six months old, most successfully, and the cicatrix is very perfect. Having always had some apprehension—not amounting to actual fear—of variolous infection, and being called, about that time, in the discharge of his duties, to visit variolous patients, he had himself re-vaccinated in the month of July 1849, but this time unsuccessfully. Dissatisfied with the result of this experiment, he had the process repeated in May 1850, when it was attended, and followed for five weeks, by much irritative inflammation and swelling of the arm; but the lymph taken from the vesicle did not communicate the disease. The cicatrix has nearly disappeared. Having thus, at the expense of much suffering, done what he could to secure an immunity from the object of his aversion, he continued, during the four following months, to minister to the

relief of the victims of the epidemic, which began its ravages in the town and neighbourhood in the month of April, and still held on its course with unabated intensity at his departure on the 13th of October.

On the evening of that day, as already stated (having called, immediately before starting, to take leave of a variolous patient who was almost convalescent), he arrived in London, feeling but not looking well. This circumstance, noticed by others during the two following days, he referred to the fatigue he had undergone before leaving Devonshire, and his subsequent journey to London. During the night of the 15th, however, the period of incubation ceased, and high fever, not preceded by chills, but accompanied by watchfulness and much thirst, ushered in the train of morbid symptoms. On the 16th these increased in intensity, and were followed (I mention them in the order of their succession) by headache, extreme languor, general weakness and malaise, pain in loins, sickness, and obstinate bilious vomiting. The bowels being much confined, repeated and powerful doses of purgatives, mercurial and others, were given, without effect till the morning of the 17th, when the free evacuation of the bowels, and the appearance of a closely packed eruption of minute papulæ, were attended with great relief.

No material change took place during that and the next day, beyond the gradual development of the papulæ, which, when I first saw him, were becoming vesicular everywhere, but especially on the exposed portions of the skin. The pulse was about 120, and moderately full, but soft and compressible. The face and hands, especially the former, were highly swollen, red and painful, and so thickly covered with the variolous eruption, that scarcely any portion of the skin was visible. The papulæ were likewise very freely sprinkled over the neck, the whole trunk, and the extremities, especially on the soles of the feet. On the tongue, which was gray and loaded, and on the fauces, were a multitude of papulæ, which caused sore throat and considerable difficulty of swallowing. The eyes were suffused and watering, but no pain was complained of. There was still present considerable epigastric tenderness, irritability of stomach, and tendency to nausea, but the bowels were tolerably free.

The fauces were ordered to be mopped over with a gr. xxx. solution of the nitrate of silver; and an effervescing draught, with excess of alkali, and mn. iv. of dilute hydrocyanic acid, to be taken every three hours. The face and hands were painted over with non-contractile collodion.

The forenoon of the 20th found him, notwithstanding a very restless night, in some respects better. The pulse, instead of becoming more rapid, had fallen considerably, he informed us, shortly after the first effervescing draught, and was, at the time of our visit, not quite 100; and the nausea had entirely left him. The

bowels continued moderately open, and the tongue was rather cleaner. The nitrate of silver solution was freely applied to the throat, and, on repeating my visit in the evening, I found that the difficulty of swallowing had somewhat abated; but the restlessness and wakefulness, which had been steadily on the increase since the 15th, now took on the form of delirious excitement, which seemed rather heightened than soothed by the administration of two pills, consisting of gr. ij. of camphor, gr. i. of calomel, and gr. iv. of hyoscyamus.—A seidlitz draught to be taken in the morning. The draughts to be continued.

During the thirty-six following hours there was no material change of symptoms; the vesicles rapidly enlarging and maturating, the pulse scarcely ever rising above 100, and, though soft, continuing pretty full; and the delirium recurring nightly, with trifling increase of intensity. It was worthy of remark that, while the swelling of the face caused great uneasiness and considerable oppression, owing to the stuffing up of the nostrils and some suffusion of the conjunctivæ, there was little or none of that intense suffering so often complained of when the soles of the feet are, as was here the case, densely studded with variolous vesicles. In other parts of the body, the vesicles appeared more superficial than is usual when they are so crowded and confluent.

The draughts were still continued, and the collodion was applied to those parts where the increasing distension had ruptured the coating previously laid on. Considerable relief of the sore throat and difficulty of swallowing followed the application, morning and evening, of the caustic solution.

On the 22d, as the pulse showed signs of failing, and the vesicles on the body looked suspiciously flattened, the hydrocyanic draughts were exchanged for gr. iiss. of sesqui-carbonate of ammonia, in ℥ss. of camphor mixture, every second hour; and on the 23d, the pulse had so completely recovered its strength, and the general symptoms were so favourable, that we had recourse to a strong dose of calomel and jalap, for the purpose of acting on the bowels, which had become rather obstinately confined. After the free operation of this purgative, the bowels were easily kept open by a daily dose of ʒss. of sulphate of magnesia; and after the night of the 22d, there was no return of the delirium. On the 24th—*i.e.*, on the eleventh or twelfth day of the disease, and the seventh of the eruption—the latter was plainly at its height, as on several parts not only of the face and neck, but of the body, the vesicles (for they never became fairly pustular) were beginning to blacken. Apprehensive of the weakening effect of the extensive drain from the whole cutaneous surface, we allowed some wine in addition to the ammonia. This, however, was more as a precaution than from necessity, as the pulse had flagged very slightly. The presence of a considerable amount of conjunctival inflammation, now rendered necessary the use of the

gr. iv. solution of nitrate of silver, which was attended with marked benefit.

On the two succeeding days we found, to our surprise, that, instead of terminating in a noisome and exhausting discharge from the whole surface, the vesicles were drying up everywhere, and in some places scaling off, with scarcely any discharge. From this date convalescence went on rapidly, and, though interrupted by the appearance of some painful furunculi, of considerable size, on various parts of the body, was complete in the second week of December.

To this account of the case, I venture to subjoin a few interesting particulars, furnished by my patient, regarding the town where he lived and its neighbourhood, in reference to variolous inoculation, vaccination, and the epidemic which was raging there when he left it for the metropolis. It is in one of those districts, of which, I believe, there are not a few in England, where an old, but strong and deep-rooted prejudice in favour of inoculation, and against the great discovery of Jenner, reigns almost universally among the working classes, and finds the means of gratifying itself in defiance of the law. For many years past, what may be fitly enough called "the inoculation epidemic" has broken out from time to time among them; and so cleverly do they manage the process, that though a fund was lately obtained by subscription for the express purpose of bringing the law to bear on its transgressors, not a scrap of evidence could be procured to bring home the offence to any one of the hundreds who were infringing the statute. So normal, indeed, had this state of law-breaking become, that those charged with carrying out the provisions of the vaccination act would seem to have given up all thoughts of struggling against the tide of popular prejudice, and the practice of vaccination had been for a number of years much neglected; but owing to the energetic measures of the present union medical officer, a very large number of children have been vaccinated during the last three years; and during the first four years of Mr P——'s apprenticeship, no case, save one, of small-pox occurred in the town or neighbourhood. At length, however, in April of last year, a sweep-boy, who had been to another town at no great distance, to exercise his calling, brought the disease back with him, in a not very malignant form, considering that he had not been vaccinated. The father and mother, having had the small-pox, escaped the disease, but all the junior members of the family, being unvaccinated, took it, and one died. One of them having gone, while labouring under the disease, to a lace-school, communicated it to some of the children. From this time it became general, and continued so till October. The epidemic was beyond a doubt (though legal proof could not be had) kept up and spread about among the people, even more by inoculation than by contagion. Very few of those who had been vaccinated during the three pre-

ceding years took the small-pox, but in those who took it there did not seem to be much difference in the symptoms from those who had not been vaccinated. It must be borne in mind that my friend was inoculated both times with the very vaccine lymph that was communicating the cow-pock most successfully to the *un*vaccinated children.

I cannot refrain from preserving one or two more gleanings of this epidemic,—gleanings that excite a longing for a more complete account of an outbreak, which appears more than ordinarily fitted to throw light on some of the vexed questions of the day. The following, for instance, is a striking enough commentary on the practice of variolous inoculation, and its vaunted efficacy as an almost unfailing protective against the casual disease. A boy, two years old, who had been inoculated, died of the effects of the disease thus imparted to him. Both father and mother had been inoculated when young; the latter first took the disease from her child in a rather severe form, and, being far advanced in pregnancy, was confined, during her illness, of a child which was born with the variolous vesicles upon it. The husband next had it, but mildly.

The next case offers evidence of a somewhat different kind, though scarcely less instructive. One boy, of a family of five children, happened to be out of the way when the medical officer called to vaccinate the others, in all of whom the operation was most successful. Some time afterwards he, the only unprotected one, contracted the disease, which he communicated to his mother immediately after her confinement, she having been inoculated in childhood. The children who had been vaccinated escaped the disease. Such are a few *out of dozens of instances*, where inoculation failed to protect those who had been subjected to it from attacks of the casual disease. I am further informed, that a lady of title, residing in the infected district, has lately had a second attack of natural small-pox.

Exactly analogous to these were the results of my own, and my friend and colleague Dr Andrew Anderson's experience, on a larger scale, many years ago, in the Fever Hospital at Glasgow. The first question put to a patient labouring under confluent small-pox was, "Where do you come from?" In the great majority of instances the reply was, "From the Highlands." There we find prevailing the same veneration for things as they were seventy years ago, as in the districts of England to which we have referred,—with this difference, that the inhabitants, as is their wont, generally render a negative obedience to the law by doing nothing. They shun both inoculation and vaccination. Accordingly, a large proportion of them, we found, had never been vaccinated at all, and had the disease in its most loathsome and malignant form. No history could be obtained in a large proportion (considerably more than a third of the entire number) of the cases admitted. But while this defect leads me to mark them as "doubtful," the absence of any evidence

of vaccination would almost seem to warrant their being classed among the *un*vaccinated.

The following is a summary of the number of variolous patients admitted from 1st November 1836 to 1st November 1838:—

	Vaccinated.	Doubtful.	Unvaccinated.	Total.
Males,.....	16	24	14	54
Females,.....	15	28	29	72
	31	52	43	126
Deaths,..... Males,	1	11	6	18
Deaths,..... Females,	0	6	9	15
	1	17	15	33
	1 in 31, or 3·2 per cent.	1 in 3·06, or 32·7 per cent.	1 in 2·86, or 34·8 per cent.	1 in 3·81, or 26·2 per cent.

For the materials from which this summary is constructed, I am indebted to the kindness of Dr Andrew Anderson, who extracted them for me from the journals of the Fever Hospital.

The general average of the mortality, 26·2 per cent., or one death in 3·81 cases, is startling enough; but it appears *moderate* when compared with 32·7 per cent., or one death in every *three* of the "doubtful" cases, which I believe to have been *un*vaccinated, with a sprinkling of *imperfectly* vaccinated patients; and we reckon it positively *small* alongside of 34·8 (nearly 35) per cent., or one death in 2·86 of the unvaccinated. From these enormous rates of mortality, it is a relief to return to the contrast presented by the vaccinated cases, among which the proportions are pretty nearly reversed; so that, instead of one death in every three cases, or more than 30 per cent., we find one death in thirty-one cases, or only 3·2 per cent. I had hoped to have obtained data considerably more extensive than the above, but the statement, so far as it goes, is very instructive.

In a series of inquiries which I instituted, in January 1845, in that district of the parish of St Pancras which it was my lot to visit, during five years, as a dispensary physician,—and the results of which I endeavoured, not very successfully, to communicate in the course of the discussion on Dr Gregory's very interesting paper, the same general principle was fully brought out. Those members of families who were first affected with the small-pox, during the very fatal epidemic then raging, were almost invariably unvaccinated. But the fact which made the deepest impression on my own mind was, that those who had been once successfully vaccinated, *both parents and children*, though exposed, in their own unventilated dwellings, to the concentrated infection of the disease in its worst forms (and some of the cases were among the worst I had ever wit-

nessed), were proof against it; while those in whom the evidence of success was doubtful, were not proof against the milder forms, and very often took the disease from convalescent children whom they met casually in the open air.

Chomel's favourite maxim, when summing up the evidence of each clinical session on this, one of his favourite subjects, used to be, "You cannot fairly exact more from vaccination than from small-pox itself." To this conclusion, all that has been stated in the foregoing pages necessarily leads. If the case which has given rise to these remarks seems to militate against the efficacy of vaccination, we surely find sufficient evidence, in the multiplication of centres of infection, in death caused by its clandestine and illegal practice, and in the repeated instances of secondary seizures, which have been recorded, to put to silence those who would legalize the use of variolous inoculations.

But I deny that my patient's case bears so strongly against the protective power of vaccination as at first sight appears. For if we consider the extreme abundance of the eruption, we shall find, in the pulse constantly below 100 after the seventh day, in the very trifling and easily-removed signs of collapse, in the superficial nature of the vesicles, in the small amount of pus they contained, and in the entire absence of exhausting discharge and secondary fever, evidence plentiful enough to satisfy those who are not hopeless doubters, that vaccination alone prevented this case of small-pox from being much more formidable,—perhaps even fatal.

I am not prepared to give any opinion as to the effect of the collodion, in preventing the formation of deep scars on the face. They are certainly trifling, compared to what might have been expected from so profuse an eruption. But, while I do not deny that the local application may have had some share in this favourable result, I suspect, with Mr Brown, that, even had it not been used, the cicatrices would not have been very unsightly. One thing seems abundantly clear, that collodion does not possess the ectrotic properties of the mercurial plaster, which, though it must be most annoying to the patient, does certainly, as I have repeatedly had occasion to witness, save the comeliness of "the human face divine" by causing abortion of the vesicles.[1]

---

[1] The following case is interesting, as bearing on a question raised by the President and Dr Marshall Hall during the discussion on this paper,—I mean the identity or non-identity of varicella and modified small-pox. I was sent for, on the 10th of February 1851, to see a gentleman, whom I found labouring under considerable febrile excitement. On the 12th, he awoke much relieved, but with an eruption chiefly confined to the face, head, and hands. The presence of several large and fully formed vesicles on the brow a few hours after the first appearance of the papular rash, seemed to leave no doubt of its being a case of varicella. But I found, on careful inquiry, that he had been exposed, four days previously, to the contagion of small-pox, and I watched with much interest the progress of the papulæ from day to day. As they became vesicular, about a

ARTICLE III.—*On Leucocythemia, or White Cell Blood.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Theory of Physic and of Clinical Medicine in the University of Edinburgh. —(*Continued from Vol. XIII., p.* 105.)

IN the January, April, and August Numbers of the Journal for the present year, I have recorded thirty-one cases more or less illustrative of that morbid condition of the blood in which the colourless cells are increased in number. The most remarkable instance of the disorder was presented in the case of Tinlay (Case II.), who died on the 22d of last July, at Hull; and I have now to give the post-mortem appearances which were observed at his dissection, together with four other cases which bear upon this inquiry.

It may be remembered, that Tinlay was dismissed from the Royal Infirmary in August 1850, having for some time expressed great impatience to return to his friends in Hull; and the further progress of the case has been kindly communicated to me by Dr Sandwith of that town. For some time he was in the Infirmary there, when the same symptoms were observed, more or less severe, that have been previously noticed. Then he lived at his parents' hovel; and finally he went into the Union Workhouse, where he died at midnight, July 22, 1851. During all this time the abdominal swelling from the tumour continued; but he had no ascites; the diarrhœa was more or less urgent, the emaciation extreme, and the weakness gradually progressive up to the moment of dissolution.

Sectio Cadaveris, twelve hours after death, was performed by Mr West, surgeon to the Union Work-house. The following report of the appearances observed was communicated to me by Dr Sandwith :—

Extreme attenuation of the entire body.

Nothing unusual in the appearance or structure of the liver, save that the larger hepatic vessels were filled with small patches of coagulated black blood, side by side with immense flakes of a dirty white matter, like imperfectly formed lymph. The gall-bladder was filled with a glairy amber-coloured fluid, not much like bile. The liver weighed three pounds twelve ounces. On cutting into the organ, there oozed out from the smaller vessels a very thin watery blood.

The spleen weighed three pounds fourteen ounces. Its surface was of a sky-blue colour, and dappled with numerous specks like cicatrices, most of them very small, but there were two much larger ones near the summit. It adhered here and there by bands of lymph to the peritoneal lining of the abdomen, and also to the peritoneal covering of the intestines. On the under surface of the organ there was a very small globular lobule, enveloped in organized lymph. A cord, run lengthwise along both surfaces of the spleen, measured twenty-four and a half inches. A cord, similarly applied across the organ,

---

fourth or fifth part of the whole number exhibited the characteristic central depression of variola, while all the rest, even when examined with a powerful lens, remained perfectly conical throughout the whole of their short course. Convalescence was complete on the 17th.

measured eighteen inches. The structure of the spleen was very firm,—indeed very much like that of liver.

The omentum was utterly wasted. The mesenteric glands were most of them somewhat enlarged, pale, and with hard gritty matter in some of them. There were several enlarged glands at the caput coli in a state of congestion. Pale enlarged glands were also seen all along the sigmoid flexure of the colon. The kidneys were unusually shrunken and small, and weighed together six ounces. There was but little difference in the relative weight of each. Their structure was firm. The heart was small, very small, with a few patchy points on its surface. Its cavities contained a whitish imperfectly-formed lymph.

The lungs had so perfectly healthy an appearance that we did not think it necessary to cut into them.

There was no more effusion into either the pleural or peritoneal cavities than is quite natural. There was, however, an effusion, a little in excess, into the bag of the pericardium.

Microscopic Examination.—Next day I received from Dr Sandwith a portion of the spleen, about four inches long, three inches deep, and one inch thick; with a nodule, the size of a large bullet, at the hylus of the organ. In structure, it was found to be simply hypertrophied, the fusiform cells of the trabeculæ presenting their normal character and arrangement, and the cells of the pulp unusually abundant.

I also received portions of the clot taken from the heart, vena cava ascendens, and vena portæ. They presented exactly the same appearance as the clot in Case I., divided into a dead-white, purulent-looking layer, and a tolerably strong healthy-looking red one. The former, on microscopic examination, was almost wholly composed of colourless corpuscles, aggregated together by molecular fibres of fibrin; and the latter, though principally composed of coloured corpuscles, also contained many colourless ones.

Two of the enlarged mesenteric glands which were sent, on section yielded a copious juice, that contained the same cells as are represented, Jan. No., Fig. 4.

Remarks.—This boy was under medical observation altogether nearly eighteen months, the morbid condition of the blood existing during the whole of that time. After death the appearance and structure of the coagulated blood exactly resembled that presented in Case I., and there was the same hypertrophy of the spleen, and similar enlargement of the lymphatic glands, but not to so great an extent. The liver, however, was normal, and the lungs apparently healthy, but not cut into.

CASE XXXII.—*Leucocythemia detected after Death*; *Hypertrophy of Spleen; Cirrhosis of Liver; Bright's Disease.* (Communicated by Mr Drummond.)

The patient was a man of exceedingly intemperate habits, and aged about 45 years, who died in the Infirmary, under the care of Dr Andrew, 20th July 1851. He first entered the Infirmary, 22d December 1850, with Bright's disease, and considerable œdema of the feet and legs, soon followed by general anasarca. He was so far relieved, however, by treatment, that he was dismissed 7th April 1851. He was again admitted 11th June, having been almost always intoxicated during the interval. The anasarca was now very general, and the urine for some time extremely albuminous, containing enormous quantities of epithelial elements and disintegrating tube casts. He died comatose, but never suffered from diarrhœa, or any form of hemorrhagic extravasation.

Sectio Cadaveris.—The liver weighed about 3 lbs., and presented the appearance of a considerably advanced stage of cirrhosis.

The kidneys were slightly enlarged, very pale, and the cortical substance occupied throughout with obscure granulations of Bright.

The parts to which I wish chiefly to refer are the spleen and the blood. The lymphatic glands were nowhere enlarged, and the other blood glands appeared quite healthy.

Spleen.—The spleen was greatly enlarged, weighing 2 lbs. 2 oz., and very soft, almost diffluent. At the posterior border of its convex surface, towards the upper extremity, was a mass of whitish substance, irregular in shape, and separated by a distinct edge from the splenic pulp. This extended over a space of rather more than a square inch ; and, when cut into, was seen to extend inwards into the substance of the spleen for about three quarters of an inch. It was comparatively firm and dense, contrasting with the soft and different condition of the surrounding splenic pulp. Few or no traces of blood-vessels were visible in it, to the naked eye at least ; it had a granular opaque texture, and was friable.

When a portion of the pulp of the spleen was examined under the microscope, with a power of 250 diameters, the following structures were seen : —1st, The usual small round granular-like cells, of which the splenic pulp is chiefly composed. 2d, The spindle-shaped fibre-cells from the trabeculæ, very distinct, and in large numbers. 3d, There were also other cells much larger than the common pulp cells, measuring about $\frac{1}{500}$th of an inch in diameter ; round, oval, or irregular in shape, and often slightly tinged with yellow. They were filled with granules, and had besides from one to three round or oval granular nuclei in their interior. These nuclei bore a strong resemblance to the cells of the pulp. 4th, Cells of large size compared with the other cells which were seen, commonly round, but often irregular in shape, with a finely granular surface, and completely filled with smaller bodies. These included bodies varied in size from $\frac{1}{3000}$th to the $\frac{1}{1500}$th of an inch, were of a round or oval shape, and slightly granular appearance. They appeared to be the same in characters as the nuclei of the above-described, and, like them, differed in none of their characters from the cells of the pulp. These cells existed in large numbers, although not so numerous as the cells with from one to three nuclei. 5th, There were considerable numbers of delicate spherical diaphonous cells of different sizes.

1st, The white substance was found to be chiefly composed of the common cells of the splenic pulp, but very delicate and irregular in shape. 2d, There were also in large numbers the spindle-shaped cells of the trabeculæ, but very much altered, being much smaller, often without the two caudate-like processes, and destitute of the nucleus which we always find in the normal fibre-cells of the spleen. In short, they appeared to be undergoing a species of degeneration. 3d, There were also traces of the cells which we have marked 3d and 4th in describing the elements of the pulp, but much altered and degenerated. 4th, Cells of every size, from that of the common pulp cell up to the large cells No. 4, completely filled with oil granules, which refracted light powerfully, and had often a tinge of orange colour. 5th, Masses of oil granules, which were destitute of a cell-wall. 6th, Collections of granules, of different sizes, sometimes surrounded by a cell-wall and sometimes not, and characterised by their bright orange colour.

Blood.—A small incision was made into the descending cava, a little above the bifurcation, and a quantity of blood drawn out by means of a pipette. It presented a dark-brown appearance, and was not at all coagulated. On microscopic examination, it was found to contain a very great excess of colourless blood-corpuscles ; but these were still inferior in numbers to the coloured corpuscles of the blood. They measured from $\frac{1}{1500}$th to the $\frac{2}{1500}$ths of an inch in diameter, presented a slightly granular surface, and contained, besides, some small globules, like oil-globules. On treatment with acetic acid, a nucleus was brought into view, which in some presented a kidney shape, in others it had a bipartite or tripartite appearance, like the nucleus of the pus-corpuscle.

The colourless corpuscles had a tendency to adhere to one another, and presented themselves in masses of considerable size; some, however, floated about free amid the coloured corpuscles of the blood.

Remarks by Mr Drummond.—With respect to the nature of the large cells which have been described as occurring in the pulp of the spleen, and containing in their interior a number of smaller bodies, I do not think that they are to be regarded as absolutely abnormal elements. I have seen similar bodies in the spleens of oxen and sheep, especially in the embryos of these animals; and in some species of fish, such as the common skate, they are very numerous and distinct. They are the same as the cells which have been described by Kölliker as cells containing blood corpuscles. Whilst examining the spleen sometime ago, it was my impression that these large cells were those in which the small bodies of the pulp were formed, and this impression receives some confirmation from a comparative study of these bodies in different animals. In the spleens of the mammalia, the bodies occurring in the pulp are of small size, and we find in like manner that the bodies contained in the larger cells are also of small size. In the spleen of the fish and reptilia, in which the cells of the pulp are twice as large as those in the spleen of the mammalia, the bodies included within the large cells present also a corresponding increase in size. Moreover, they agree in every other particular with one another,—they have the same shape, the same granular appearance, and contain sometimes a nucleolus, sometimes not. It is interesting to notice that there is also a remarkable correspondence in size between the cells of the splenic pulp and the white corpuscles of the blood in the different classes of animals.

With respect to the nature of the white deposit-like substance, there can be little doubt, from its structure, that it is only a portion of the splenic tissue undergoing degeneration. I remember a somewhat similar case which happened last summer, in which there was a portion of white firm substance about the size of a hazel nut imbedded in the centre of the pulp of the spleen, and surrounded by a firm capsule of fibrous tissue. On microscopic examination, it was found to be made up entirely of the altered elements of the splenic tissue,—the white appearance seeming to depend on the absence of blood-vessels.

CASE XXXIII.—*Leucocythemia detected during Life and after Death; no Hypertrophy of the Spleen or Liver; Enlargement, with Cancerous Infiltration of the Thyroid Body and Lymphatic Glands of the Neck; Cancer of the Lungs; Bronchitis.*

Margaret Stewart, a cook, æt. 60, admitted into the clinical ward July 16, 1851. For some years back she has been subject to a short dry cough, which has never been troublesome except after cooking a larger dinner than usual. With the exception of an attack of diarrhœa when the cholera was prevalent, she has been more or less constipated. Has never suffered from epistaxis or other form of hemorrhage. Four weeks ago she first perceived a swelling in the neck, which, commencing in front, has gradually spread towards the right

side. Latterly her breathing has become short and hurried; her strength has decreased, and the cough has been accompanied by considerable expectoration. On admission, the neck presents a prominent indurated swelling anteriorly, measuring about four inches in diameter, evidently owing to enlargement of the thyroid body. A chain of enlarged glands extends from the anterior swelling round the right side of the neck, a little beyond the ear. She complains of great weakness, constant sweating at night, and cough with copious frothy expectoration. The chest is everywhere resonant on percussion. There are loud sonorous and moist râles heard over the whole chest, especially posteriorly and inferiorly. The vocal resonance is also unusually loud, but equal on both sides. The tongue is furred, dark brown in the centre, deglutition difficult, apparently from pressure of the enlarged cervical glands. The appetite is bad, with an acid taste in the mouth. Other functions properly performed. She continued in this condition for several days, during which iodine and counter-irritants were applied to the neck, and expectorants and antispasmodics taken internally to relieve the cough. The dyspnœa, however, gradually increased; deglutition became more difficult, and her strength diminished. On the 30th of July the urine was ascertained to contain albumen, which had previously not existed. She died without a struggle, August 5th.

The day previous to her death I examined the blood with a microscope, conceiving that the enlargement of the lymphatic glands in the neck rendered this case somewhat similar to that recorded by Virchow (Case VI.) I found the colourless corpuscles greatly increased in number, not so numerous as in the case of Tinlay (Case II.), but fully as much so as I remember to have seen them in Case III. They were much smaller in size than those previously figured, varying in diameter from the $\frac{1}{150}$th to the $\frac{1}{100}$th of a millimetre. They presented the same re-action, on the addition of water, as in Case II.; but the divided or tripartite nucleus, like the corpuscle itself, was much smaller in size.

Fig. 1. Fig. 2.

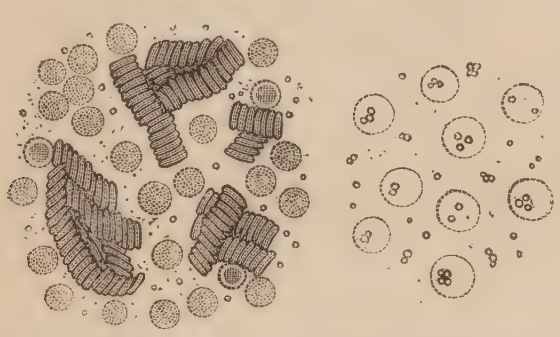


Fig. 1.—Appearance of the blood in Case XXXIII.
Fig. 2 —The same, after the addition of acetic acid.

Sectio Cadaveris, August 7.—Body greatly emaciated.

The Blood.—On carefully opening the vena portæ, the blood was found to be fluid, presenting its normal characters to the naked eye. On puncturing the vena cava descendens, the blood was of a thin gelatinous consistence, of dark red colour, approaching black. In the cavities of the heart the blood was firmly coagulated, and presented a colourless layer of the clot to half its extent, which, when torn up, was found to contain here and there masses of a cream-coloured granular material, exhibiting a marked contrast to the yellowish normal coagulum surrounding them.

Neck.—On dissecting the integuments from the neck on the right side, a considerable number of glands, about the size of a barley-corn and small pea, were observed in clusters between the platysma myoides and the sterno-mastoid muscle. A hard tumour existed in front of the neck, stretching along the

whole front of the trachea, and over the great vessels on either side beneath the sterno-mastoid muscles, and posteriorly on the right side, as far back as the transverse processes of the vertebræ, and down beneath the clavicle to the anterior surface of the first rib, where it was firmly adherent to the periosteum. A prolongation of the tumour, about the size of two walnuts, passed beneath the sternum at its upper end, being attached to its periosteum. This prolongation on section presented the outline of a congeries of enlarged lymphatic glands, having a white appearance, in some places soft, and even diffluent, and yielding on pressure a copious milky cancerous juice.

Chest.—There were lax adhesions at various points on the pleura on both sides. The pleural cavities contained a little fluid, on the right side amounting to about five ounces. At the lower part of the left lung, and also at the back part of right lung, there was a small amount of recent membranous exudation. A multitude of small cancerous nodules were scattered throughout the whole of both lungs. Some were immediately below the pleuræ, and some in the substance of the organs. For the most part these masses were scattered pretty equally, being as numerous at the base as at the apex, and varying in size from a millet seed to that of a small walnut. Some were of firm consistence, and others soft and friable, presenting various degrees of induration. They all on pressure yielded a copious milky juice.

Abdominal organs healthy.

Microscopic Examination.—The fluid blood from the vena portæ, and the more coagulated blood from the vena cava, contained an unusual number of colourless corpuscles, but they were not so numerous as was observed in the blood during life. This was explained by afterwards examining the coagula in the heart and large vessels, which contained an immense number of these bodies, aggregated together by means of molecular filaments of fibrine. The cancerous juice squeezed from the cervical glands, and the nodules scattered throughout the lungs, contained numerous cancer-cells, which it is unnecessary to describe minutely here. Associated with these were a considerable number of round colourless corpuscles, varying in diameter from the $\frac{1}{150}$th to the $\frac{1}{100}$th of a millimetre in diameter. They were globular in form, slightly granular on the surface, and no nucleus could be detected in them. (Fig. 3.) On the addition of acetic acid, however, the walls of these cells were partially dissolved, and there could now be seen a single, bipartite or tripartite nucleus, similar in appearance to what has been described in previous cases, but much smaller, the individual granules, though varying in size, not exceeding the $\frac{1}{500}$th of a millimetre in diameter. (See Fig. 4.) Further examina-

Fig 3. Fig. 4.

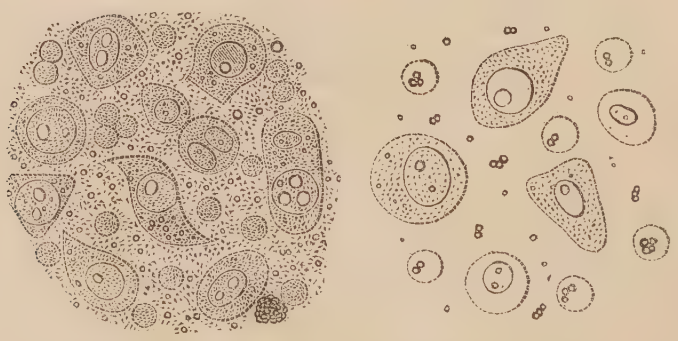


Fig. 3.—Corpuscles in cancerous juice, from thyroid body, in Case XXXIII.
Fig. 4.—The same, after the addition of acetic acid.

tion demonstrated that these latter bodies were not found in all the cancerous pulmonary nodules, but only in such as accompanied the large bronchial tubes, and which, therefore, were probably originally glands.

Remarks.—In this case there were none of the symptoms we have so frequently seen to exist in cases of leucocythemia, associated with enlargement of the abdominal organs. Remembering, however, the case of Virchow, in which the lymphatic glands only were affected, I examined the blood, and found it to contain a marked excess of colourless cell elements. These cells were much smaller than those observed in many other cases, and closely resembled those usually found in lymphatic glands. Indeed there can be little doubt that, associated with the cancer cells, were numerous cells peculiar to the lymphatic glands, a supposition that is strengthened by finding these corpuscles only in cancerous masses, which had apparently been at one time lymphatic or bronchial glands. This case also appears to me capable of throwing light upon Case X., recorded by Dr Fuller of London, in which there was an encephaloid tumour of the abdomen. May not some of the lymphatic glands have been involved in such a tumour, and, as in the one now before us, have given rise to admixture of cancerous matter, with the normal cells of such structures? That such an idea is more than probable, is also supported by the following case, the leucocythemia being explicable by the fact, that when cancerous infiltration takes place in a glandular structure, it frequently happens that its other component elements are more or less hypertrophied, and that the glandular cells find their way into the blood.

CASE XXXIV.—*Leucocythemia detected during Life and after Death; Cancer of Liver and Lymphatic Glands; Ulcer of Stomach.*—(Communicated by Dr W. Robertson.)

James Maxwell, aged 53, was admitted to ward 5, in the Royal Infirmary, on the 22d July 1851.

History.—Till the year 1824, he worked as a field labourer in the neighbourhood of Edinburgh, where he was born, and also about Falkirk and Glasgow. For the last twenty-seven years he has been employed as a porter about Leith, and has led a very irregular life, being addicted to the use of spirits, &c. About eighteen years ago was treated in the Infirmary by Mr Syme for stricture of the urethra, which soon yielded to dilatation with bougies. Was a strong and healthy man, till within the last seven years. His present illness, the first serious one with which he was ever affected, commenced in 1845 with a lax state of the bowels, from which he has since habitually suffered. The dejections often consisted of, or were mingled with, quantities of black matter, —never, to his knowledge, with red blood. No vomiting, want of appetite, pain at stomach, œdema, or other symptom attracted his attention, till the month of May 1851. At this time he began to be annoyed with fits of vomiting, occurring at irregular intervals, — symptoms of stricture again showed themselves,—the habitual diarrhœa became more profuse,—the appetite and strength suddenly failed,—he lost flesh rapidly,—and the right testis became enlarged and painful. Soon afterwards, he began to suffer from pain in the abdomen, increased by pressure, and chiefly referred to the region of the stomach. Being unable to work, he now sought admission into the hospital.

Present Condition.—Short in stature, dark-haired but nearly bald, toothless, and to all appearance nearer 70 than 53, which he states to be his real age. Is drowsy, and answers questions with difficulty and slowly, apparently from inability to sustain his attention, and from excessive feebleness. He is emaciated,

particularly about the face. Skin dry, cold, and sallow. Breathes slowly. Pulse about 80 in a minute, feeble. Complains of debility; pain in abdomen, increased by pressure; fits of griping, vomiting, diarrhœa, and difficult micturition.

Abdomen feels full; the recti muscles are firmly contracted; fluctuation is perceptible in both flanks; a good deal of fluid gurgling in the bowels. The liver protrudes a full span below the right false ribs and the ensiform cartilage. Its surface can be felt, hard, and covered with irregular nodules of various sizes, some of which are impressed with, or include, cup-like depressions of smaller size. These nodules may be distinctly felt descending with the substance of the liver during inspiration. The spleen does not seem to transgress its normal limits. The superficial veins in the epigastrium are distended. The pulsation of the abdominal aorta is obscurely communicated to the swollen liver. The bladder, on percussion, seems full of fluid. The right tunica vaginalis is occupied with a tumour, part fluid, part solid. Urine not examined, being passed at stool, and in small quantities.

August 9.—Bougies have been several times used, and the stricture admitted number 6 a day or two ago. The urine is now passed more easily; it is scanty, of a light straw colour, specific gravity 1015, slightly turbid when heated. On the addition of nitric acid, a blackish precipitation takes place,—redissolved by the addition of more acid, and passing through dark olive green and purple to a light reddish tinge; albuminous flocculi remain suspended in it.

The treatment has been palliative, consisting of diuretics, including gin, aqua potassæ, sp. æther. nitrici, and tincture of hyosciamus. Gallic acid and opium have been used to mitigate the diarrhœa; and wine has been given in moderate quantities.

Vomiting of dark-coloured matters, and purging of dark fæces, mixed with blood, have of late been very urgent. The strength has greatly declined, and the emaciation made progress, while œdema of the lower extremities has come on. The tumour in the right side of the scrotum now exceeds in size a large turkey's-egg. A drop of blood examined to-day microscopically, was seen to contain an unusual proportion of colourless corpuscles, irregular in size, and, for the most part, not larger than the red corpuscles. Died August 11.

Sectio Cadaveris, August 12.—*Chest.*—Left sac of pleura contained about ℥iv. of clear serum. Left lung slightly adherent. Less fluid in right pleura, and a few adhesions posteriorly. Both lungs slightly œdematous; a few stripes or cicatrices at each apex. Bronchial tubes contain much frothy mucus; mucous membrane much congested. Heart contains coagula of yellowish-white colour, and dark fluid blood, in all its cavities. It weighs 8 oz.

Abdomen.—About two pints of serum in peritoneal cavity; for the most part clear, but in the pelvis turbid and mixed with soft strings and flakes of lymph. Lower margin of *liver* extending from the cartilage of ninth rib on either side, straight across the epigastrium. Surface of liver thickly studded with yellow-tubera, in size varying between that of a walnut and pea; adherent to the stomach, and, when separated, disclosing a large black, fœtid, ulcerated aperture into the cavity of this viscus. Liver weighs 5 lbs. 12 oz. On section, the usual characters of encephaloid disease are apparent, and some of the tubera are found to be as large as a hen's-egg.

Two perforating ulcers existed,—one in the anterior, and the other in the posterior wall of the stomach, both nearer the cardiac than the pyloric extremity, opposite each other, and about an inch in diameter. Both were closed up by adhering peritoneum, and their edges were thickened, sloughy, greenish, and fœtid.

The *spleen* measures nearly 5 inches by 3½ inches, and weighs 15½ oz. It is adherent to diaphragm, and to the great end of the stomach; and, when separated, discloses another ulcerated perforation of considerable size. Surface of

spleen at different parts white and semi-cartilaginous; its substance firm, and on section normal.

Kidneys weighed 10 oz.; pancreas 2½ oz. The mesenteric and lumbar glands hypertrophied, and some of them cancerous.

The great vessels, especially in the lumbar region, surrounded and compressed by enlarged glands.

Bladder empty. A short and narrow stricture of the urethra about four inches from the external meatus.

Hydrocele of right tunica vaginalis. Right testis enlarged.

Mucous membrane of small intestine congested, except about Peyer's glands, which are pale and shrunk. Membrane generally firmer than usual. In colour throughout minutely granular, irregularly thickened, and of dark scarlet colour.

Fat of omentum peculiarly granular, and appendices epiploicæ firmer than usual.

The blood was taken carefully from the vena portæ and vena cava descendens. The first presented a thickish fluid of a dark colour; the last was formed into soft gelatinous clots. The blood was firmly coagulated in all the cavities of the heart, the coagula extending into the large vessels. They presented a dark and yellowish layer; the last exhibited, for the most part, a semi-transparent fibrous appearance, but towards its upper portion the clot was opaque, of dirty yellow, and readily broke down under pressure, yielding a copious purulent-looking fluid. This difference in various parts of the colourless coagulum was easily detected by difference both of colour and general consistence.

Microscopic Examination by the Author.—During the life of this individual I was enabled, by the kindness of Dr Robertson, to examine blood, taken from the finger by the prick of a pin, several times. The colourless corpuscles, which were much increased in number, presented two distinct sizes. The larger were evidently the ordinary colourless bodies, such as are seen in health, and were observed in most of the other cases we have recorded. They appeared to be in their normal proportion to the coloured corpuscles, and when acted on by acetic acid exhibited the usual double or tripartite nucleus. The smaller colourless bodies, which were much more numerous, varied somewhat in size, but were of the average diameter of $\frac{1}{200}$th of a millimetre. They were round, had a finely granular surface, and, on the addition of acetic acid, underwent no change in size, but their external margin became more distinct, and the whole corpuscle somewhat more transparent. (Figs. 5 and 6.)

Fig. 5. Fig. 6.

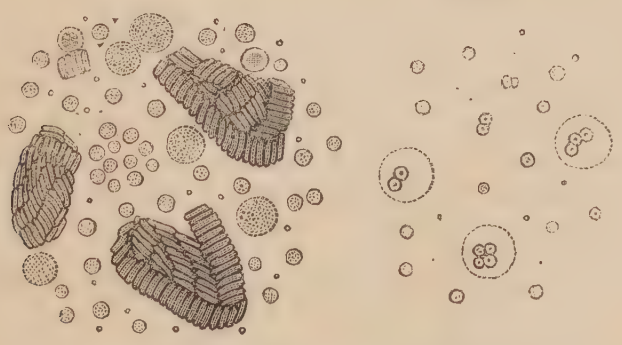


Fig. 5.—Appearance of blood in Case XXXIV.
Fig. 6.—The same, after the addition of acetic acid.

On examining the portal blood after death, it was found to be crowded with colourless corpuscles of the two distinct sizes formerly noticed. That from the vena cava contained also more than usual, but by no means so many as were present in the portal blood. The colourless coagulum from the heart presented, for the most part, its normal fibrous structure; but its upper fourth, where it

was softer and readily broke down, was almost wholly composed of the colourless bodies formerly described.

The cancerous masses in the liver yielded a copious milky juice, which contained numerous cancer cells. These, in the smaller nodules, were of young formation, while in the larger ones they were more or less loaded with fatty granules. Masses of reticulum were also present in these larger nodules, consisting of fatty molecules and granules, with disintegrated cells and nuclei. Very few of the colourless corpuscles were found in the cancerous juice from the liver, and these apparently were derived from the blood.

The white cancerous-looking lymphatic glands contained numerous cancer cells; but there was associated with them a multitude of naked nuclei, and a few cells peculiar to the glands. These last, on being acted on by acetic acid, presented a round nucleus, which, both as to size and structure, exactly resembled those previously observed in the blood. In some lymphatic glands the cancer cells were few in number, and in others altogether absent; but these latter were enlarged, yielded a turbid gray juice on pressure, and contained a multitude of the colourless bodies just alluded to.

The spleen pulp was composed almost wholly of colourless bodies, exactly the same as those found in the lymphatic glands and in the blood, mixed with a few blood globules, and the usual fusiform cells of which the trabeculæ are composed.

Remarks.—In this case the colourless corpuscles in the blood were of two distinct sizes. The larger were the usual bodies found in the blood, very slightly increased in number, the smaller were evidently the same as the numerous naked nuclei found in such large numbers in the hypertrophied glands, and in the spleen pulp. The most careful examination, the action of re-agents, and exact measurement, left me in no doubt of this fact. In Case VI. Virchow speaks of certain free nuclei in the blood,—a statement which has always appeared inconsistent with my observations until I met with the present case; but which is now fully confirmed. On examining the larger colourless bodies after the action of acetic acid upon them, there might be detected evident transitional changes between the single nucleus exactly like those which were naked, and the double tripartite, and even quadruple body, so frequently presented. This case, therefore, in conjunction with the last one, demonstrates, that hypertrophy of the lymphatic glands causes increase of their cells or nuclear elements, and that these find their way into the blood. It is also deserving of notice, that the spleen in this case was only a very little enlarged, though soft, but abounded in corpuscles. The importance of these facts will be dwelt upon in the Second Part of this Memoir.

The following case offers another good illustration of enlarged spleen, without leucocythemia:—

CASE XXXV.—*Hypertrophy of the Spleen.* (Communicated by Dr James Wallace of Greenock.)

John Maclachlan, mill-worker, single, Irish, æt. 22, admitted into the Glasgow Infirmary, under Dr Easton, 20th March 1850. Two years ago, in consequence, as he supposes, of having strained himself when lifting a heavy weight, the patient became affected with acute pain below the margin of the false ribs on the left side. This was relieved by cupping, but he ever afterwards per-

ceived that there was some fulness in the affected part. He continued, however, free from uneasiness till twelve months ago, when he again became troubled with pain, followed by dropsy of the abdomen, for which he was treated in the Clyde Street Hospital, where he resided for eight weeks. From that time till five weeks ago he enjoyed a fair state of health, but he has since become once more affected with pain below the floating ribs, and also in the left renal region. Has likewise been troubled with occasional diarrhœa, accompanied with tormina and tenesmus, the stools being sometimes slimy, and tinged with blood, and the secretion of urine considerably diminished.

At present, the abdomen is very full, and fluctuation detected in the lower part when the patient is made to sit. The left hypochondrium appears fuller than the right, and there is felt a firm tumour, slightly moveable, extending over to the linea alba, and down to a line drawn transversely, and about an inch above the umbilicus. Its margins cannot well be defined by tracing it with the fingers, but percussion over the whole is dull, as well as over the lower half of the left lateral, infra-scapular, and infra-mammary regions. There is pain, on pressure, over the whole of the tumour, and also over the right hypochondrium; but the patient complains principally of pain, aggravated on pressure, in the left posterior lumbar region, in which a small firm and irregularly defined tumour can be felt. Urine exceedingly scanty, and loaded with lithate of ammonia and epithelial scales, the specific gravity being 1036; bowels loose, the stools containing some slime and blood; some tenesmus and tormina; skin of a moderate heat and dry; tongue clean and moist; pulse 88, soft.

The treatment consisted principally of diuretics, and occasional counter-irritation to the left hypochondrium, with the effect of diminishing the dropsy, but in no way checking the diarrhœa, or reducing the emaciation, and size of the tumour. He was dismissed by desire, May 20th.

Remarks by Dr Wallace.—In this Case it is exceedingly probable, from the pain felt in the right hypochondrium, that in addition to hypertrophy of the spleen there existed enlargement of the liver, but this could not be well ascertained in consequence of the very great accumulation of fluid in the abdominal cavity. The secretion of urine was throughout very imperfect, the quantity in twenty-four hours frequently not amounting to two ounces, and having a specific gravity as high as 1038. This might be the result partly of the diarrhœa and partly of the mechanical effect produced on the kidneys by the ascitic fluid; but I am more disposed to believe that there existed an organic affection of these organs independent of the latter cause, both because the patient, on admission, complained of severe pain in the loins, and particularly in the left renal region, where an irregular tumour was distinctly perceptible, and because, on referring to the book in which I recorded my observations on the urine, I find that it deposited, in addition to the lithate of ammonia, a large quantity of epithelial scales, which did not disappear, even after the pain abated, but continued as long as the case was under treatment. An opportunity, however, for verifying this opinion was not afforded, for the patient, who had become very weak during the last month of his residence in the hospital, was taken home by his friends, because they abhorred the idea of a post-mortem examination, which they suspected, from the interest manifested in the case, would be eagerly desired. I learned afterwards, from the nurse

of the ward, that he died suddenly about a fortnight after removal, the operation of *paracentesis abdominis* having, in the interval, been performed by a surgeon in private.

Very fortunately, about a fortnight before losing sight of the patient, I abstracted, at the request of Dr Easton, a small quantity of blood from one of the veins of the forearm, and subjected it to a microscopical examination, but failed, as did also Professor Allen Thomson, to whom I took a portion of it, to discover the presence of "white cells." This is not reported in the journal, but I made at the time a private memorandum, in which, besides, I find that the blood discs were evidently diminished in amount, that the clot was exceedingly soft, and the complete separation of the serum very rapid, occupying less than an hour.

Part II.

I have now collected together all the cases which are known to me, in which an excess of colourless corpuscles has been observed in the blood. These serve to establish the occasional existence of a morbid condition, eminently calculated to arrest the attention of the physiologist and pathologist. That the blood may be loaded with a multitude of cells, exactly resembling those of pus; that such blood may circulate in the human subject for months, or even years, without destruction to life, and that this condition is always associated with disease in those organs, the functions of which have hitherto been involved in the greatest obscurity, constitute facts which seem calculated to exercise an important influence on many views that have been long agitated in science. The constitution of the blood itself; the origin of its morphological elements and chemical proximate principles; the importance of the lymphatic system; the functions of the spleen and other blood glands; the nature of purulent infection, and other diseases of the blood, may be expected to be more or less elucidated by a study of the accompanying phenomena, causes, and results, of leucocythemia. With a view, then, of stating as succinctly as possible the conclusions which may be legitimately derived from the thirty-five cases previously recorded, I shall divide this part of the inquiry into several sections, in which all these important topics will be shortly considered.

Section I.—Symptoms observed in individuals affected with leucocythemia.

The symptoms have been very carefully observed in several of the cases recorded in the first part of this Memoir, but we have great difficulty in referring any of them to the mere alteration in the blood. Several of those which were most constant and best marked in advanced cases, were apparently caused by the increased size of the spleen or liver, as they have been seen to occur in other cases where these organs have been enlarged, without the occurrence of leucocy-

themia (Case XXVIII). No doubt the peculiar change in the blood of which we are treating has been discovered in individuals affected with enlarged spleen; but this may arise from the circumstance, that the circulating fluid has been more frequently examined in persons labouring under that complication. When leucocythemia, however, is more generally studied, it will very probably be found associated with enlargement in other organs, especially of the thyroid, thymus, supra-renal capsules, and lymphatic glands. Hence, I am persuaded, no systematic history of the symptoms connected with this morbid state can be given in the present state of our knowledge; and I shall therefore merely content myself with an analysis of those observed in the cases recorded.

Of the thirty-five cases which are given in the preceding pages, leucocythemia was demonstrated to exist, by careful microscopic examination, in twenty-five. The facts presented by these may be afterwards compared with those offered by such cases as were doubtful, or by those in which, associated with enlarged spleen, it was proved that the blood was quite healthy.

Sex.—Of the twenty-five cases, sixteen occurred in males and nine in females.

Age.—The youngest case in which leucocythemia was observed was in a girl aged 9, and the oldest in a woman aged 69 years. In two cases the ages are not stated, but in the remaining twenty-three they may be arranged as follows:—Under 10 years, one case; between 10 and 20, two cases; between 20 and 30, three cases; between 30 and 40, seven cases; between 40 and 50, four cases; between 50 and 60, three cases; and between 60 and 70, three cases. So far as this analysis goes, the disease would appear to be most common in adult life, and more frequent in advanced age than in youth.

Abdominal swelling.—Greater or less swelling of the abdomen was present in twenty out of the twenty-five cases,—evidently dependent, in the majority of these, on enlargement of the spleen and liver, singly or united. In five cases ascites was also present. In several of the cases there was more or less abdominal pain or tenderness, while in a few the enlargement only produced inconvenience from its size or weight.

Respiration.—The respiration was more or less affected in twelve out of the twenty-five cases. Of these dyspnœa existed in eight. The respiration is said to have been hurried in one; short in a second; laborious in a third; and slow in a fourth. The disordered respiration appeared to be dependent in some cases on enlargement of the abdomen, and corresponding compression of the pulmonary organs, in others (five cases) it may have resulted from disease of the lungs themselves.

Vomiting was present in seven cases. In two at the commencement, in three it was occasional, in one there was hematemesis, and in one it was connected with ulcer of the stomach.

Diarrhœa was present in twelve cases, and in some was the leading symptom throughout the progress of the disease. In Tinlay, for instance (Case II.), during the six months he was in the Infirmary, the bowels were opened from eight to twelve times a-day for weeks together. In other cases this symptom only came on latterly, and and in a few was not urgent.

Constipation is said to have existed in five cases.

Hemorrhages.—Extravasation of blood occurred in fourteen out of the twenty-five cases. Of these there was epistaxis in six cases; hematemesis in one case; hemorrhage by stool, including hemorrhoids, in four cases; hemoptysis in one case; flooding after delivery in one case, and bleeding from spongy gums in one case. In some of these cases bleeding from the gums or bowels was associated with epistaxis, and this last symptom was observed in some of the best marked cases of the disease, with enlarged spleen.

Dropsy was present, more or less, in thirteen cases, generally dependent on the abdominal tumour. There was anasarca in two cases, ascites in four cases, and œdema of the lower extremities in seven cases.

Fever.—More or less fever was observed in eleven cases, indicated by increase of pulse, loss of appetite, thirst, and heat of skin. It was occasionally present at the commencement, at other times at the termination, of the disease. In no case did it exist to any extent, or was long continued. From the frequency of splenic enlargement, it might be supposed by some that the disease was connected with intermittent fever, but that this ever occurred is very doubtful. It is said to have preceded the disease in three cases. In Case VIII. the report says, that four months previous to admission there had been intermittent fever; but Dr Walshe adds, "this point was not sufficiently inquired into." In Case X. there had been repeated attacks of ague, the last of which occurred nine years before became under observation, and seven years before the abdominal tumour was perceived. In Case XIX. the patient also had laboured under intermittent fever, but seventeen years previous to the commencement of the abdominal swelling. So far as the recorded cases are concerned, therefore, there is every reason to believe that intermittent fever is in no way concerned with the production of leucocythemia.

Palor of the surface.—An unusual palor of the surface was observed in many cases, resembling that of anemia. The conjunctivæ, also, were of a peculiar light blue tint.

Jaundice.—In one case only of all those in which the liver was affected, was jaundice observed.

Emaciation.—In most of the fatal cases emaciation was extreme.

Complications.—Disease of the lung was present in five cases, including one case of bronchitis, one of phthisis, and three of pneumonia. Bright's disease existed in two cases,—cerebral hemorrhage in one case; cancer was present in three cases,—in one, in the form of an undescribed abdominal tumour (Case X.), in a second, there was cancer of the thyroid body and neighbouring lymphatics, and in a

third, cancer of the liver, with ulcer of the stomach, stricture of the urethra, and hydrocele. All these diseases were characterised by their peculiar symptoms, or physical signs during life.

It must not be supposed that the above numerical account of the symptoms exhibits even an approximation to the proportion which any particular one holds to the number of cases on record. Owing to the imperfection with which many of these are described, important symptoms in some not being even alluded to, this is obviously impossible. Statistics are no more applicable to this subject than to any other in medicine, where the cases have not been expressly drawn up in reference to such an inquiry.

SECTION II.—CONDITION OF THE BLOOD IN CASES OF LEUCOCYTHEMIA.

Of the twenty-five cases of undoubted leucocythemia, it was detected after death only, in ten; during life only, in six; and both during life and after death, in nine cases. Thus, it has been detected in the living body in fifteen cases, and in the dead body in nineteen cases.

On examining the blood of living persons (which is most readily accomplished by extracting a drop from the finger by pricking it with a needle, and then examining it between glasses under the microscope in the usual way), the yellow and colourless corpuscles are at first seen rolling confusedly together, and the excess in number of the latter over the former is at once perceived. This, however, becomes more evident after a short time, when the coloured bodies are aggregated together in rolls, and leave clear spaces between them, which are more or less crowded with the colourless ones. Means are altogether wanting to enable us to determine with exactitude the relative proportion of the two kinds of corpuscles in different cases. In some the colourless corpuscles are only slightly increased beyond their usual number. In one case they are described as five times as numerous as those in health. They are also said in particular instances to be "greatly increased," "one-third as numerous," and "as numerous" as the coloured corpuscles. In all these statements there is nothing exact. Perhaps the best method of judging is to regard the spaces or meshes left between the rolls or aggregations of yellow blood corpuscles. When these are completely filled up, the colourless bodies do not, in fact, amount to one-third of the coloured ones, on account of the large number of the latter which may exist in a small space, in the form of rouleaus. This will appear from counting them in Fig. 6, given in the January No.

The size of the colourless corpuscles in the various cases given differs considerably. Even when at first sight they appear to be of tolerably uniform size in any one case, it may be observed, when they are magnified highly and carefully measured, that some are twice the size of others, with all the intervening sizes between them. In some cases, though comparatively few in number, they are de-

scribed as being three or four times larger than the coloured corpuscles, and in two cases recorded in the present number of the Journal (Cases XXXIII. and XXXIV.) they were in one about the same size, or somewhat smaller, and in the other of two sizes, one larger and the other decidedly smaller.

In the nineteen cases in which the blood was carefully examined after death, the same variations with regard to number and size of the colourless corpuscles were found to exist, as have just been referred to in blood drawn fresh from the finger. It was always observable, however, that they were most numerous in the clot; and when they existed in any number, as in Cases I. and II., they communicated to the colourless coagulum a peculiar dull, whitish look, and rendered it more friable under pressure (Case I.) When less numerous, portions of the colourless coagulum from the heart and large vessels might be seen to present a dull cream colour, easily distinguishable from the gelatinous and fibrous appearance of a healthy clot, and such altered portions always contained a large number of the colourless bodies. This was especially observable in Case XXXIV.

There is one remarkable fact which has been strongly impressed upon me by careful observation of the preceding cases. In no one instance has the condition of the blood been observed to undergo any marked change after the excess of white cells in it was discovered. In no case has this condition of the blood been seen to appear and progress gradually, as is observed in so many other lesions. In the case of Tinlay (Case II.), the patient was under medical observation for a period of eighteen months, and the same excess of colourless corpuscles existed at the end of that time as at its commencement. In the case of Kerr (Case XIX.), the corpuscles were only slightly augmented in number, and yet at the end of eleven months they were not more numerous than when first examined. Cases are still to be met with, therefore, in which the commencement and progress of leucocythemia are to be observed. Such can only be expected to be found when the microscopical investigation of the blood is more generally practised in clinical investigation, as it is commencing to be in the Royal Infirmary.

SECTION III.—CHEMICAL COMPOSITION OF THE BLOOD IN CASES OF LEUCOCYTHEMIA.

The chemical analysis of white cell-blood has been undertaken in only five cases, a number far too few to arrive at any important results. One cause of this is, that the majority of the twenty-five undoubted cases were only discovered after death, when any analysis of the blood in reference to the relative proportions of all its constituents cannot be determined. Another cause is owing to the circumstance, that several of the cases observed during life were so weak and exhausted, that the abstraction of even two oz. of blood, for the purpose of analysis, could not be safely ventured upon. Of the five analyses, three

were performed by Dr William Robertson of Edinburgh, one by Dr Parkes of London, and one by Dr Strecker of Giessen. Dr Robertson also analysed the blood of a sixth case (Case XXVIII.), in which there was enlargement of the spleen without leucocythemia. The following is a tabular view of these analyses, the inferences from which will be given on a future occasion :—

Analyses of the Blood.

Case.	Sp. Grav.	Sp. Gr. of Serum.	Fibrin.	Serous Solids.	Globules.	Total Solids.	Water.	
II.	1041·5	1026·5	6·0	72·0	67·5	145·5	854·5	Leucocythemia.
III.	1036·0	1023·0	2·3	67·0	49·7	119·0	881·0	
VIII.			7·08	75·22	101·63	183·93	816·07	
Later analysis.			4·75	77·52	97·93	180·2	819·8	
XIX.	1049·5	1029·0	5·0	95·0	80·0	180·0	820·0	
XXIX.			4·46	82·35	97·39	184·2	815·8	
XXVIII.	1042·0	1025·5	3·9	75·7	76·3	155·9	844·1	

SECTION IV.—MORBID ANATOMY OF INDIVIDUALS AFFECTED WITH LEUCOCYTHEMIA.

Of the twenty-five undoubted cases of leucocythemia which have been recorded, the body has been examined after death in nineteen. The information obtained from this source may be still further extended by a consideration of four cases in which the existence of this condition of the blood is highly probable; of seven cases recorded by Dr Hodgkin of enlargement of the spleen and lymphatic glands, and of two cases examined after death where the spleen was hypertrophied without leucocythemia. In all, thiry-two dissections.

The organs which have been found to be most uniformly diseased are the spleen, the liver, and lymphatic glands, and of these I shall speak separately. The other lesions found in the brain, lungs, heart, kidneys, &c., alluded to in Section I., under the head of complications, were evidently accidental or consecutive, and need not be alluded to especially, in this place.

Condition of the Spleen.—Of the nineteen cases of leucocythemia in which the body was examined after death, the spleen was found to be more or less enlarged in sixteen. In the other three, although it was healthy, the pulp in one is said to be "a little more compact than usual;" in a second its condition after death is not alluded to, although an encephaloid tumour occupied the left side of the abdomen; and in a third, the spleen was "healthy."

Of the sixteen cases in which the spleen was increased in volume, it weighed above 7 lbs. in three; above 5 lbs. in two; above 3 lbs. in two; above 2 lbs. in four; and nearly 1 lb. in one case. In four cases it was not weighed. The greatest weight of a spleen was 7 lbs. 13 oz., and

the largest measurement 16½ inches long, and 9½ inches broad. The texture of the organ varied in different cases, in some being of unusual density, in others natural, and in a third class more or less soft and pulpy. In a few cases it contained yellowish masses, apparently a form of deposit, but in reality degenerated tissue. The structure was examined microscopically in seven cases, in all of which it was demonstrated that the cell and nuclear elements were increased, while the fibrous portion of the organ was apparently normal.

In four cases in which the existence of leucocythemia is probable, changes similar to those just stated occurred in the spleen, and in Dr Hodgkin's cases similar lesions were found associated with enlarged lymphatic glands.

It is clear, however, that mere enlargement of the spleen is not necessarily connected with white cell-blood, for in case XXVII. it was simply hypertrophied and weighed three pounds and a half; and in numerous other cases where this organ has been undoubtedly enlarged, it has been proved by careful examination that the blood was normal—(Cases XXVI., XXVIII., XXXV.) It remains to be ascertained what are the structural differences in the spleen existing between cases like these last, and those in which leucocythemia exists.

Condition of the Liver.—Of the nineteen cases examined after death, the liver was diseased in thirteen. In the other six it is distinctly stated to have been healthy in five, while in one it is not noticed in the report.

Of the thirteen cases, the liver was cirrhosed in two,—one in its incipient and one in the advanced stage of that disease. In a third case there was cancer of the organ, and in the ten others the liver was more or less hypertrophied. Of these, it weighed above 13 lbs. in one; above 12 lbs. in one; above 10 lbs. in one; above 6 lbs. in three; and above 5 lbs. in two cases. In two cases, though much enlarged, the weight is not stated. In these cases the organ was more or less congested, and its consistence varied from great firmness to a degree of softening amounting to diffluence. The minute structure of the liver was carefully examined in four cases; and found to be unaffected in three, while in the fourth it was infiltrated with cancerous exudation.

In the six probable cases of leucocythemia, it is said that the liver was greatly hypertrophied in four. In the other two its condition is not stated.

Condition of the Lymphatic Glands.—Of the nineteen cases examined after death, the lymphatic glands were more or less diseased in eleven. Indeed, it is very probable that they were affected in a larger number, as in most of the other cases they were in no way alluded to, and may possibly have escaped observation from an unacquaintance with the importance which, as we shall see, ought to be attached to them.

Of the eleven cases, the lymphatic glands throughout the body were greatly enlarged in four, and more or less cancerous in three others. The mesenteric glands were especially affected in two; the thyroid and epigastric glands in one; and the solitary and aggregate intestinal glands in one. In some cases they were soft, presenting on section a granular whitish appearance, and yielding a copious turbid juice on pressure. In other cases they were more indurated; and in one there were slight calcareous deposits. The glandular structure was carefully examined microscopically in eight cases, and in all exhibited increase of the normal tissue, the juice abounding in cell or nuclear elements. In two cases, cancer cells were mingled with the healthy textures of the glands.

In the 17th vol. of the Medico-Chirurgical Transactions, Dr Hodgkin has recorded seven cases in which the lymphatic glands were more or less enlarged, and at the same time associated with increased size of the spleen. He considers the enlargement of both structures to be allied, and to depend upon a primary lesion unconnected with inflammation or adventitious structures. The appearance of a bloody serum in the thoracic duct and absorbents struck him in two of these cases, but the blood itself was not apparently noticed. At the time Dr Hodgkin wrote (1832) the microscope was not much employed in pathological investigation, but had the blood been examined in these cases, I cannot resist the conviction that the discovery of leucocythemia would not have been reserved for the year 1845.

In the concluding portion of this memoir, it will be my endeavour to establish from the foregoing facts, and from numerous other observations and experiments:—

1. That the coloured blood-corpuscle is derived from the colourless one.

2. That the colourless blood-corpuscles are derived from the glands of the lymphatic system.

3. That the lymphatic glandular system is composed of the spleen, supra-renal capsules, thyroid body, thymus (pituitary, pineal?) and lymphatic glands, and that these constitute an extensive apparatus for the formation and elaboration of blood-corpuscles.

4. That the fibrin of the blood is derived from the solution of the blood-corpuscles, and the effete matter resulting from the disintegration of the tissues.

5. That these propositions concerning the origin, development, and disintegration of the blood-corpuscles are now as a consistent theory advanced for the first time, receive proof of their correctness from the cases of leucocythemia previously detailed, and are in harmony with the facts elicited by the labours of Hewson, Nasse, Wagner, Richert, Gulliver, Zimmerman, Wharton Jones, Simon, Kölliker, Milne Edwards, Goodsir, and others.

ARTICLE IV.—*On the Utility of the Persesquinitrate of Iron in the Treatment of Ague.* By WILLIAM KERR, Surgeon, Corresponding Member of the Medical and Physical Society of Calcutta, and of the Medico-Chirurgical Society of Glasgow.

AGUE, in its various forms, prevails in Canada from the commencement of rainy weather in spring till frost in winter; in most years from the beginning of March till the end of October or November. Frequent alternations of rain, with warmth and cold, always produce a sickly season, and cause an extension of ague into localities which, in drier years, are nearly exempt from it. It is not in the recesses of the forest that the settler encounters this disease; it is generally after trees have been cut down over a considerable extent of ground, and the beams of the sun freely admitted to the surface of the earth, that pestilential miasmata are exhaled. The land is in general so level, that obstructions to the flow of streams and rills are readily produced, and give rise to swamps of greater or less size. A person newly arrived from Scotland will be much surprised at the small extent of marshy ground, which is here the source of ague to the inmates of the immediately adjoining cottages. The distance to which miasmata spread, and the height to which they rise, depend upon the size of the marsh from which they originate;—a small marsh affects only those living immediately adjoining, but a large one affects people a mile or two distant, and sixty feet above its surface. The formation of mill-ponds, which in this country generally have a considerable proportion of shallow water, almost invariably gives rise to the disease in their vicinity.

A healthy individual may reside a long time in an aguish district without being affected; but if he be chilled in summer or autumn, or by travelling during the night, or by exposure to rain, he will almost certainly be seized with ague, and in a week or two the man who was ruddy and robust will be sallow and emaciated—the spectre merely of what he was.

The varieties of ague are remittent or bilious fever, tertian and quotidian ague, chill fever, and dumb ague. Remittent fever is the severe and deadly variety. It prevails most during August and September, in localities where miasmata may be supposed to be dense. Of it I have scarcely any practical knowledge, and shall, therefore, confine myself to the other varieties of ague, commencing with tertian.

The first paroxysm is generally preceded for several weeks, or even months, by declining health, marked by lassitude, pains in the limbs, head, and back, diminished appetite, and impaired digestion. The aguish character of these symptoms is determined by their great aggravation in rainy or very warm days; in dry or moderately warm weather pain is little felt in any part, though the appetite may remain indifferent. But, as the paroxysm approaches, the prelimi-

nary symptoms are more constantly present, and for a few mornings previously great lassitude and melancholy, headach, and a white furred tongue, are almost invariably associated. At this time the urine is scanty and high-coloured. The paroxysm is often ushered in by a febrile attack, accompanied by pain in the limbs, head, and back, which, in the course of possibly a day, terminate in rigors, followed by fever and sweating. Diarrhœa sometimes precedes or accompanies the rigor. The shivering generally commences from eight till ten o'clock forenoon, but sometimes not till evening. The first symptom is excessive yawning, followed by a sensation of cold water trickling down the back; the countenance becomes pale, the nails livid, and the patient soon shakes with incontrollable violence. During this period, which lasts an hour, or an hour and a half, warmth, in whatever way applied, altogether fails to relieve. The patient may lie as close as possible to a blazing fire or lighted stove, or, when the thermometer is at 90°, may be covered by a feather-bed, and may shut every door and window, yet the sensation of cold will continue. With the fever comes on intense headach, accompanied with intolerance of light and noise, and occasionally delirium; pain stretches from the back to the præcordia, every limb aches, the muscles seem to the patient to be on the rack; lying still is painful, yet every attempt to change the position aggravates the pain. Sometimes there are vomiting and tension of the abdomen. In two or three hours the fever declines, the skin becomes moist, and is soon covered by a copious perspiration having a yellow colour, and an odour peculiar to ague; pain diminishes; and at the distance, perhaps, of twelve hours from the commencement of the paroxysm, the patient falls asleep.

Next day he is jaded, but able to go out of doors; next again, the attack is renewed with all its accompanying symptoms. After continuing for some time as a tertian, it generally, if unchecked by medicine, becomes a quotidian. When left to nature, how long the disease may continue I am unable to say; but when no remedial measures had been adopted, I have seen paroxysms of ague occurring daily, after the lapse of two months.

When paroxysms have been prevented or postponed by quinine, the disease, after a few months, occasionally changes into chill fever,—that is, the chill does not amount to rigor,—is followed by fever and sweating, and is often more distressing than tertian ague.

Dumb ague signifies headach, frequently accompanied by pain in the back and limbs, occurring usually at a certain hour every day, or every second day, more especially in wet or very warm weather. The appetite and digestion are not good. These symptoms may exist for weeks before the occurrence of a paroxysm of ague, and for years after, and point out that the disease still exists in the constitution, ready to make its attacks in fully formed pa-

roxysms, whenever the patient is exposed to rain, fatigue, or exertion in hot weather.

Tertian and quotidian ague, chill-fever, and dumb ague, may all be seen in the same individual in the course of one illness, and must, therefore, be regarded as varieties of one disease, originally caused by exposure to miasmata; but afterwards called into action by causes unconnected with miasmata. The names chill-fever and dumb ague, though not used by systematic writers, are inserted to designate varieties which are common.

Though ague be painful and distressing to a degree which makes almost every sufferer under a paroxysm say that he is utterly indifferent to life, yet the most disheartening feature of the disease is its strong disposition, when it has once made an attack, to return again. This disposition may remain for months or years, during which the patient cannot say that he is well; more especially in warm or in wet weather he is listless, and indisposed to perform his work, and will say that he has not half his former strength. If an European, he will have lost his ruddy complexion, and acquired the sallow hue, and possibly the lank figure, of Americans. The appetite is indifferent; and pains in the head, back, and limbs, are readily excited by hot weather, fatigue, or exposure to rain. These causes acting with more intensity reproduce ague, or even produce it for the first time, though the patient be far distant from malaria, provided the disposition has been formed by a residence, however short, in an aguish district during summer or autumn. Instances are numerous of individuals removing from such districts to places or countries where ague is not endemic, being again seized with it. A medical friend informs me of a sailor, under his observation, who contracted the disease in China, and was attacked with it in a fog on the Banks of Newfoundland.

It is well known that the failure of the British expedition to Walcheren, in 1809, was owing to the occurrence of remittent fever and ague to a most alarming extent. In the course of three weeks from the date of landing, sickness appeared; and in the succeeding four months, out of an armament of 70,000 soldiers and sailors, or more correctly chiefly out of 42,000 to 43,000 soldiers, 36,000 were sent to hospitals, almost entirely with remittent fever and ague. Several of the most eminent medical men in the service of the Government proceeded to investigate the disease, and render their assistance. The sick were brought to Britain as speedily as possible, and ordered to healthy districts, to get rid of ague and gain strength. When these objects were supposed to be accomplished, the troops were sent to Spain, where a sanguinary war required numerous reinforcements. Of those who had been in Walcheren,[1]

[1] Contributions to Statistics of the Sickness and Mortality which occurred among the Troops employed in the Expedition to the Scheldt, in the year 1809. By Henry Marshall, Deputy Inspector-General of Army Hospitals, in the "Edinburgh Medical and Surgical Journal," vol. xlviii.

Dr Marshall informs us, that "many of the men were, upon the first exposure to cold and fatigue, rendered unfit for duty, so as frequently not to leave one-third of the strength fit for service." The same author says,—"In numerous instances the noxious effects of the miasmata did not appear until nearly a year after the troops returned to England" from Walcheren, "when they became suddenly affected with the specific symptoms of the disease, apparently in consequence of the slight fatigue of a short march." In Canada, most of the members of a farmer's family in an aguish district may be incapacitated by ague for exertion in seed-time, or in harvest, when their labour is indispensably necessary. I have been told of all the families in a locality being confined to bed at one time, and their crops rotting on the ground, from want of reapers to cut them down. In a merely pecuniary point of view, therefore, ague entails a heavy loss upon the community where it prevails. The expense of the Walcheren expedition has been variously estimated, possibly from one author limiting himself to the direct cost, and another including the indirect. In the seventeenth volume of the "Edinburgh Review," it is stated that, from facts extracted from evidence laid before the Houses of Parliament, it appears that the cost of the expedition may be put between two and three millions. In the "Annual Register," for 1809, it is asserted that the cost amounted to twenty millions sterling.

Take the following illustrations from civil life. A family of nine persons, consisting of the husband, a blacksmith by trade, wife, and seven children, with L.70 in their possession, arrived here from Scotland last summer. Induced by the prospect of employment, they settled near a mill-pond, twenty miles distant. The husband died from remittent fever, and the wife and six children were seized with tertian ague, which soon became quotidian. At the end of two months' sickness they returned to Hamilton, only one of their number, a girl of eleven years of age, in health; the others, from the mother to an infant, suffering from ague; their prospects ruined, their little piece of ground bringing no return, and their money nearly gone. A gentleman commenced business in Chatham, on the Canadian river Thames. Last summer the whole family were prostrated by ague; a brother was sent for to carry on the business, but he, too, was seized. At the time this information was communicated to me, the business was being wound up, and the family preparing to depart for some place where the disease is not endemic. A commercial friend has shown me a letter from a customer, complaining that he cannot execute an order, on account of all his men and himself being confined with ague, and the unhealthy character of the place being such, that other men could not be got. In an aguish district improvements go on languidly, because those who have the disease in their constitutions have neither spirit nor strength to perform even their ordinary work.

Repeated attacks of ague injure the constitution greatly, and lay

a foundation for other complaints, most commonly hydrothorax. I have often heard medical men say, that a great proportion of the ill health and sickness here originates from ague.

As the object of this paper is to point out the utility of the persesquinitrate of iron in curing ague, I shall not enter into a systematic consideration of the treatment. In a paper on the persesquinitrate of iron, published in the "Monthly Journal of Medical Science," for May 1848, I have stated, that I had recently found this medicine efficacious in cutaneous diseases depending upon disordered digestion. A case of this kind occurred to me in May 1849, but immediately after I was consulted, and before treatment was commenced, the patient was seized with tertian ague. It was my first practical acquaintance with the complaint, and therefore one or two paroxysms had occurred before I had made up my mind respecting the treatment. It was clear that a cutaneous disease existed in the same condition of body with ague; I thought that this cutaneous disease required the use of persesquinitrate of iron, and that the ague likewise might be greatly mitigated by the adoption of the chalybeate treatment. Influenced by this view, I gave a dose of calomel, and fifteen grains of the sulphate of quinine, divided into three doses, along with persesquinitrate of iron, in the interval preceding an expected paroxysm. A paroxysm did not follow; the persesquinitrate alone was continued in doses of a tea-spoonful thrice a-day. The patient's appetite and strength immediately began to improve, and the cutaneous eruption disappeared. A paroxysm has not since occurred, and neither wet nor very warm weather produces any of these symptoms which indicate the existence of ague in the constitution.[1]

[1] I now prepare the solution of the persesquinitrate of iron of half the strength of the formula in the "Monthly Journal of Medical Science," in order to preserve it transparent a reasonable length of time during the hot summers of Canada; this dilution has the additional convenience of bringing the ordinary dose to a tea-spoonful, instead of thirty drops. I therefore request that the solution of the persesquinitrate of iron may hereafter be prepared according to the following formula?—

Take of Iron Wire, that known by the name of Number 17, one ounce.
Nitric Acid, three fluid ounces.
Water, one hundred and seventeen fluid ounces.

Mix the nitric acid with fifteen ounces of water in an earthenware or glass vessel; put into it the iron wire, broken into several pieces, and so twisted as to extend into every portion of the liquid (on this the complete saturation of the acid depends); place the vessel in a moderately warm situation; and in eight to twelve hours the solution will be completed, when the liquid is to be poured off the remainder of the wire, which ought always to be in slight excess. The residue of the water is now to be added, so as to increase the solution to one hundred and twenty ounces.

By using a large quantity of water, muriatic acid is not required. A glass receiver over the vessel in which the solution goes on, with its lower end immersed in water, will cause the absorption of the fumes of nitrous acid, and allow the process to be carried on within doors. In this country, in the open air, the cold of winter would prevent the formation of persesquinitrate, and the iron would be dissolved in the form of protonitrate.

I was soon after consulted by a gentleman, who for two months had suffered greatly from ague. The paroxysms had been frequently interrupted by quinine; but his constitution was shattered, his appetite much impaired, and his complexion sallow. I advised him to take persesquinitrate of iron for a month or two thrice a-day, an hour before meals. As soon as he commenced this treatment he began to improve in every respect, has not since had ague, and is at present robust and muscular.

I have since seen at least one hundred cases of ague, chill-fever, and dumb ague, some of which I shall now relate. A mechanic, on the 18th January 1850, had his elbow-joint cut open by a circular saw. While suffering from this he was seized, on the 9th February, with a severe paroxysm of ague. He had had this disease four and a half years previously; during the intervening time several attacks had occurred, he had experienced aguish symptoms in wet or warm weather, and had frequently been treated with quinine, arsenic, blood-letting, &c. The paroxysm on the 9th February was the most severe he had ever had, and under far more unfavourable circumstances than any previous one, yet it was more easily and speedily cured. Quinine and persesquinitrate of iron were immediately given, another paroxysm threatened, but did not fully form; besides, he has since—for a period of a year—been entirely free from aguish symptoms, and till lately in the enjoyment of excellent health. In the end of November he had a severe attack of pneumonia, during which I was gratified to find that no symptom of ague appeared. Before the administration of the persesquinitrate of iron, which was continued for at least a month, he took annually about sixty grains of quinine, without eradicating ague; since that period, he has not taken or required any.

The multitude of poor emigrants who annually come to this city, the most westerly port on Lake Ontario, have rendered necessary an Orphan Asylum, to which is attached a Destitute School. The former of these usually contains from twenty to thirty children, the latter from eighty to ninety. Prior to October 1849, the medicine given for ague was solution of arsenic, with tincture of gentian, administered by the master or matron, under the inspection, when required, of a medical gentleman. This medicine broke ague, to use the popular language,—that is, stopped the accession of the paroxysms for a short time, but did not eradicate the disease; so that the first exposure to rain, or any other hurtful cause, brought it back. During the interval the children were languid, sallow, unhealthy, liable to headach, pain in the limbs and back, and their appetites indifferent,—showing the existence of ague in their constitutions. In this climate during summer, rain usually falls every fortnight, consequently one or two of the children were scarcely tolerably well, when one or two others were seized. During winter, owing to the prevalence of frost, rain is of less frequent occurrence,—so that ague is always, in that season, more rare, and the health of the

children was therefore better. In October 1849, I furnished the institution with a quantity of persesquinitrate of iron, which was given to every one who had ague at that time, and to every one who had had it previously. The whole of the children were thoroughly cured, and no relapse, or even appearance of the disease, has since occurred. By a statement furnished to me by Mr and Mrs Wilson, the master and matron, fifteen children having ague have been cured, and thirty-seven, whose sallow and sickly countenances seemed to indicate its existence, have been restored by the persesquinitrate of iron to healthy complexions. Not only has the constitution been altogether free from ague, except in cases of children having it when admitted, and who have all been cured, but there has been scarcely any other disease. This I ascribe to the greater vigour of the children, who are now stirring and active, well coloured, muscular, with good appetites, and not subject to headach, nor pain in the limbs and back.

Elijah Smith, aged 9 years, an orphan, who had ague when 4½ years old, or possibly earlier, was admitted into the Asylum in 1848. Prior to October 1849 he had several attacks of age, and in the intervals was not in good health. At the period now mentioned he took persesquinitrate of iron, and has not since had ague. The following winter he could not go out in wet weather, nor be exposed to rain, without having pain in his head and limbs; this winter he is under no restraint, has been exposed to rain, and walked amongst melting snow with impunity.

Matilda Anderson, aged 13, for two years previously to October 1849, had ague, and latterly very severely and frequently. Notwithstanding arsenic and gentian, almost every return of wet weather brought it back. Persesquinitrate of iron was given to her along with the other children. She has not since had ague, though for the last twelvemonths she has resided in one of the most aguish parts of Canada. The persesquinitrate of iron was given to this girl about six weeks, and on an average to the others about two or three weeks, and at any time afterwards for a short period if the child looked pale and sickly, and had an indifferent appetite.

A gentleman, who held an important office here, became affected with ague, and, finding all his efforts to obtain a cure unavailing, resigned his situation, and between three and four years ago returned to Britain in the hope that a residence in his native land, joined with the best medical advice, would restore him to health. In this, however, he was disappointed, and last summer a near relation of mine met him in the Highlands of Scotland suffering from ague. After applying to me for instructions, he commenced the use of the persesquinitrate of iron; and about a month ago I received a letter, informing me that he is completely cured of ague, and that he attributes the cure to my medicine.

In the beginning of last May I visited a gentleman, 60 years of

age, who had immigrated from England twenty years previously. Thirteen years after his arrival he was seized with ague, and during the six years which intervened before my visit, had been confined at least one-half of his time to bed by aguish attacks. He was necessarily melancholy to a degree which made himself miserable, and greatly diminished the happiness of those around him. In wet weather he could not venture to the threshold of the house without a chill passing through him, followed by fever and sweating. In the warmest and calmest day he could not stand in the open air above half an hour without the same symptoms immediately following. If, instead of standing still, he walked, he could bear a longer exposure to the air, and the attacks of ague did not come on till next morning. The progress of one of these was chillness in the region of the stomach, such as might be produced by a ball of ice, diarrhœa, rigors, succeeded by fever, and pain in the head, back, and limbs; sweating followed, and at the distance of twenty hours from the commencement of the paroxysm he was able to leave his bed. Even in his bed-room he was at any time sensible of rain from the occurrence of aguish pains. A draught of cold water could not be taken without producing a paroxysm of ague. His appetite was bad, his countenance sallow even to his lips, he had almost constant headach and uneasiness in his back, he was feeble, his flesh soft, his step tottering, and old age had come on prematurely. He resided in one of the most healthy localities, the brow of the rocky steep which faces Lake Ontario, and upwards of 300 feet above its surface.

I gave him persesquinitrate of iron, and at the end of a fortnight he drove to my house, a distance of two miles, with perfect impunity, and as he said in good health. His appetite was good, and he had not any aguish symptoms. He has since not been a single day, or part of a day, in bed from ague, of which he is entirely cured, and his complexion has long been as ruddy as that of an inhabitant of Britain.

Mrs Campbell immigrated from Scotland upwards of six years ago. A year after she was seized with ague. At this time she was unmarried, and supported herself by making bonnets. During two years, more especially from March till November, paroxysms occurred so frequently that possibly a third of this period was spent in bed, and her earnings, while she was able to work, became inadequate to support herself and purchase medicines. Of quinine alone, she estimates that she took about 150 grains annually, besides every other medicine which her acquaintances happened to recommend as specifics, arsenic included. The disease appeared so hopeless that she meditated returning to Scotland, and was prevented only by want of means. She was listless, drowsy, and melancholy. About this period she married, and possibly owing to the necessity for close application to work being thereby diminished, the disease became less severe, and for two and a half years before I saw her in the beginning of March 1850, it existed chiefly as chill-fever and dumb

ague. She was sallow even to her lips, easily fatigued, or rather every exertion was burdensome, even conversation with an intimate friend gave pain instead of pleasure. She could scarcely go beyond the walls of the house without being chilled, and having aguish symptoms brought back. She had been only a few times at church since the malady first seized her, and when she did go generally suffered; indeed for a long period she was unable to walk above half a mile or a mile without next day having a paroxysm of ague. The strong disposition to chillness caused her to heat her house beyond the degree which was comfortable to healthy people. She had frequent headach, almost constant uneasiness in her back, a trifling appetite, and impaired digestion.

I prescribed the solution of the persesquinitrate of iron, of which she used eight ounces. She has since neither had quinine nor any other medicine; though previously her annual consumpt of quinine could not latterly be less than 100 grains. The improvement in her health was so rapid, that she ceased taking persesquinitrate of iron by the end of April, being then in good health. At present she has not a single symptom of ague; does not dress more warmly than healthy people, has been exposed to rain, and walked in melting snow with impunity, has a good colour and a good appetite, and, instead of being a third of her time in bed, or, at least, as she expressed herself, fitter to be in bed than out of it, she has since April been able to perform her domestic duties with comfort. She says that, during the four and a half years she had ague she was wretched; now she is happy and perfectly well. In these years she was constipated, so as to require medicine weekly; now she does not require any. An opinion is very common in Canada, that the continued use of quinine injures the constitution; my inquiries do not confirm this. The patient whose case has just been related, and who took as much quinine annually as most of the sufferers from the complaint, next day, after eight or ten grains, had headach and giddiness, which ceased during the night, and left no trace of injury behind.

Two brothers, aged 10 and 12, had ague for twelve months, before I saw them in April 1850. In this period each had taken about 100 grains of quinine, with no better effect than suspending the paroxysms for a fortnight or three weeks. During the winter there was little frost, and the paroxysms continued nearly as frequent as in summer. In both, these were preceded by diarrhœa, and in one of the boys frequently attended by delirium. Like other aguish patients, they were sallow and their appetite indifferent. During the intervals of attacks they were able to go to school, but were too feeble and aguish to join in the sports of their companions.

In April I prescribed to each eight grains of quinine to suspend the paroxysm, accompanied and followed by persesquinitrate of iron to eradicate the disease. Neither have since had ague, and both are now ruddy and robust, and able to go out in weather of every kind.

In August 1850 I was consulted by a mason, who had had ague for two years. The preceding autumn it was stopped, but not eradicated, by arsenic and gentian. In spring paroxysms recommenced, and from March till August he had taken about sixty grains of quinine. He regarded his health as giving way, and was convinced that some other disease besides ague was making inroads upon his constitution. Palpitation of the heart was frequent, and nausea, and often vomiting followed every meal.

I gave quinine to suspend the paroxysms, accompanied and followed by persesquinitrate of iron, to eradicate the disease. After he commenced this treatment he had only one paroxysm, and one threatening of a paroxysm. In the course of a week or two, nausea and vomiting entirely ceased, his appetite became good, and he soon was ruddy and muscular. After I saw him, he took sixteen grains of quinine and twelve ounces of the solution of the persesquinitrate of iron. The arsenic taken the preceding year did not remove his sallow complexion, cure nausea, nor prevent the aguish pains in wet weather—in other words, did not remove the disease from his constitution; persesquinitrate of iron did. He was robust before ague commenced; but, to quote his own words, he did not experience the sensations of health from that time, till he used persesquinitrate of iron.

A year ago I was consulted regarding a girl seven years of age, who had been first seized with ague five years previously, and had ever since been aguish. She was puny, pale, easily chilled by slight exposure to cold, childish in mind beneath her years, and had a pustular eruption on both hands, which had existed for several months. Her appetite was likewise bad. By my advice she commenced using persesquinitrate of iron; in two or three days the appetite became better; soon after the eruption diminished, and when four ounces of the solution were finished she was quite well in every respect. At present she is a healthy ruddy-looking girl, and has grown considerably in the last twelvemonths. Before I saw her she bore exposure to the open air so ill, that her education was in a great measure suspended; but from a short period after she commenced using my prescription she has been regularly at school.

A few months after I began to make trials of the persesquinitrate of iron in ague, I communicated my observations to Mr Macpherson, surgeon, who had long had great experience of this disease in one of the most aguish parts of Canada, the village and neighbourhood of Caledonia on the Grand River. I am under deep obligations to Mr Macpherson for the zeal with which he entered into, and has since prosecuted, this investigation. Long previous acquaintance with ague well qualified him for testing the value of a new remedy; and now, after a trial of eighteen months in upwards of 400 cases of remittent fever, and the different varieties of ague, his opinion is most decidedly in favour of the great utility of the persesquinitrate of iron.

Were I to relate all the cases which his kindness has supplied, I might possibly swell this paper too much; I shall therefore state only a few.

A man, whose constitution was thoroughly wrecked from chronic ague, who was sallow and emaciated, and incapable of the slightest exertion without bringing on a paroxysm next day, had applied to Mr Macpherson three years previously when the disease was commencing. The usual remedies failed to cure, and after some time the patient removed to an elevated part of the country, hoping that a drier atmosphere would aid medicine in producing a cure. Another medical man here prescribed for him, but with no better effect. Hopeless of obtaining a cure, he returned to his own house on the bank of the Grand River. Here, in the autumn of 1849, Mr Macpherson called upon him, to recommend the persesquinitrate of iron, of which, by this time, he had had numerous and successful trials. The patient consented, and was speedily and perfectly cured. Though still residing where he was first seized, and so long suffered, he remains in good health.

The following seems to point out the value of the persesquinitrate as a preventive of ague, doubtless by preventing or removing impaired digestion, which, in most instances, precedes the disease. A family of twelve immigrants arrived at Caledonia, two of whom died of cholera, and four were seized with choleric diarrhœa. To these four persesquinitrate of iron was given, and the diarrhœa cured. The remaining six were soon after seized with ague, while the four who had taken persesquinitrate of iron remained exempt. A hope that this medicine may be useful as a preventive is not unreasonable, seeing that the symptoms preceding an attack are often the same as those following, over which the persesquinitrate has so much power.

In proportion as Mr Macpherson gained confidence in the persesquinitrate of iron, his treatment became almost restricted to a few doses of quinine to suspend the paroxysm, and persesquinitrate of iron to eradicate the disease. He says, that of twenty recent cases, probably ten will be cured by ten grains of quinine and four ounces of the solution of the persesquinitrate; five, at the distance of a fortnight, will be threatened with paroxysm, and will therefore require twenty grains of quinine, and eight ounces of persesquinitrate; and the remaining five will have two threatenings a fortnight apart, and will consequently require thirty grains of the one, and twelve ounces of the other. Cases where paroxysms do not occur, comprehending most instances of chronic ague, will be cured by the persesquinitrate alone in four to six weeks.

I have learned from several other medical men, partly directly, and partly indirectly from individuals on whom I can entirely rely, that last summer they gave the persesquinitrate of iron with great success in ague. David Christie, Esq., one of the most eminent of Canadian agriculturists, who has taken much interest in diffusing a knowledge of this medicine, reports to me its success within his own

locality, and equally favourable statements from his acquaintances in other localities.

I shall now state the mode of treatment adopted. If the patient has paroxysms of ague, ten grains of sulphate of quinine, divided either into two or three portions, ought to be given before the expected accession of the paroxysm,—say one portion the preceding night, and two, with two hours between them, the following morning, each along with a tea-spoonful of persesquinitrate of iron. This treatment will most probably prevent the accession of the paroxysm. The succeeding treatment consists in giving a tea-spoonful of the persesquinitrate thrice a-day, about an hour before meals. In many individuals quinine suspends a paroxysm of ague only for a fortnight or thereabout, the disease returning thereafter every second day as formerly. If this relapse be anticipated, by a few grains of quinine, given as the first, ague will again be prevented. Patients who, from previous experience, know that the disease will return at the end of a fortnight, ought therefore to take quinine at the end of this period, and to persevere in the use of the persesquinitrate till they are restored to perfect health. As stated by Mr Macpherson, possibly five out of twenty may require a third quantity of quinine at the end of a second fortnight, when the patient's health ought to be so much improved by the persesquinitrate of iron that the disposition to relapse shall have altogether ceased. In perhaps one-half of the instances, as already stated, one quantity of quinine and the continued use of iron will cure the disease. I know one instance of an adult, and several of children, in whom the paroxysms were prevented, and the disease cured, by persesquinitrate alone; but such cases ought to be regarded as exceptions from the general rule. If the practitioner is not called till after the state of fever in, or paroxysm of, ague has commenced, two or three tea-spoonfuls of persesquinitrate of iron will greatly abridge the duration of the distressing headach and pains in the back and limbs, and often remove vomiting. Besides using medicine, the patient ought to avoid fatigue and exposure to rain, till his health and strength are re-established. Constipation must, of course, be removed; but it is only in a few cases of ague that persesquinitrate of iron constipates.

When the patient has ague, but without distinct paroxysms occurring at the time, persesquinitrate of iron alone will in a few days produce all the sensations of health, and effect a thorough cure in a few weeks, probably four to six. In this state of the disease, quinine or arsenic are not productive of any benefit.

The cases I have related are, I believe, fair specimens of the result of the treatment of the different varieties of ague by persesquinitrate of iron, either alone or along with quinine. In a very small number the persesquinitrate will fail, possibly owing to peculiarities in the constitution of the patients; but such instances are so rare that they ought to be regarded as exceptions to a general rule. Almost the whole of the failures arise from inattention on the part

of the patient in taking the medicine properly,—that is, trusting to the persesquinitrate alone, where quinine is also necessary,—or not taking the former with regularity for a sufficient length of time. Failures may also arise from fatigue, exposure to rain, or some other hurtful cause, before the strength is restored.

There are several points of resemblance between the use of the persesquinitrate of iron in ague and in chronic ague. When diarrhœa becomes chronic, opium fails to cure, but persesquinitrate of iron is more efficacious; and when ague becomes chronic, quinine fails to cure, but persesquinitrate of iron removes the disease. In diarrhœa, only a small proportion of cases becomes chronic; in ague, under quinine, arsenic, and the usual remedies, a great proportion, perhaps the greater part. It is very remarkable how rapidly in ague the patient improves, when treated with persesquinitrate of iron; generally in a day or two the appetite is perceptibly better, and is soon very good, the sensations of discomfort speedily diminish, the patient finds his strength increasing, and consequently his spirits improve; and generally before the protective power of one quantity of quinine has terminated,—that is, before a fortnight is ended,—the patient is much better, and soon after is quite well.

To complete the subject, I ought now to take up remittent fever; but as I have not any practical knowledge of this disease, I shall merely state, that Mr Macpherson assures me, that last autumn numerous trials convinced him that the severity and duration of the disease were diminished by the free use of the persesquinitrate of iron, the usual remedies being employed at the same time.

HAMILTON, CANADA WEST, 4th February 1851.

ARTICLE V.—*On Restoration of the Upper Lip.* By R. J. MACKENZIE, F.R.C.S.E., Junior Ordinary Surgeon to the Royal Infirmary, Edinburgh.

(*Read at Meeting of Edinburgh Medico-Chirurgical Society, on July 9th* 1851.)

WHILST the lower lip is a common seat of malignant disease, and its removal, in part or in whole, consequently an operation which is frequently performed, the upper lip is rarely the seat of cancer or other disease requiring removal by the knife.

To remedy the deficiency produced by the loss of the lower lip, various modes of operative procedure have been recommended, whilst, from cases of deficiency of the upper lip being comparatively rare, much less attention has been bestowed in attempts to restore the lost parts in this situation.

Blasius, Dieffenbach and others have successfully restored the lost upper lip by flaps, cut in different ways, from the cheeks; and in cases, where the upper lip alone has been destroyed, it will not

require much ingenuity on the part of the surgeon to effect the restoration in one of these ways.

In cases of harelip in which the cleft was unusually wide, so as to amount nearly to absence of the upper lip, the edges of the wide cleft have been allowed to be brought into apposition by cutting through the whole thickness of the cheek, in a line curved upwards from each angle of the mouth. I have resorted to this proceeding, and have more than once seen it successfully practised by others, in cases of deficiency of the greater part of the lip, when the soft parts in the situation of the cheek were in a sound condition.

These modes of operation are suited for cases where the upper lip *alone* is deficient, and in which the cheek has not been involved in the injury or disease by which the parts have been destroyed. One of the most common causes, however, of destruction of the soft parts of the face is *cancrum oris*, as well as the mortification which occasionally attacks these parts during fever and the eruptive diseases of children, and in such cases the cheek seems rarely to escape the ravages of the disease.

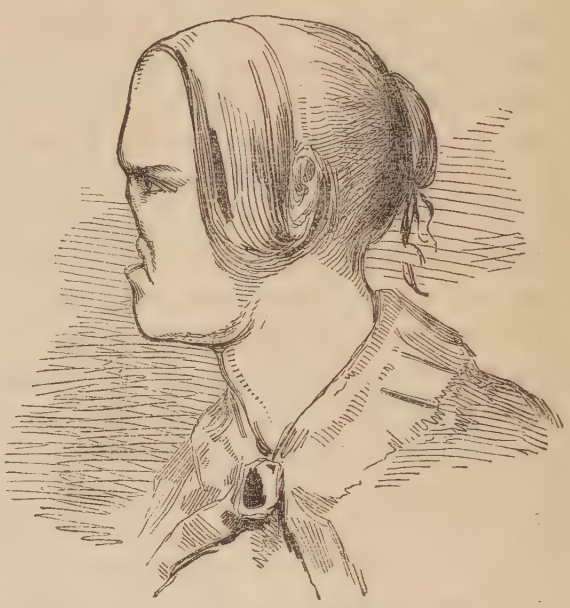
Two such cases have lately presented themselves in the hospital, in both of which, from the destruction of the cheek which had taken place, it was impossible for me to restore the lost parts by any method, as far as I know, hitherto practised; and it is to the plan which I have adopted in these cases that I am anxious to direct attention. It has proved in the following case successful in removing the extreme deformity which existed, and promises, I think, in similar cases, to answer the purpose in view satisfactorily.

The subject of the first operation was a girl, of eighteen years of age, who, when between two and three years of age, lost her entire nose, nearly the whole of the upper lip, and the greater part of the right cheek, from mortification, the sequela of an attack of fever. She subsequently grew up a strong and healthy girl, and was in the enjoyment of excellent health when she entered the hospital (12th March 1851) with the determination to submit to anything which could be done for the removal of her excessive deformity.

The sketches at p. 350 give a good representation of the appearance presented by her face. The hard palate was widely cleft from exfoliation of the superior maxillary bones, which had taken place in nearly the whole extent of the roof of the mouth. Her articulation was very indistinct, and, as she spoke, her tongue protruded through the opening of the alveolar arch. The remains of the right cheek were drawn in by a thin cicatrix, which adhered to the superior maxillary bone above the angle of the mouth. The integuments of the brow were ample, and in excellent condition for the formation of a new nose; and the only difficulty lay in the restoration of the lip, which evidently could not be effected by incisions, however extensive, through the remains of the cheek.

The only plan by which it appeared to me the lost parts could be restored was, *the transposition of the lower lip to the situation of the*

upper, whilst, at the same time, by extending the incisions under the base of the jaw, the integuments in that situation might be brought up to replace the lower lip, according to the plan recommended and successfully practised by Mr Syme. The incisions required to effect this were pretty extensive; but it appeared to me



that any measures, however severe, were warrantable, if they held out the prospect of at all effacing the hideous appearance which the girl presented, whilst she herself was most willing to submit to anything which was likely to effect this end.

The operation was performed (19th March) in the following manner:—After removing a slice from the free border of the remains of the upper lip on the left side (Fig. 1, A A), and freeing this part from its adhesions to the gum, I made a curved incision

Fig. 1.

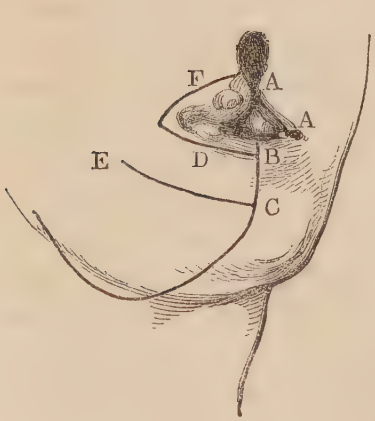


Fig. 2.

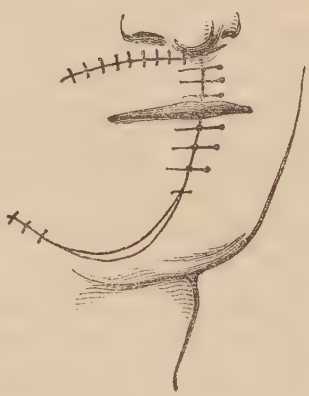


Fig. 3.

(E C) from about an inch outside the angle of the mouth through the whole thickness of the lower lip to about its centre, and parallel with the border of the lip. A second incision (B C) was then made, at right angles with the termination of the first, and pro-

longed downwards under the base of the jaw, in the manner practised by Mr Syme in the restoration of the lower lip. The *prolabium* of that part of the lip included in the incisions was then removed, and a cut surface (F) made in the natural situation of the base of the upper lip, by removing the cicatrix from the nostril to the right angle of the mouth. The two flaps formed by these incisions were then dissected fully back, and the bleeding vessels were secured by ligature. The upper flap was then raised and united (as shown in the diagram, A A, to B C, and D to F) in the situation of the upper lip, whilst the lower and larger flap was brought up and united to the cut surface of the lower lip and chin.

The right half of the lower lip was thus made to occupy the situation of the upper, whilst its place was supplied by the flap from beneath, and the right side of the mouth was now formed by the incision (E C) between the two flaps.

Fig. 2 explains the position of the parts when united in their new situation.

The operation was tedious and embarrassing from the unsteadiness of the patient, owing to the extreme difficulty of bringing her under the full influence of chloroform,—a difficulty which was experienced previously in removing two projecting teeth, and equally so afterwards in the operation for the restoration of the nose. She suffered from very little constitutional disturbance, however, and perfect union of the parts was obtained in the course of a few days.

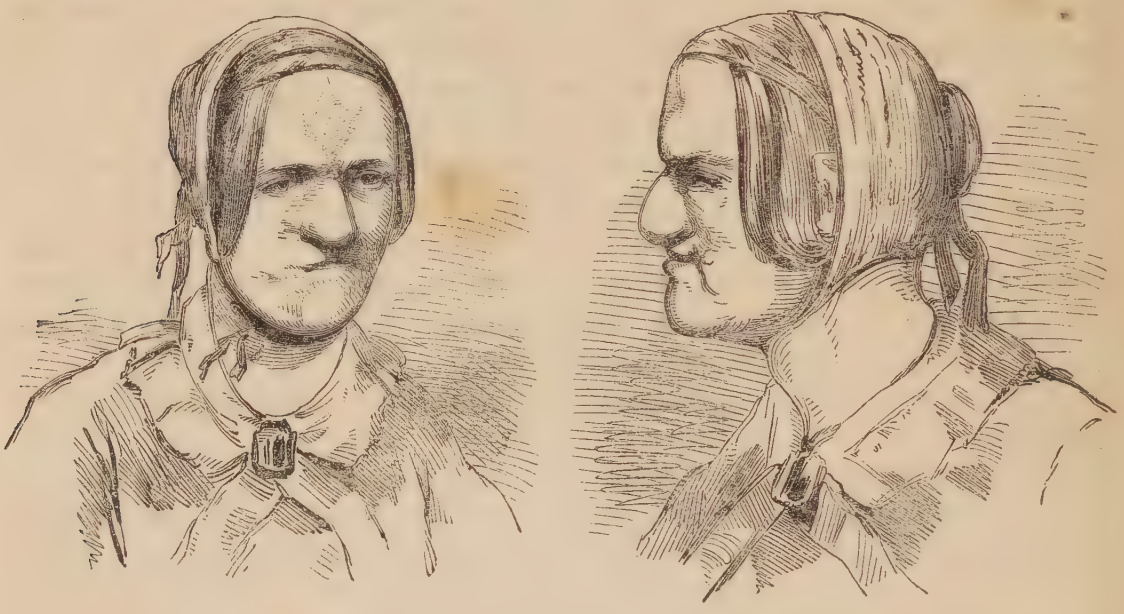
A new difficulty, however, presented itself in the contraction of the mouth, which, in spite of all endeavours to prevent it, went on till the entire granulating surfaces of the two flaps had united. This contracted state of the mouth was afterwards obviated to a considerable extent by an incision, in which the integument and mucous membrane were divided at different levels. The mucous membrane was then everted and united to the skin, so as to form a *prolabium* for the upper lip, whilst the skin was similarly inverted and united to the mucous membrane of the lower lip. The mouth was thus enlarged; but a small portion of the lower lip was unfortunately lost by sloughing, which has left a slight permanent deficiency at the lower part of the angle of the mouth.

About ten weeks after the first operation, when the parts were firmly cicatrised in their new situation, the nose was restored by bringing a flap from the brow. The operation was performed in the usual way, with the exception of the *columna* being taken from the scalp, according to the Indian method. This was done, as it was thought better not to interfere further with the new lip, which would have been considerably diminished in size by raising a *columna* from its centre.

About a month after the formation of the nose, the collateral circulation of the new organ being well established, I divided the twisted neck of the flap, and attached it in the usual way.

The sketches at p. 352 show the result of these operation

The front face is from a daguerreotype, which, with a sketch of the profile, was taken five months after the formation of the lip, and three months after the restoration of the nose. The nose appears large,—a fault which, I need scarcely add, is unlikely to be permanent.



The chief difficulty met with in the progress of the case arose, as I have observed, from the obstinate contraction of the mouth; and the error committed in the operation, which gave rise to this difficulty, consisted evidently in the removal of the *prolabium*. This inconvenience, I have since found, may be avoided by preserving the *prolabium* of the lower lip in its situation, and uniting it to the flap brought up from over the base of the jaw.

The lines in Fig. 3 will serve to explain the situation of the incisions by which this object is attained; and if the method of operation I have described is thought worthy of performance, this modification of it (the preservation of the *prolabium in situ*) will be found of the greatest advantage in obviating the contracted state of the mouth.

The second case, in which a somewhat similar operation has been performed, was that of a girl, of eight years of age, who, when two years old, suffered in a similar way to the preceding case, from mortification of the face during fever. The same parts were destroyed as in the case I have related; but the left half of the upper lip had retained its vitality. The whole of the soft parts, however, between the eye and the lower lip had been lost, a small portion of the lower eyelid at the outer angle only remaining. From the contraction during cicatrisation, the remains of the angle of the mouth had been drawn upwards close to the eye. The nose, too, had been entirely destroyed. In this case I attempted to restore the lost parts by bringing up a large flap, consisting of the lower lip (saving the *pro-*

labium, as in Fig. 3), and integuments over the base of the jaw, so as to fill up the whole gap at once; but the result of the operation was unsuccessful, from an unforeseen accident. From the effects of the chloroform, which had been administered pretty freely during the time of the operation, the poor child vomited, with little cessation, for thirty-six hours. From the long-continued drag thus made on the sutures, union failed in the entire extent of the wound, and the flap, in spite of all means used to keep the edges together, gradually retracted and receded from the surface to which it had been attached. By a second operation, however, the flap was united to the remains of the upper lip. The upper lip has thus been completely replaced, and, from the preservation of the *prolabium* (as shown in Fig. 3), the natural size, position, and appearance, of the mouth has been entirely restored.

The restoration of the eyelid and a small part of the cheek, which are still deficient, may in this case be accomplished without much difficulty; whilst no obstacle exists to the formation of a new nose by a future operation.

ARTICLE VI.—*On the Reported Incompetency of the Aboriginal Females of New Holland to Procreate with Native Males after having Borne Children by a European or White.* By THOMAS R. HEYWOOD THOMSON, M.D., Surgeon R.N.

THE very just remark of Locke,—"all that is to be found in books is not built upon a true foundation," was forcibly recalled to my mind of late, by noticing that some of our physiological and medico-legal writers have copied into their works, almost without comment, the very extraordinary statement made by Count Strzlecki in his Physical History of New South Wales (published 1845), to the effect, that when once a female of the aboriginal tribes of Australia has borne a child to a European or white, she ceases entirely to reproduce with males of her own race. A statement so utterly inconsistent with all the known facts connected with the history of the human race, induced me in 1849, during my sojourn in New South Wales, to make particular inquiries, both from natives and settlers of acknowledged observation and veracity.

On informing some of the Broken Bay and Hunter River blacks that such had been reported of their "gins," or women, they scouted the idea altogether, assuring me that it had occurred repeatedly for native women to bear children to their husbands after having cohabited with, and borne *copper-coloured* children to Europeans. Indeed, their accompanying remarks were not at all flattering either to the distinguished traveller who originated, or to myself who then reported, the anomaly. Among the settlers to whom I referred the

matter, nearly all expressed themselves surprised at the statement; one of them (Mr Coulson), whose knowledge of the Moreton Bay natives was very considerable, repudiated altogether this new feature in the history of the aborigines, averring that he had seen it happen several times among the King Bar tribe, the Lady Plains tribe, and Logan Plains tribe, for "gins," or native women, to have black children to their native males, after having had copper-coloured children to squatters or settlers. And to confirm this, a gentleman (Mr Hayes) who recently returned from New South Wales, after living fourteen years near the Bogan and Mole tribes on the Macquarrie River, and who is now residing near me here (in the Ise of Man), informs me that Count Strzlecki's statement is erroneous, and could only have been imposed on him by some ill-informed or designing persons. He states, that a black "gin," at his station on the Macquarrie River, had a copper-coloured child by the European servant of a neighbouring settler; and that this "gin" afterwards joined her native husband and tribe, and had a perfectly black child, without any European trace. And he (Mr H.) saw both the elder or copper-coloured child by the European, and the younger or black child by the native father, with their mother, not more than six months ago, at his station. He says, moreover, that he would have no difficulty in adding several similar cases, which had come under his notice; but such a startling fact had never reached him, or he would have collected abundance of evidence to refute it.

That the native woman should sometimes become sterile after a period of concubinage with a European is not denied; nor is it to be wondered at, when we take into consideration the change of life which follows on placing herself under the roof of the white settler. From living in a state of nature, with irregular and uncertain diet, exposed to every vicissitude of climate, with no other protection than a few opossum skins, or a roll of bark, or of the "tulka," she enters on a more regular life, partakes of regular meals, and sleeps no longer exposed. But even with this alteration for the better, she does not bear to the white man more prolifically than she would to her native husband; on the contrary, her fecundity appears to decrease,—for, on partaking of the white man's comforts, she is a recipient of his vices; she passes much of her time in a half-inebriated state, smoking tobacco and drinking ardent spirits whenever they can be procured. Indeed, it is well known, that among the chief inducements for the native female to remain with a European, are the rum and tobacco with which she is supplied *ad libitum*. Can we then wonder, if, after some years spent in a manner which must militate against her capabilities for procreating, more than her previous rude mode of life, she returns to her tribe with a broken constitution, and probably past the usual term of life for conception (they seldom bear children after thirty years of age) to prove in such instances sterile?

We readily admit, that wherever our settlers are commingled with the aborigines in Australia, the native race disappears, and partly from the infecundity of the females. This, however, does not arise from any deviation of nature's laws, as is attempted to be proven; but because the European, wherever he takes his civilization, takes with him his vices also; and the miserable energy-striken aborigines (scarcely ever sober), noticed by the traveller so commonly around the settlements in the bush, afford too striking evidence of the cause of decline in the native tribes.

Nor must we omit to mention another and sad feature to account for the rapid decrease of the aboriginal population,—infanticide; which is now well ascertained to be very prevalent among them, and which is not likely to be diminished by their connection with the white people;—nay, we would suspect the reverse. The "gin," or native female, knows that her white paramour is anxious, rather than otherwise, to preserve the half-caste offspring, and she has no duties which prevent her attending to such; but when she rejoins her tribe, in addition to all her other labours, she must, if she procreates, be further burdened with her children; and it is to be feared that one great reason of their decline is the destruction of the helpless offspring, merely to avoid the additional trouble which they involve.

As well might Count Strzlecki, or any other authority, refer to the cause he assigns, the gradual disappearance of some of the South American tribes, after Pizzaro's invasion; or of the North American tribes since the United States have sent their hardy pioneers into the far west, as do so in the case of the aborigines of Australia.

We all know that many tribes of both North and South American Indians are extinct; and that among other causes of their decline there may have been some influencing their fecundity. But the principal causes have been, the altered habits, the diseases, the enervating and depraving vices, which the European has taken with him in his pretended civilization. That the various races of mankind can commingle and procreate with each other, without losing the capability of reproducing with their peculiar division of the species, is abundantly proven wherever the white man has turned his steps. Let it be North or South America, India, Africa, Japan, China, or Polynesia, the traveller will now find the half-caste mulatto, the Creole, the *medio-branco*,—too often the elder brother of the jet black, the brown or olive, thorough bred children, to confirm what is now confidently asserted, to hold good of the aborigines of New Holland.

1, Mona Terrace, Douglas, Isle of Man.
July 1, 1851.

ARTICLE VII.—*On Supernumerary Fingers and Toes.* By ROBERT CRAWFORD, M.D., Ratho.

THE following facts are worthy of being put upon record. They are curious and interesting in a physiological point of view, as illustrating the *paternal* influence on the *physical* conformation of the offspring, and afford an example of the rare occurrence, that a man may have by different women children with the same malformation, as observed by Luber and Meckel, and, what is more rare still, that a deformity of the *father* may be transmitted to his children, as remarked by Burdoch and Osiander,—nay, even to his children's children, disproving the notion frequently entertained, that monstrosities are dependent on the imagination of the mother, frights, &c. &c., and, I may add, are independent of any *original* malformation of the germ, which has been assigned as a cause of these aberrations of nature.

Mrs T., after a safe and speedy labour, was delivered of her second child, a strong healthy male, on 19th ult. The infant had two supernumerary fingers, one attached by a pellicle to the external aspect of the metacarpo-phalangeal articulation of each little finger. They were well formed, with nails and three phalanges each, but the tendons were in a very rudimentary state. Mr Craig informs me that his former assistant, Dr Legat, now in extensive practice at South Shields, attended Mrs T. in her first labour, and removed from the baby (a female) two supernumerary fingers similarly situated. I have ascertained also from Mr T., that he had two fingers removed in infancy from the same parts of the hands as his children had, the cicatrices of which he showed me. He has, besides, six toes on each foot. The supernumerary toes project from the dorsum of the middle metatarsal bones. All Mr T.'s brothers and sisters had more than the natural number of fingers or toes,—some both. His father, who had six toes on each foot, by a second marriage had a son and three daughters. There was nothing peculiar about the latter, but the son has the proximal extremity of both great toes single. They appear to bifurcate about the centre,—or rather, each forms, *from the centre*, two toes surrounded by one continuous skin, terminating at the distal extremity by two nails each. A sister of Mr T.'s, married, had four children, of whom one (a daughter) has six toes on each foot.

On the authority of a respected clerical friend, I also state the following facts, which he can personally verify, and which occurred in a parish where he at one time held the *cure* :—

A female relative (either a sister or aunt) of A. B., had a child with a supernumerary finger or toe on each extremity. A. B. married, he had eight children, two of whom had six fingers or toes on each extremity. A young woman in the neighbourhood accused A. B. of being the father of a child with which she was pregnant;

but this he strenuously denied. The woman brought forth twins, and each had six fingers on each hand, and six toes on each foot,—certainly a remarkable coincidence, if the woman's accusation was false; but more probably a strong presumptive proof of the paternity of the offspring. I may add an instructive medico-legal fact, which occurred in the same locality, and which I state on the same authority. A theft was committed, and the authorities, while investigating the matter, detected the impression of a foot on the soil; it presented the appearance of *six toes*, and was traced to the son of A. B. without any difficulty.

Ludgate Lodge, Ratho, August 10, 1851.

Part Second.

REVIEWS.

On Enlargements of the Spleen, observed during an Epidemic of Intermittent Fever, which attacked the 18th Regiment, Madras Native Infantry, in Goomsoor, in 1847-48. By G. SMITH, M.D., Assistant-Surgeon, Madras Medical Service. Contained in Medical Reports selected by the Medical Board from the Records of their office, and published under the sanction of Government. Madras, 1850.

THE Medical Board of the East India Company's Service in Madras have from time to time published valuable reports by the medical officers of that establishment, on important subjects connected with the pathology and treatment of the diseases of hot climates, and on the utility of various new remedies. In the series contained in the volume now before us, there are several interesting documents on the health of various corps in the Madras Presidency, and on the medical topography of some of its districts. One of the most interesting, however, of the whole is the paper of which the title is given in detail at the head of this article. It is important in several respects, but more especially at the present time, as supplying impartial evidence for testing the theory which has been for some time supported by M. Piorry and his followers, regarding the pathology of intermittent fever.

According to Piorry, intermittent and remittent fever own a distinct anatomical cause, as they depend on an enlarged condition of the spleen. He maintains, that in all distinct cases of intermittent fever the spleen is more or less enlarged in volume; that he

has found it enlarged in 154 cases out of 161, and painful in 4 of the remaining 7; that after two or three attacks of intermittent, the spleen is always observed to exceed its natural dimensions; that it is more voluminous during the paroxysms than during the intermissions; that the paroxysms diminish in severity as the spleen gradually regains its ordinary size; and that, under the use of large doses of quinine, its morbid enlargement is reduced with great rapidity. Were these facts all genuine, the inference would be almost irresistible, that intermittent depends on enlargement of the spleen; and that quinine cures the disease by a specific action, which quickly reduces congestion of that organ.

This doctrine of Piorry has gained many converts in France, but few in this country; yet we are not aware that it has been fairly put to the test by any actual observations in Britain. Our experience on the subject is necessarily limited, because few cases of intermittent are observed in Edinburgh. But among those which are occasionally met with in the persons of Irish railway labourers and reapers, who have contracted the disease in the fenny parts of Lincolnshire and Cambridgeshire, we have been seldom able to observe any unequivocal enlargement of the spleen, and on only one occasion could remark an enlargement during the fit, and contraction after the sudden arrestment of the disease by a few large doses of quinine.

The experience of Dr Smith, on a most extensive scale of observation, appears to us to set the question at rest against the views of Piorry. Few have been more favourably circumstanced for carrying out such an inquiry. Goomsoor, the station where his observations were made, is regarded as the Walcheren of India. Here, in a native regiment with an average strength of 900 men, he had to treat no fewer than 4000 cases of intermittent in ten months. In the single month of May 38 per cent, and in the following month 44 per cent of the strength of the regiment were admitted into hospital on account of this disease; and in the ten months, 7 per cent of the whole force died from that cause alone. Particular attention was paid to the condition of the spleen, which was examined, according to Piorry's method, by the pleximeter; and the following were the results. In 100 cases of quotidian and tertian fever examined within three months from the commencement of the epidemic, the spleen exceeded its natural size of four inches in length only twelve times; and in not one of nearly 700 cases was there any complaint of pain in the splenic region, even when smartly struck with the pleximetric hammer. Although the frequency of splenic enlargement increases with the frequency of the recurrence of the disease, still the organic affection is far from invariable even after many attacks. In 11 primary cases Dr Smith did not detect any enlargement; in 19 cases of a second attack he observed it once only; in 25 cases of a third attack he witnessed it thrice; and in 45 cases, in which the disease had recurred oftener than thrice, he met with

it eight times. Of 200 cases, taken indiscriminately at the close of the epidemic from the class of patients who had been repeatedly ill, there were only 35, or 17¼ per cent, who were affected with appreciable enlargement of the spleen. He could never remark, in cases in which the spleen was previously healthy, that its size increased during the paroxysm, and decreased again during the intermission; but this alternation was remarked in those who had enlarged spleen when attacked. Neither could he remark that the intensity of the paroxysms bore any relation to the size of the spleen. His observations as to the influence of quinine on enlargement of the spleen are acknowledged by him to be not sufficiently numerous to test Piorry's statement effectually; but out of 17 experiments with large doses, he remarked no difference in 6, but a greater or less diminution of the spleen in 11.

A few well-observed negative facts are sufficient to throw very great doubts on the theory, that intermittent depends on congestion, or other disease, of the spleen. But negative results, so numerous and on so extended a scale of observation, upset it entirely. Of course, no one doubts that the spleen is often much enlarged in the course of intermittent and remittent fever. This fact however is evidently, from Dr Smith's inquiries, nothing more than what it was always supposed to be until Piorry's researches:—the spleen is enlarged consecutively, not primarily; its enlargement being the effect, not the cause, of the fever. It is to be regretted that the author had no opportunity of investigating the anatomical characters of the disease of the spleen at its different stages. No doubt, however, can be entertained that for a long time it consists of congestion only. This appears sufficiently from the ease and rapidity with which it may in general be cured. In some cases, even in persons who had suffered often from intermittent, the mere arrestment of the fever, with the subsequent use of laxatives and diaphoretics, removed very considerable enlargements in periods varying from five to ten days; in one instance a cure was effected in sixteen days, though the edge of the spleen was at one time an inch and a half below the umbilicus; and occasionally the cure takes place quite suddenly, the spleen returning to its natural size within four-and-twenty hours, though it may have remained much enlarged for several days after the cessation of fever. When these simple measures do not appear in a few days to promise speedy recovery, the remedies principally to be depended on are chalybeates, and the stimulus of pressure and kneading. Dr Smith usually gave iron in the form of sulphate, and along with aloes and iodine. Two grains of sulphate of iron, two of aloes, and one of iodine, were given twice or thrice a day, in the form of pill, made up with aromatic confection; and twice or thrice daily the enlarged organ was subjected to firm kneading with the hands. The efficacy of this treatment was tested by subjecting some severe cases in the first instance to diet, regimen, and laxatives alone,

without avail. Even where there is reason to suppose, from the long duration and great extent of the enlargement, that the disease must have advanced partially to the stage of hypertrophy, a cure may be accomplished by the same means; but then it is tedious, and may extend through several months.

While it appears that few can resist the tendency to spleen disease in localities so favourable as Goomsoor to the production of intermittent, Dr Smith mentions some interesting facts, in proof that some constitutional tendencies must predispose to visceral disease in such circumstances, and others operate as a protection. It is remarkable that in the epidemic he describes, the European constitution seemed less inclined to disease of the spleen than that of the natives. Of twenty-three European officers, twenty had fever, but none of them suffered from enlargement of the spleen. And he adds the case of an officer, who had constantly remained exempt during repeated attacks of severe ague, throughout a period of twenty years. Age diminishes the predisposition; for the disease occurred almost entirely among the young and middle-aged sepoys, the old members of the regiment, who suffered from fever, being very rarely observed to suffer from enlargement of the spleen.

Œuvres Completes d'Hippocrate. Traduction nouvelle, avec le texte Grec en regard, collationné sur les manuscrits et toutes les editions, accompagnée d'une introduction, de commentaires medicaux, de variantes, et de notes philologiques, suivie d'une table générale des matieres. Par E. Littre, de l'Institut, &c. &c. T. I.—VII. A Paris, chez J. B. Ballière.

Collection des Médecins Grecs et Latins publiée sous les auspices du Ministere de l'Instruction publique, conformément au Plan approuvé par l'Académie des Inscriptions et Belles-Lettres et par l'Académie de Médecine. Par le Dr Ch. Daremberg.

Œuvres d'Oribase, Texte Grec, en grande partie inedit, collationné sur les manuscrits, traduit pour la premiere fois en Français, avec une introduction, des notes, des tables, et des planches. Par les Docteurs Bussemaker et Daremberg. T. I. A Paris. Imprimé per autorisation du Gouvernement. MDCCCLI.

The present age is so much an age of progression that it is not to be wondered at if, to a certain extent, unfounded prejudices should prevail against historical research and the study of antiquity, as having, it is supposed, a tendency to give a false direction to the pursuits of those who assiduously cultivate them, and thereby to retard, rather than promote, that advancement which all look forward to and desiderate. In most instances, however, there can be no doubt, that an affected contempt of ancient authorities is the

veil with which ignorance seeks to conceal from self-love a sense of its own defects, and that no one pretends to undervalue the intellectual treasures of former generations but he who, from indolence, incapacity, or want of instruction, is debarred from the use of them. At the same time it must be admitted, that in the medical profession, individuals possessed of the most shallow attainments often attain a measure of success far superior to those who are deeply skilled in its literature, and it is natural for them to undervalue accomplishments the want of which they have never felt as any barrier to their worldly prosperity. Accordingly, many practical men (as they hold themselves to be), finding it more profitable to devote their leisure hours to society and female gossip than to literary pursuits of any kind, readily affect a great contempt of book-learning, as being the amusement of those alone of their professional brethren whose time is not more profitably occupied; whereas they, forsooth, are so busy that they have no leisure to read books, or if they had, they do not see what benefit could be derived from this occupation of their time. Such persons, then, pretend to regard the mental condition of the man who busies himself in surveying the labours of our forefathers, as resembling the figures of the soothsayers drawn in the "Inferno" of Dante, who are represented as having got a twist at the middle, whereby their faces and upper parts were turned in the opposite direction to their inferior extremities, and thus they looked one way and walked in the opposite direction; for, as the poet feelingly laments,

"Il veder dinanzi era lor tolto."

If it were really true that a deferential regard for the wisdom of our forefathers had a tendency to check the progress of improvement, we should certainly be the last to recommend the study of medical history; for we look with great contempt on the man who is ignorant of the present state of knowledge in his own profession, and despairs of its ever making any further advancement. We are well convinced, however, that instead of checking progression in the medical art, nothing has so great a tendency to promote it as a proper attention to the great authorities of antiquity; for it will be found on investigation, that in modern times, those who have raised to themselves the most enduring reputations, and have contributed most to advance knowledge in their profession, have generally been men remarkable for the enthusiastic devotion which they paid to the genius of our ancient masters. For example, in Britain we all acknowledge Sydenham as the *coryphœus* of our order; and Sydenham, it is well known, looked up to the opinions of "the divine old man of Cos" as being absolutely oracular on professional matters. Of all the German authorities in medical literature, we are inclined to rank Richter as the first; and we may take this opportunity of mentioning, that, having lately read his works with

some attention, we were much struck with the coincidence between his views on many therapeutical points and those which have only of late obtained credit in this country. We allude, in particular, to the mild system of treatment, in contradistinction to the heroic method which prevailed in our earlier days. Now, Richter shows himself on all occasions to have been familiarly acquainted with the treasures of the ancients, and, like our Sydenham, appeals to Hippocrates as the highest of our medical authorities. There is no recent name in medicine of which France has greater reason to be proud than of Laennec, and we need not say that he admits having found a nearer approach to his own method of diagnosis in the Hippocratic treatises than in all the other works on medicine down to Avenbrugger, his immediate forerunner. But let us turn to present times, and to our own country. To our honour it can be said, that among our physicians there are not a few well versed in classic lore, who, not content with the more modern of professional traditions, study for themselves the works of the great founders of our art. Nor can such occupation of leisure hours be deemed unfruitful, even though much of the knowledge obtained be inapplicable to direct practical purposes in the present day. These studies have a liberalising and ennobling tendency; and examples are not wanting to prove, that a genius for original discovery in medicine is by no means incompatible with considerable enthusiasm in the prosecution of subjects involving literary research and archæological investigation.

With such illustrious examples on our side, we are not ashamed to admit ourselves to be *laudatores temporis acti*, and it has afforded us much gratification of late to remark, both in this country and on the continent, a growing conviction of the importance of medical history, and the study of ancient authorities. In proof of this, we need only refer to the institution of the Sydenham Society in this country; for the professed object of its founders was to diffuse a knowledge of the older writers on medicine; and of the volumes issued by the council during the first five years, it will be remarked, that not fewer than six are devoted to Greek, Roman, and Arabian medicine, and one to the medical literature of the Middle Ages. Although a taste for the ancient literature of medicine, then, be by no means so generally diffused among us as we could wish, we certainly think there are unmistakeable indications that it is on the increase.

The publications which we have placed at the head of this article, afford undoubted proof that our brethren in France are animated with an equally strong spirit for the cultivation of ancient medicine. Of M. Littré's edition of Hippocrates we need say little, since its reputation is now fully established all over Europe. We have no hesitation in pronouncing respecting it, that it is a work which ought to be in the hands of every medical student who has any proper curiosity to examine carefully the *incunabula nostræ*

artis. But it is to the other work that, on the present occasion, we wish more especially to direct the attention of our readers, as being in our judgment one of the most important accessions which the literature of medicine has obtained in the present age. We proceed, then, to give our readers a short account of this publication.

Dr Daremberg, of Paris, a name now well known in all the learned circles of our profession throughout Europe, has been actively occupied for several years past in accumulating materials for publishing a complete collection of the medical works of the Greeks and Romans subsequent to Hippocrates. When we take into account the extent of the preparations already made by him, in order that he may be enabled to perform this task in an adequate manner, together with the number and respectability of his coadjutors, we are not afraid to give it as our decided opinion, that it is an undertaking in the success of which the medical profession all over the world is deeply interested. The labours of Kühn, in the same line, commenced at Leipsic, about thirty years ago, it is well known, have disappointed the just expectations of all true scholars, so that the present undertaking is nearly as great a desideratum as if the edition of Kühn had never made its appearance. Kühn's editions, in fact, are deficient in every requisite of a good edition; the typography is of a very inferior stamp, the text often in a very dubious state, there are no *lectiones variantes*, and no notes or commentaries, if we except the edition of Dioscorides, the publication of which was entrusted to Karl Sprengel, and which is not unworthy of his high reputation. On the other hand, Dr Daremberg is fully prepared to supply all the deficiencies now adverted to as occurring in the editions of Kühn. For example, with respect to the works which he is about to publish, he has consulted personally all the MSS. and editions which exist in the great public libraries of Great Britain, France, Germany, and Italy, and further meditates investigating the contents of the unexplored shelves of the Escurial, Morocco, Fez, Ispahan, and Teheran. He has already enlisted in his services most of the learned men of Europe, who possess any reputation in this line. He spares no pains in illustrating, with learned annotations, the subject-matters treated of by his author; and he has the advantage of having his work printed at perhaps the most elegant Greek press in the world. The whole publication, moreover, is entirely divested of the mercantile spirit. It is in fact a great national undertaking, recommended by learned societies, patronised by the minister of Instruction, and liberally supported by every government of the day, whether monarchical or republican. And here we would beg to digress for a moment to express our regret that Great Britain is so far behind most of the nations of Europe, in regard to educational institutions. Here we have no minister of Instruction, and consequently our government has no member whose business it is to patronise any liberal project

which is calculated to promote the interests of literature in the country. On this side of the channel, then, there is not the slightest probability of any such publication as that of Dr Daremberg being ever attempted, either by private enterprise or public spirit, for no individual connected with the profession can be supposed likely to possess the means which so vast an undertaking would require. No bookseller would think of embarking in a speculation so little likely to prove remunerative; and we have no public body nor minister who will come forward and supply the necessary funds for a national purpose of this kind, however important. We must admit, then, in the present instance, that "they manage these matters better in France."

Of this great collection the first volume has just issued from the press, consisting of the first six books of the "Collectanea Medicinalia" of Oribasius, under the joint editorship of Drs Bussemaker and Daremberg; and we must say that a more judicious selection of an author to commence with we cannot well imagine, whether we regard the intrinsic value of the matters contained in these books, or the difficulty which formerly existed of procuring access to them. On this subject we must now say a few words.

Most of our readers are probably acquainted with the chief particulars which are known respecting the personal history of Oribasius, for which our principal authority is the meagre biography given of him by Eunapius, along with the imperfect analysis of his works contained in the "Bibliotheca" of Photius. He was born about the middle of the 4th century at Pergamus, the native place of the still more illustrious Galen, so that, according to his biographer, the celebrity of his birthplace contributed not a little to the early development of his reputation. He was physician to Julian, surnamed the Apostate, who addresses his 17th epistle to him; and although he was banished by Julian's successors, it would appear that his celebrity soon procured his recall, and that he enjoyed domestic happiness and professional distinction during the remainder of a long life. Of his medical works, which were very voluminous, the most celebrated is his Ἰατρικαὶ συναγωγαὶ, "Collectanea Medicinalia," which consisted originally of seventy books, scarcely the half of which have come down to our time. Of these, the first 15 books were published at Moscow in 1808, by Matthæi; but this edition, even if it could be procured, would appear to be very incomplete, since it wants all the numerous extracts from Galen, Dioscorides, and Rufus; and, moreover, as the greater part of the impression was consumed in the memorable conflagration of Moscow, a copy of it is one of the greatest rarities imaginable. The 24th and 25th books, containing a very interesting compendium of Galen's works on anatomy, were printed in an elegant form at Paris in 1556, and reprinted at Leyden in 1735. Extracts from the 46th and 47th books are published by Cocchi in his "Chirurgici Græci," and there are mutilated portions of the 44th, 45th, 48th,

49th, 50th, and 51st books in Mai's fourth volume of "Classici Auctores." Altogether, then, but a small portion of the extant works of Oribasius have ever been published in the original, and to the large amount of valuable information contained in the remaining books, our only medium of access is the Latin translation of Rasorius, executed at Venice in 1553, and published among the "Medicæ Artis Principes," by Henry Stephens. Even through this imperfect medium, the value of their contents has been in a certain degree appreciated by Freind, in his "History of Medicine," and by the commentator on Paulus Ægineta, in the English version of his works published a few years ago by the Sydenham Society. To the learned members of the medical profession, then, the present publication must prove invaluable. When we take further into account, that the interest it is calculated to excite is not confined to them; and that the philologist, the antiquarian, and the philosopher will find, each in his own department, abundance of materials to engage his attention, we cannot fail to regard it as one of the most important phenomena which have lately arisen in the literary world.

The circumstances of the present time would also appear to be particularly auspicious to the regeneration of the study of ancient authorities in medicine. The routine course of medical practice, based on fanciful hypotheses, and fostered by an ignorant contempt for the opinions of our forefathers, is rapidly giving way to a more rational system, founded on unbiassed observation and the results drawn from the accumulated experience of bygone ages. In such a state of the professional mind, it is difficult to conceive that anything could be presented to it more seasonable than the subject-matters treated of in the first six books of Oribasius, now for the first time made fully available to the profession. We have here presented to us, in a condensed shape, the established opinions of the Greek and Roman physicians, from Hippocrates to Oribasius,—that is to say, for the space of 800 or 900 years,—with regard to the characters of the most important articles of food and drink used by them, and the various modes of exercise which they practised for the preservation and restoration of health. Surely, if in any case much importance is to be attached to the accumulated experience of distant ages, it is on such a subject as hygiene; and it will become the duty of the profession seriously to consider its results, as here recorded by the highest authority in ancient medicine after Galen. All the six books are interesting, but more especially the 6th, which treats of exercises, and is illustrated by copious annotations and figures borrowed from Mercuriali and Krause.

Altogether, we look upon the volume which has just been issued as almost faultless in its way. The original text is given at the top of the page in a most elegant type, there is a French version below, and *lectiones variantes* at the foot. On the margin of the text, references are given to the parallel pages of Hippocrates and

Galen, and on the margin of the translations the names of the subject-matters treated of by the author. At the end of the volume are copious annotations explanatory of the text, and embracing a variety of interesting matters in medicine, criticism, and philology. There is also a copious table of the contents of the chapters, and an index to the principal terms explained in the notes. If there is anything in the volume which we could wish different from what we find it, it would be, that the contents of the annotations were given in a somewhat more condensed shape, for the style of Dr D.'s commentaries is, like that of his countryman M. Littré, rather diffuse. But our necessary limits compel us to draw to a close. In conclusion, we would repeat that in our estimation this edition of Oribasius is second to no edition of a medical author that has ever appeared. We are pleased, indeed, to remark, that Dr Daremberg candidly admits that in getting up his work he has taken the Oxford edition of Theophilus (*de corporis humani fabricâ*) as his model; and certainly, in our opinion, he could not have chosen a better. The English editor, like his French imitator, evidently undertook the task with a conscientious determination to do his best; like him, Dr Greenhill had the command of extensive libraries, and a most elegant press, and gratefully availed himself of whatever assistance he could procure, from contemporaries of congenial habits of study. In the lack of learned editions of the ancient writers on medicine in this country, it is some consolation, then, that we can boast of one performance in this line, which our more learned brethren on the Continent regard as a model for their imitation. We venerate scholars of this stamp, who, from a disinterested love of learning, prosecute studies which they must be well aware will bring them neither personal gain nor public honours at all commensurate to their labours, and who seek no other reward but what they derive from the inward sense of intellectual improvement, and the conviction that they have contributed a notable service to the republic of letters. But verily they *have* their reward!

Part Third.

PERISCOPE.

PHYSIOLOGY.

EXACT (?) METHOD OF CALCULATING THE AMOUNT OF BLOOD-CORPUSCLES AND PLASMA IN 1000 PARTS. BY G. ZIMMERMANN.

The author believes that he has obtained the solution of the much-agitated problem,—How to discover the amount and composition of the moist blood-cells in a given case?—by the following procedure, and that he has thereby set aside

all former methods, even that of C. Schmidt, which he considers to be founded on erroneous assumptions. He starts with the doctrine, that nitrate of baryta, which, like other salts, has the property of preventing the coagulation of the blood, does not, while the corpuscles subside, pass into the blood-cells, in virtue of the ordinary laws of endosmosis and exosmosis. He has chosen nitrate of baryta, both because the blood contains no salt of baryta, and because the addition of nitric acid does not interfere with the quantitative determination of the blood salts. He mixes a weighed portion of blood with a certain proportion of solution of nitrate of baryta, of known strength. When the blood-cells have subsided, he determines the amount of solids contained in the serum and cruor, and hence calculates the solid contents of the whole blood by subtracting the weight of baryta salt which has been mixed. He also determines the amount of nitrate of baryta in the serum, and finds, by subtracting this from the previously ascertained total serous solids, the amount of the true dried constituents of the plasma. Then he finds the amount of nitrate of baryta in the cruor, and how many parts of serum this corresponds to. Finally, by subtracting this proportion of serum from the cruor, he determines the weight of the solids of the blood-corpuscles. If from another portion of blood the amount of fibrine has been determined, it is easy to calculate the per-centage of the different blood-constituents in 1000 parts. If the proportions be sought by estimating the amount of chlorine in the serum and cruor, according to the assumption of Berzelius, that the blood-cells contain no chloride of sodium, a smaller amount of blood-cells containing more solids than water is obtained. Hence he concludes that the blood-corpuscles do contain chloride of sodium, and that his method of estimating their amount is the more exact. As proofs that the blood-corpuscles absorb no nitrate of baryta, he adds:—(1.) That the blood-corpuscle will not take up every salt; (2.) That baryta is an element quite strange to the blood-cells, and hence unfit to enter them; (3.) That the salts of the blood-cells would immediately form insoluble compounds with baryta.

The commentator in Schmidt's Jahrbuch remarks, that the term "exact" cannot be correctly applied to this mode of analysis, and that the truth of the premises on which it is founded may be denied by those physiologists who admit, like ourselves, that the blood-corpuscle is subject to the ordinary laws of exosmosis and endosmosis. For as nobody who has studied the nature of a cell can doubt that the blood-globules not only obey the laws of endosmosis, but that the very performance of the physiological functions generally ascribed to them would without exosmosis be impossible, the commentator inquires what right Zimmermann has to assume for them any extraordinary properties exempting them from known physical laws. Endosmosis seems to be a uniform law of nature, depending upon the condition of a membrane and of solutions on either side of it. When Zimmermann asserts, that the living blood-cells contain no baryta, because none can be detected in the blood, it is a truism; but he does not offer a single proof that they are incapable of absorbing it from the intercellular fluid when it is dissolved there. When he asserts that the blood-cell will not receive every salt, he asserts what he has not proved; and even were this proposition granted, he has not proved that the baryta salt cannot be taken up by the corpuscles. Although he says that the baryta would form insoluble compounds with the salts of the cells, he does not explain how this should hinder the imbibition of baryta salts according to the ordinary laws of exosmosis. It cannot be shown by direct experiment that the baryta *does* enter the cells, although the commentator deems this probable,—at all events far more so than that the cells exclude it,—and until Zimmermann has shown this to be the case, the exactitude of his mode of analysis must be doubted.—*Schmidt's Jahrbücher*, Aug. 1851.

[*Note.*—The following example may illustrate the mode of making the calculation proposed by Zimmermann:—

1st, Let it be ascertained what percentage of dry fibrin the blood yields:—suppose 1000 parts yield 3.

2d, Mix 1000 grs. blood with 1000 grs. water, and 60 grs. of baryta salt. Evaporate a portion—say 100 grs.—of the clear supernatant fluid to dryness at 212°, and let the amount of dry residue = 10 grs., of which 4 grs. are found by analysis to be baryta salt. The remainder of the dry residue = 6 grs. of serous solids+fibrin. Of the 90 parts of water driven off, $\frac{4}{60}$ths of 1000 = 66.66 must be estimated as derived from the baryta solution, and 23.33 as belonging to the serum.

3d, Evaporate the remainder of the blood = 1960 grs. to dryness at 212°, and suppose the solid residue is found = 266 grs.; whereof, of course, 56 grs. are baryta salt. Then the blood solids in this portion are 266—56 = 210 grs., and the composition of 1000 parts would be estimated by Zimmermann as follows:—

Total solids in 1000 parts = 210 + 6 = . .			216
Water in do. = 1000—216 = . .			784
			1000
Fibrin = ...	3		
Serous solids (4 : 6 : : 60 : 90) = 90—3 Fib. =	87	= 440	Plasma.
Water of serum (4 : 23.33 : : 60 : 350) = .	350		
Dry blood-cells = 216—87—3 = .	126	= 560	Corpuscles.
Water of blood cells = 1000—126—440 =	434		
	1000	1000	

W. R.]

UPON THE QUANTITY OF PHOSPHORIC ACID EXISTING IN HEALTHY URINE.

BY DR D. BREED.

Notwithstanding the importance attached to every increase of our knowledge relating to physiology and pathology, an extended examination of the quantity of phosphoric acid in the urine has not been hitherto carried out by either a chemist or physician. A method, given by Professor Liebig, for determining phosphoric acid accurately and quickly, enabled the author to supply this deficiency, and to elicit some facts which may be of importance in the treatment of diseases.

This method consists in treating the urine with a solution of known strength of perchloride of iron until a small quantity of the mixture, when filtered and tested with ferrocyanide of potassium, gives a blue colour. It is based upon the fact, that a liquid containing phosphoric acid, both when neutral and when acidified with acetic acid, yields an insoluble precipitate with a solution containing peroxide of iron. The solution of perchloride of iron is best prepared by dissolving 15·556 grms. of iron in muriatic acid with the addition of nitric acid, evaporating to dryness in the water-bath to remove the excess of acid, and then dissolving the remaining salt in 2000 cub. centims. of water. 1 cub. centim. of this solution precipitates 10 milligrms. of phosphoric acid. Instead of this solution of perchloride of iron, one of indefinite concentration may also be used, the strength of which may be ascertained by testing with a solution of phosphate of soda, the quantity of phosphoric acid contained in which is known. The solution of perchloride of iron used must be perfectly free from the protochloride.

If the urine, the amount of phosphoric acid in which we are about to determine, has acquired an alkaline re-action from the decomposition of the urea, a portion of the phosphoric acid may have been precipitated in combination with lime or magnesia, and the precipitate must then be dissolved by the addition of a few drops of muriatic acid. The urine is measured and well shaken; a measured quantity, about 100 cub. centims. (or more) is drawn off with a pipette, and placed in a beaker, and acetate of soda (in considerable quantity if muriatic acid has been added) and acetic acid are added. The solution of the perchloride of iron is then cautiously added, and the mixture is frequently tested to ascertain if all the phosphoric acid is precipitated, until a trace of the iron solution has been added in excess. To detect the latter, a piece of filtering paper

imbued with ferrocyanide of potassium is placed upon a white porcelain surface (or upon a glass plate with a piece of white paper lying beneath it), and a doubled piece of filtering paper pressed against it with a glass rod from which a drop of the urine is hanging; if the urine contains an excess of the solution of iron, a blue colour is produced within from three to four seconds. The quantity of the solution of iron used is noticed. Two other portions are treated in exactly the same way; if the results agree, the quantity of solution of chloride of iron which would have been required for the entire quantity of urine is calculated, and the amount of phosphoric acid corresponding to this quantity of the solution of iron is contained in the urine, and is then ascertained. By this method, a physician who has but little spare time can easily make several determinations of the phosphoric acid in urine every day.

The quantity of phosphoric acid in the urine of a man in health and living a regular life, is nearly constant; but recent experiments have shown that the mode of living, as also diseases and medicines, cause a variation in the amount of phosphoric acid in the urine.

The following mean results were obtained by the author. (We omit the individual data):—

A. *Urine of Twenty-four Hours.*

1. *Urine of four different Persons* (*but mostly one and the same*).—On the average, 1000 cub. centims. of urine contain 2·317 grms. of phosphoric acid, or in twenty-four hours we have 1610 cub. centims. of urine and 3·732 grms. phosphoric acid.

2. *Urine of a Man who drank a large quantity of Water.*—1000 cub. centims. of urine correspond to 2·027 grms. of phosphoric acid, or for twenty-four hours we have 2086 cub. centims. of urine and 4·228 grms. of phosphoric acid.

3. *Urine of a Man who drank but little Water.*—1000 cub. centims. of the urine contain 4·062 grms. phosphoric acid, or for the twenty-four hours we have 988 cub. centims. of urine and 4.015 grms. of phosphoric acid.

B. *Urine passed during the Day and after Sleep.*

1. *Urine of different Persons.—Urine after Sleep.*—2·284 grms. of phosphoric acid correspond to 1000 cub. centims. of urine. *Urine during the Day.*—2·763 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

2. *Urine of a Man who drank a large quantity of Water.—Urine after Sleep.*—1·755 grms. of phosphoric acid correspond to 1000 cub. centims. of urine. *Urine during the Day.*—2·130 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

3. *Urine of a Man who drank but little Water.—Urine after Sleep.*—3·599 grms. of phosphoric acid correspond to 1000 cub. centims. of urine. *Urine during the Day.*—3·979 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

C. *Urine passed before and after Dinner* (*excluding the hours of Sleep*).

1 *Urine of different Persons.—Urine before Dinner.*—2·239 grms. of phosphoric acid correspond to 1000 cub. centims. of urine. *Urine after Dinner.*—3·745 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

2. *Urine of a Man who drank a large quantity of Water.—Urine before Dinner.*—1·743 grms. of phosphoric acid correspond to 1000 cub. centims. of urine. *Urine after Dinner.*—2·820 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

3. *Urine of a Man who drank but little Water.—Urine before Dinner.*—3·244 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.—*Urine after Dinner.*—4·541 grms. of phosphoric acid correspond to 1000 cub. centims. of urine.

Dr Breed found phosphoric acid in a liquid which Professor Vogel obtained by

the operation of *paracentesis abdominis*. 3105 cub. centims. of the liquid gave 4·648 grms. of phosphoric acid. The urine of twenty-four hours from this patient contained only 1·2 grm. of phosphoric acid.—*Ann. der Chem. und Pharm.*, vol. lxxviii. p. 150, in *Chemical Gazette*, 15th September 1851.

MEDICINE.

CASES OF PARALYSIS CONNECTED WITH HYSTERIA. BY DR SANDRAS.

(The following observations are extracted from the "Cours Clinique" of the Hôpital Beaujon :—)

I desire to recall your attention to two patients whom you have just seen, and in whom affections quite dissimilar in form seem to be connected with the same general morbid condition—with chlorosis.

The first patient is a woman, 32 years of age, tolerably strong, and presenting the indications of a lymphatic temperament. She has never had any serious illness, but has had various symptoms which are to be regarded as premonitory of her present condition. When menstruation commenced, she was subject to palpitations and to fits of breathlessness, accompanied with pains at stomach, and loss of appetite, but these complaints were never severe, and promptly subsided. Five years ago, after some mental annoyances, nervous symptoms, complicated with vomiting, were observed, but in a few days subsided. Two years ago she underwent considerable fatigue, which was followed by numbness and tingling of the extremities, especially of the fingers; sewing became difficult. These symptoms disappeared in three weeks without treatment, and her health continued good till her present illness. In August 1850 she went with her mistress (she was a lady's maid) to Carlsbad. She says that the temperature of Carlsbad was cold, and that she frequently suffered from it. Fifteen days after her arrival there, she was seized with feebleness of the legs,—a permanent feebleness,—and so extreme in degree, as to render her gait unsteady. Almost at the same time, or immediately afterwards, she had severe pains of the stomach, loss of appetite, vomitings, vertigo, and divergent strabismus, accompanied, not with *double*, but with *triple* vision. She had no fever or other grave constitutional disturbance. The menses were disordered and diminished in amount; the patient says the discharge was thin. A month later the upper extremities became feeble like the lower; then speech became embarrassed, the ideas confused, the memory diminished, and the affections lessened in intensity, or almost abolished. Thus, on returning to France, she manifested no emotion or sign of pleasure on meeting her husband and child.

Different plans of treatment have been tried without success. One of the physicians, under whose care she was, recommended the country, and she passed three months there without using any medicine. During this time the squinting ceased; the other symptoms remained unaltered. Early in 1851, there was notable spontaneous amelioration; the embarrassment of speech and the confusion of intellect disappeared, the memory was partially recovered, and the affections seemed on the point of regaining their original force; but soon afterwards this happy progress of the disease was suspended, and the woman was placed under my charge in hospital. Her symptoms at this time were—weakness of the limbs, more especially of the legs, complete incontinence of urine, impaired eyesight, and slight deafness. The affections are far less strong than before her illness, but are not so thoroughly deadened as they have been. Memory is obviously weakened. The most careful examination of the spine was made, and no pain about the spinous processes, either spontaneous or excited by pressure, could be detected. The general health is good. No symptom referable to the digestive organs except constipation. Respiration is natural. A well-marked blowing murmur, synchronous with the first sound of the heart, is heard in the cardiac region, and extends along the aorta and carotids, where its intensity is even

augmented. There are no palpitations, but the patient is easily put out of breath. The menstrual discharge is said to be thin, but the periods are regular.

After the history of the case now detailed, you might naturally entertain some doubts as to the nature of the disease. Nevertheless, I at once decided on dismissing the hypothesis of having here to do with an organic lesion of the nervous centres. The absence of all pain in the course of the spinous processes, the general nature of the paralysis, the circumstance of the eyes, tongue, &c., being attacked, were inconsistent with the idea of spinal lesion. There are none of those grave symptoms which indicate disease of the membranes of the brain and cord. The disorders of the intellect and of the moral feelings, the strabismus, the hesitating speech, might indeed be symptomatic of disease of the brain; but you know the ordinary course of these organic lesions,—they do not usually retrograde; and yet this patient no longer squints, her speech is now very distinct, and the intellect far from being feeble, appears well developed. I consequently am of opinion that she labours under no organic lesion of the nervous central organs, and consider all her symptoms as purely chlorotic. In fact, they were at first associated with well-marked symptoms of chlorosis, such as I have witnessed in many analogous cases. In illustration, I may allude to the case of a young woman lately treated here, who, while extremely chlorotic, presented symptoms like those of our present patient, divergent strabismus, diplopia, numbness of the extremities, &c., all of which symptoms disappeared under treatment directed against the chlorosis.

In the present case I hope we shall be equally fortunate. I propose to assist the antichlorotic remedies by the methodical application of galvanism.

Our second patient is affected in a most unusual manner. She is a female, 22 years of age, little, thin, and slim, and of exceedingly nervous temperament. From her earliest childhood she has been almost always sickly, subject to pains in the stomach, to shortness of breath, palpitations, &c. In November 1850, while she was enjoying a satisfactory state of health, without apparent cause, without exposure to a low temperature, the extremities of the fingers suddenly became the seat of an extreme cold sensation, and assumed a dull red colour, with violet tinge of the nails. At the same time there was violent local pain, compared by the patient to that of whitlow. These symptoms continued, and soon became irregularly exacerbated. In fifteen days the fingers were of a dark violet-red colour, at certain points passing into black. The pains had become intolerable, and the slightest touch caused acute suffering, soon followed by violent convulsions, to which the patient had not been previously subject. The symptoms were most acute on the left side of the body. One of our most skilful surgeons was consulted; concluded that there was inflammation of the arteries; and ordered at different times the application of forty-four leeches to the palm of the hand, followed by cataplasms with laudanum, and draughts containing ether. The symptoms were somewhat mitigated, but not entirely removed. About the same time the tip of the nose assumed the same violet colour as the fingers. The patient was transferred to another part of the hospital, where these phenomena were regarded as symptomatic of heart disease. This opinion was, I am informed, founded upon the presence of violent palpitations and loud blowing murmur. Numerous bleedings were practised, cauteries and blisters were applied in the precordial region, and digitalis was given. These means seemed to calm the pains, but not otherwise to affect her condition; the palpitations became more urgent. About the end of January 1851, the circumference of the ear became cyanosed like the nose; and the patient relieved, but not cured, quitted the hospital, to be soon re-admitted to my department.

We now found, as had been formerly noticed, an intense blowing murmur with the first heart sound; but it was a soft murmur, of *maximum* intensity at the base of the heart and in the aorta, propagated along the carotids, ceasing when the patient exerted herself, and, in short, a purely chlorotic murmur. There were very strong palpitations, breathlessness on walking, humming in the ears, and vertigo, without any of those other symptoms which indicate organic disease.

The extremities of the fingers were still of a violet tint, which spread over the entire distal phalanges, and became fainter towards the hand. The tint was deeper in the right than in the left fingers, it extended rather further along the inner border of the fingers than in any other direction, and stretched along the inside of the little fingers to the hand and lower third of the fore-arm. The tips of the fingers appeared dry, wrinkled, and cold, their tactile sensibility was lost, and the patient could not even feel a pin rolled between two of her fingers, nor hold it when placed there. A puncture, if superficial, is not felt, but pain is caused if the pin penetrates deeply into the true skin. The pulp of the fingers is the seat of a pain, compared by the patient to that of whitlow; they are benumbed and cold. The pain shoots to the shoulder, without following the track of the vessels and nerves; it is at times most acute, and is then especially severe in the shoulder. The least touch on such occasions induces agony, and violent hysterical convulsion fits,—unaccompanied, however, with *globus*, and without any epileptiform character. These paroxysms of pain have been of late very frequent, occurring five or six times a-day. During the fits the finger tips become of a blackish violet, *and at the same time warm.* At other times they seem of a dull white, and absolutely bloodless; they are then quite cold, and the patient says "she no longer feels their presence." Finally, at intervals they resume the normal condition.

The right index finger presents none of these phenomena; and we have made a curious observation on it, by applying to the fingers during the fits compresses steeped in chloroform. On the sound index there was produced a phlyctena, while nothing of the sort was observed on other parts of the body, or on the other fingers to which the same topical irritant was applied.

Finally, I should add, that at times a cold perspiration bedews the extremities of the affected fingers, and seems to be poured out almost as fast as it is wiped away. It is proper to notice, besides, that the disease has sometimes passed from one side to the other,—from the right to the left hand, and reciprocally.

Nothing of the kind is observed in the toes, nor in any other part of the body, except the tip of the nose, which is violet, cold, and painless.

Here, then, we have a curious disorder, which it is difficult to range in any known category. The details above given will probably induce you to agree with me, notwithstanding the opinions of my able colleagues, who have already treated the case, in dismissing the supposition of organic disease of the heart or vessels; for we know of none which could cause these singular phenomena without termination, so mobile, so prone to vary in intensity and seat. Such symptoms, if induced by a disease of the heart, would be observed in all the extremities, in the feet, ears, and lips, as well as in the hands. A permanent obstacle to the circulation capable of inducing cyanosis would not fail to induce serous infiltrations likewise. To what, then, must we refer these disorders, which seem to have their exclusive seat in the capillary circulation and sensitive nerves of the skin? For my own part, I have no hesitation in announcing that we have here one of the thousand curious manifestations of hysteria, and especially of hysteria complicated with chlorosis, and I can conceive no other rational explanation of the features of the case. Consequently, I shall direct the treatment against the chlorosis, the probable cause of all the other symptoms, and hope soon to obtain an encouraging result.

Note.—These two patients have since passed from under my charge. The first treated by chalybeates, cold alkaline baths, good diet, and frequent applications of electricity, has in every point improved, and has gone to the country to complete the cure. The second left the hospital perfectly cured of her hysteria, chlorosis, and disordered circulation, as well as of the discoloured and painful condition of the fingers. The means employed to obtain this happy result were frequent applications of chloroform when the fingers were painful and cold, of laudanum when simply painful, of cold douche continued for several hours when pain and heat co-existed. During convalescence the use of an ointment containing morphia, and subsequently, when flexion of the joints ceased to produce pain in

the fingers or muscles, an ointment containing sulphate of strychnia; finally, and, above all, the incessant administration of preparations of iron along with the food. —*L'Union Médicale*, No. 105, 1851.

CURE OF ASCITES AND HYDRARTHOSIS BY IODURETTED INJECTIONS.

(*Translated from Journal de Médecine et de Chirurgie Pratiques*, July 1851.)

M. Costes, physician of the Hospital St André at Bordeaux, has published, in the *Journal de Médecine* of that city, two cases of uncomplicated ascites, treated by injections containing iodine.

The most interesting of these observations is the case of a rope-maker, fifty years of age, who, after repeated attacks of intermittent fever, observed that his belly became enlarged. In fact, on the 2d September 1850, M. Costes examined him, and found signs of a considerable effusion into the abdominal cavity. Paracentesis was several times performed, and each time the most minute examination proved that there was no appreciable organic lesion of any internal organ. On the 16th September, after withdrawing, by a puncture made in the point of election, 7750 grammes (about 250 oz. Troy) of fluid, M. Costes proceeded to inject into the peritoneal cavity the following mixture:—Distilled water, 100 grammes (1550 grs.); tincture of iodine, 20 grammes (310 grs.); hydriodate of potass, 2 grammes (30·5 grs.) The injected fluid was suffered to remain for about two minutes, and was then withdrawn through the canula. Immediately after the injection the patient complained of acute pain, especially towards the left iliac fossa. This pain soon subsided, and was then renewed with greater intensity; at the same time there were rigors, nausea, thirst, and dyspnœa; the belly became tense and tender, the skin bedewed with a cold perspiration, &c. A vessel of warm water was applied to the feet, an opiate cataplasm to the abdomen, and in the evening 5 centigrammes (·77 of a grain) of extract of opium were prescribed. On the 17th, twenty-four hours after the injection, there was notable amelioration; the patient had had some sleep; the pain of the belly, though still present, was no longer acute; the fever had abated. On the 25th the patient's state was most satisfactory. Œdema of the legs was observed, and treated by proper bandaging. On the 1st October the œdema had diminished, the belly resumed its normal size, and palpation no longer detected the presence of fluid. On the 27th October the patient left the hospital, and on his return in May (?) 1851 it was found that his health had continued good.

In the second case, that of a woman, the effusion re-accumulated after the operation. The injection was not repeated; and as the subsequent treatment consisted of the repeated use of 30 centigrammes (4·6 grains) of gamboge, and of lemonade containing 8 grammes (℥ijss.) to the litre (1·76 imperial pint), the cure which followed cannot be cited as due to the injection. M. Costes adds, that he has recently performed the operation in a third case in his clinique, and has obtained a result so successful as to remove all doubts of the value of the procedure.

Remarks by the Editor of the Journal de Médecine.—Clinical observations so important as these of M. Costes cannot be passed over in silence. We have considered it proper to mention them, in order that they may be associated with those which MM. Dieulafoy, Leriche, Greffon, and Rul-Ogez have already communicated to the medical world. It is thus that science is promoted, and possibly the day is not far distant when we may assent to the proposition of the illustrious physician of Bordeaux,—that to cure idiopathic ascites recourse may simply be had to the ioduretted injections. One incontestible fact is, that the introduction of an irritating fluid into serous cavities is not so dangerous as was formerly believed. In 1829 M. Jobert de Lamballe had an opportunity of convincing himself of this, when he injected into the peritoneum of a woman suffering from simple ascites a large syringe-full of water, containing *eight ounces of alcohol.* It is true that this surgeon did not dare, in the first instance, to empty the cavity, and allow the injection to act upon its naked walls, but contented himself with adding the injection to the effused fluid. This operation, assisted by the mani-

pulation of "ballottement," immediately produced a sensation of heat, and even of acute pain, in the part where the column of alcoholised fluid impinged most directly. The curious observation was also made, that the patient continued all day in a thorough state of intoxication. No trace of inflammation ensued, and the patient, of whom, unhappily, nothing is known since she left the hospital, was cured at the period when she was dismissed. Since that period M. Jobert has been in the habit of injecting diluted alcohol and tincture of iodine into various joints, without ever inducing untoward symptoms. The introduction of the irritating fluid does not seem to be *inert;* it sometimes causes excessive pain and an assemblage of local symptoms which would terrify one who did not know, that all this disturbance rapidly passes off (s'humanise) without recourse being had to the ordinary antiphlogistics apparently indicated. We lately saw an example of this, in a case of hydrarthosis of the knee, treated in the Hotel Dieu by the surgeon just named,—a most interesting case, which cannot be more appropriately introduced than in this place.

About the end of April, M. Jobert de Lamballe received under his care, in the Hotel Dieu, a jeweller, forty-six years of age, with an old swelling of the left knee. Fluctuation, and the peculiar crepitation termed "bruit de chaînon," described by Dupuytren in his account of certain cysts of the fore-arm, and attributed to the presence of albuminous corpuscles, were distinctly perceptible. The patient could not walk without pain. On the 21st May, M. Jobert, having previously, without much benefit, employed discutient compresses, punctured the joint on the inner side with a trocar, and injected through the canula equal parts of distilled water and tincture of iodine. He left a portion of the fluid in the joint, and withdrew the rest; a piece of diachylon was applied to the orifice to prevent the access of air, the patient was then laid in the horizontal posture, and absolute rest of the limb maintained. At the moment of the injection there was a moderate amount of pain, but next day the joint was so swollen and red that fears were entertained of the occurrence of gangrene. This was the result of excessive exhalation of albuminous fluid. Now, as nature is very powerful in rapidly producing absorption of such effusions, the local symptoms were soon observed to become less intense, no constitutional disturbance supervened, and by the 23d the patient had slept and felt himself better. The tumefaction and redness were lessened. On the 30th May pain had ceased, and the knee had recovered its normal volume, except at the inner and upper part, where albuminous masses were still perceptible. These M. Jobert is at present attempting to remove by the continuous application of compresses steeped in lime water and solution of sal-ammoniac. At present, on the 28th June, the man is in the most satisfactory state; he walks without pain, and will be cured like another patient, a lady, on whom M. Jobert recently performed the same operation for the same complaint. She had suffered for six months, and the treatment by injection was proposed. She at first refused to submit, but the operation was at last performed in presence of MM. Velpeau and Delafolie. The pain was excessive, and the patient was thrown, by inhalation of chloroform, into an alarming nervous condition. As in the preceding case, there followed enormous distension of the knee, and things then fell into their normal condition; on the fifteenth day the patient could raise the leg, which she could not formerly do without giving vent to cries.

What is most singular, the cure in these cases is obtained without compromising the mobility of the articulations.

In M. Costes' observation, we are informed that the abdominal viscera seemed to retain their natural mobility. M. Jobert, too, has found in all the cases of hydrarthosis on which he has operated by injection of irritating substances, that perfect freedom of motion was preserved. Facts, then, would seem to show, that the cure by injection is obtained by simply modifying the vitality of the serous membranes, and not, as has been asserted of the *tunica vaginalis*, in hydrocele, by the agglutination of the contiguous surfaces of the serous membrane by adhesions.

GLANDERS COMMUNICATED TO THE HUMAN SUBJECT. BY M. GRASLEPOIS.

On the 12th June 1851, M. Condamy, a veterinary surgeon in the commune of Vars (Charente), sixty-four years of age, of bilio-nervous temperament, in general a healthy man, for the last few years a sufferer from slight epiphora, dependent upon an atonic (?) state of the lachrymal canals of the right side, was called to attend a mare in the commune of Sonneville in the canton of Rouillac. After an attentive examination, the animal was pronounced to be glandered, and at the instant when by means of the fingers the nostrils were held open, to demonstrate to the proprietor and bystanders, the existence of a large and deep ulceration of the nasal mucous membrane, the animal sneezed and threw into the face of the surgeon a jet of greenish-yellow mucus, which he immediately sought to wipe away, by washing with simple water. After ordering the animal to be killed, M. Condamy returned home, not even suspecting that he bore the germ of a disease eminently contagious, which was shortly to conduct him to the tomb.

On the 17th, when the disease first manifested itself, the patient, who at the time was travelling, complained of acute smarting pain of the internal angle of the right eye, followed by tumefaction, redness, and œdema; these local symptoms persisted, and became aggravated up to the 20th, when M. Graslepois was called to his assistance. He found considerable swelling of the eyelids and of the ocular and palpebral conjunctiva, vivid redness of these parts, complete immobility of the iris, abolition of vision on the right side, intense headach, full, sharp, and strong pulse, without irregularity, a hot and dry skin, and excessive thirst. Thinking that he had to deal with an ordinary inflammatory attack, and uninformed of the antecedent circumstances, he bled the patient to one quarter of a kilogramme (half a pound), and ordered linseed poultices, &c. In the evening, there was general prostration, a more feeble and excited pulse, lividity of the eyelids, and of the conjunctiva, which projected from between them, a pustular knob (bouton) below the greater angle of the eye, œdema of the cheeks and of the lateral parts of the neck, incipient delirium, spasmodic agitation of the limbs, pain in the principal joints, with swelling and livid discolouration, abundant discharge of a yellowish-green fluid from the right nostril, and shortly afterwards, a similar discharge from the sore on the eyelids. A blister was applied to the nucha. Next day, another practitioner, called into consultation, agreed with M. Graslepois, in ascribing the symptoms to the bite of some insect; a tonic potion, a lotion, and blisters to the legs were ordered. The blistered surface on the nucha was livid, and the suppuration ill-conditioned; in the evening, a pustule had appeared on the right wrist and back of the right hand, surrounded with diffused livid redness, there was doughiness (empâtement) of the neighbouring cellular tissue, more marked delirium, tympanitic abdomen, abundant fetid perspirations,—in a word, all the symptoms of general infection. The author does not mention engorgement of the glands and lymphatic ganglia.

The symptoms having assumed so grave an aspect, were naturally ascribed to some other cause than the bite of an insect, and on consulting the patient's journal, with M. V., his son-in-law, a veterinary surgeon at Ruffa, M. Graslepois found that on the 12th June, as has been already stated, he might have been inoculated with glanders. The disease continued to make progress, the respiration became embarrassed, the deglutition impossible, and the patient died, furnishing an additional instance of the communication of glanders from animals to men, and causing deep distress to his family and numerous friends.—*Journal de Médecine et de Chirurgie Pratiques*, August 1851, p. 363.

CASE OF ACUTE GLANDERS, FOLLOWED BY RECOVERY. BY DR F. W. MACKENZIE.

William Wilcox, a horsekeeper, aged 58, was admitted into the Paddington Infirmary late in the evening of the 19th July. He was seen by Dr M. the following day, and appeared to be very dangerously ill. His face generally was swollen and suffused, but more especially in the submaxillary region. A large quantity of saliva was pouring from his mouth, his breath was extremely fetid

and sickening, his gums were swollen, and his teeth loose,—some indeed were merely hanging from the gums. The tongue was much enlarged, and could not be protruded beyond the margin of the teeth, and it was forced upwards against the roof of the mouth by the swelling of the salivary glands below; the throat was constricted, and consequently both breathing and swallowing were difficult. His nostrils were filled with a thick glutinous secretion of an offensive odour, which was with difficulty dislodged. The patient had been very restless all night, had been constantly throwing his arms about, was alternately chilly and hot, and at times sweated profusely; he had a quick pulse, and was in a state of extreme prostration. Looking to the profuse salivation, the condition of the salivary glands, the extreme fetor of the breath, and the state of the gums and teeth, Dr M.'s first impression was that he had been severely salivated with mercury. It appeared, however, that he had not taken a particle of this medicine for some time, and his general symptoms were, moreover, inconsistent with this view. After a great deal of trouble, occasioned by the difficulty he had in articulating, the following history was obtained:—

He had for some time past been in very destitute circumstances, in consequence of having been out of regular employment; his food had been scanty and bad, and he had been accustomed to sleep about in stables, or wherever he could get a shelter for the night. Under these circumstances, he accepted the charge of some glandered horses at Islington. He began attending to them on the 30th of June; and, at one time, had thirteen under his care, but some became so bad that they had to be sent to the knackers. In grooming them, the secretions from their mouths and nostrils were frequently blown upon his face, and the stench of the stable in which they were kept was very great. A week after he had been looking after them, he began to feel feverish and ill; his stomach in particular became disordered, and his appetite bad. He was now anxious to give up his employment, but his master pressed him to stay, and he remained for several days longer. He got progressively worse, the febrile symptoms increased, and his stomach became so much disordered that he could not retain any food; at times he broke out into profuse cold sweats, but he had no rheumatic pains, nor had he as yet any local affection of the face, nose, or salivary glands.

On Sunday morning, July 13th, while cleaning the horses, he felt completely overpowered by the stench, and became so ill, that he was obliged to leave the stable abruptly. He was now seized with severe headach, giddiness, and swelling of the face. Lachrymation and profuse salivation soon followed. He says that half a pint of fluid ran from his mouth in the course of an hour; his teeth became loose, and the gums painful and tumid. His tongue began to swell, his throat felt constricted, and his nostrils became filled with a thick, yellow, fetid, glutinous secretion. He had frequent rigors, succeeded by heats and cold sweats, and felt very low and weak. He was now obliged to give up his work, and was advised to take some gin, which, together with beer, he drank from time to time, but his stomach was so irritable that very little would stay upon it. He left Islington, and came to the Iron-gate Wharf at Paddington, in the hope of getting better, but still continued to get worse, and on the 18th July, applied at St Mary's Hospital for advice. He received some medicine as an out-patient, but having no home to go to, did not take it. The next day he went again, and had a blister applied below the jaw, but feeling worse towards evening, he applied for, and received, an order for admission into the Paddington Infirmary.

When admitted, he was in a very exhausted state; he breathed with difficulty, and could scarcely swallow some wine which was given him. The fetor from his mouth was most intolerable; he was restless, passed the night without any sleep, and was observed to throw his arms continually about. Dr Mackenzie adds:—I saw him at two P.M. on the following day. He was then worse than when admitted, his breathing was more difficult, and the swelling of his face had increased; this was most marked, as I have said, under the lower jaw, where it formed an enormous tumour. I have already mentioned some of his symptoms,

but it may not be amiss to recapitulate the following:—There was a profuse discharge of saliva running from his mouth; the tongue was tumid, and its surface covered with a white moist fur; the gums were soft and swollen, and his teeth loose. On passing my finger under the tongue, a large elastic tumour was felt on each side of the frænum linguæ, apparently consisting of the enlarged sublingual and submaxillary glands, and the distended ducts of the latter. I made a free incision in each of these, and a great quantity of clear glairy mucus poured out, which gave the patient much relief. I was unable to examine the throat, on account of the general swelling of the parts, but from the character of the breathing, and the difficulty of swallowing, it was evidently swollen, and the isthmus faucium contracted. I could not, however, see any appearance of erosion, ulceration, or pustules. The nostrils were filled with a viscid, yellow, opaque secretion, which, as I have said, was with difficulty dislodged, on account of its tenacity. The conjunctivæ were injected, and the eyes watered profusely. The patient complained of severe headach, which was greatest over the eyes and frontal sinuses, but at times it was felt over the greater part of the head. He was very sick, could not retain food upon his stomach, and was very restless and uneasy. He stated that he had not slept for a week. He was alternately chilly and hot, and at times broke out into cold sweats. His pulse was quick and weak, and he complained of great lassitude. He had no rheumatic pains, or any perceptible eruption on the skin or mucous membrane; the blister which had been applied had risen, and had a greenish, sloughy, unhealthy appearance. His bowels had twice acted during the morning, and he had made water freely.

Treatment.—He was directed to take immediately an emetic, consisting of half a drachm of ipecacuanha, and, as soon as vomiting had ceased, to take five grains of the sesquicarbonate of ammonia in water every hour, using as little water as possible; to use frequently a gargle of the chloride of lime, and to have wine and such nourishment as he could swallow given freely, and to take at night an opiate, consisting of twenty drops of the tincture.

July 21*st.*—On visiting him in the afternoon, his appearance was in every respect improved; his face was less swollen, as was also the tongue and salivary glands. He spoke, breathed, and swallowed better; his tongue was reduced in size; he had slept comfortably during the night, and taken nourishment, such as beef-tea, freely. His pulse had fallen to 80, the skin was cool, and he had experienced no rigors during the day. He said that the emetic occasioned copious vomiting of green unhealthy matter, and that as soon as this had taken place he felt relieved, and as if a load had been taken off him. He subsequently took the ammonia as directed. He was ordered to continue this, and to take five grains of blue pill at bed-time, as the tongue was still rather white.

July 22*d.*—Improving; the tongue was almost of its natural size, and there was very little swelling of the face or salivary glands. He spoke, breathed, and swallowed better; the breath was less fetid; the pulse was quiet; the skin cool; and the bowels open; he had slept well, and had a desire for solid food. To omit wine and beef-tea, and to have animal food, with a pint of porter daily. To continue the ammonia every four hours.

July 23*d.*—In every respect better. The swelling of the face and throat was less, as was also the discharge of saliva. He slept well, and enjoyed his food. To continue the ammonia every four hours.

July 24*th.*—Better. He had slept well and ate well; the bowels were open; the swelling of the face was now confined to the submaxillary region, and was very slight; he could breathe through his nose, and swallow without difficulty; the tongue was clean, moist, and of its natural size; the gums were but very little swollen; the teeth were firmer; there was no fever or shivering; skin cool; pulse 72. To continue the ammonia every four hours.

July 25*th.*—He was convalescing favourably.

July 26*th.*—Up to this date he had been daily gaining ground. The ammonia was discontinued on the 25th, and since then he has been taking merely a nour-

ishing diet, with a pint of porter daily. He now wished to be discharged, declaring that he never felt better in his life. He left the next day.—*London Journal of Medicine*, September 1851, p. 789.

ULCERATION OF THE CŒCUM. BY DR STORER.

Early in December last, Dr S. was called to visit Miss L., aged about twenty-five years. She complained of great pain in the right iliac fossa, and was suffering from considerable constitutional excitement:—Upon examining the part affected, he found it tumefied from about two inches anterior to the crest of the ilium, to some distance backward upon its spine; this tumefaction was exceedingly sensitive, and pressure upon a portion of it detected fluctuation. Dr S. ascertained that she had taken a long walk a week or two previously, when the ground was damp, with thin shoes—and, that at that time she felt chilled;—immediately after this exposure, she began to complain of uneasiness in the affected part, and her suffering had since constantly increased. The abscess was punctured, and egress given to several ounces of exceedingly offensive, ill-conditioned pus, together with a small portion of air: great relief was immediately experienced. The purulent matter continued to flow, unmixed with any foreign substance, until the third day after the opening was made. At this time, Dr Storer noticed three or four small black masses about the size of an ordinary pea, looking like little scybala, and the patient told him, that during the previous night she had been disturbed by the passage of air from the orifice. On the fourth day, liquid fecal matter flowed freely from the wound—this continued four or five days, requiring that dressings should be applied repeatedly during the day. From this period, nearly five weeks elapsed without the passage of any fecal matter, the discharge being entirely purulent, accompanied occasionally by air. At the termination of a month, the patient, having complained for a day or two of local heat and fulness, together with general febrile excitement, observed in the purulent discharge from the wound, "quite a number" of fig-seeds, which had been swallowed a fortnight previously. Succeeding the passage of these seeds, a long, somewhat compressed substance was observed in the aperture of the skin, which proved to be a portion of apple-peel, several folds of which had become rolled upon themselves, forming a fine, pointed body. Fecal matter followed these foreign substances, and continued to flow for four days, when the discharge gradually subsided.—*Records of the Boston Society for Medical Improvement*, in *American Journal of the Medical Sciences*, July 1851.

KELOIDES OCCURRING IN A CHILD. BY DR CABOT.

S. H., 4 years of age in May 1850, very healthy, and free from all cutaneous disease, until May last, when she was very severely affected by small-pox, never having been vaccinated—is the seventh child of a mother 28 years old, who was married the day before she was 14 years old, and whose oldest child was born one month before she was 15. She had, she says, scrofula at 5 or 6 years of age, and has a large scar from it on the side of her neck; she lost, some years ago, the upper joint of a finger, from "felon." One of her children had herpes zoster six years ago, when 3 years old. The child S. H. is rather of a leucophlegmatic complexion and cast of countenance, thickish upper lip, &c. He recovered from the small-pox, and was about the house early in July; about two weeks afterwards an appearance like a burn was observed by the family near the left angle of the mouth.

Dr C.'s attention was called to this and other tumours on the 3d of October, when he found the principal tumour, near the left angle of the mouth, about two inches long in its longer diameter, and about three-fourths of an inch broad, very irregular in its outline, and having processes shooting out in different directions, as if formed by the running together of several tumours having their axis in different directions; the surface smooth, pinkish, with some vessels ramifying on its surface; firm and hard to the touch; resembling very much in its external characteristics the cicatrix of a burn. There was another smaller tumour close

to the first, and, on the opposite side of his mouth were seven others, of which the longest was rather more than one-half by one-fourth of an inch. Besides these, there were five or six quite small ones, in differents parts of the face, and two on the right arm. He never complains of pain except when touched on the tumours; cries often when washed, though not a crying child; seemed to suffer very severely from a fall upon the part, and, at another time, when the principal tumour was roughly handled. October 12th, tincture of iodine, diluted with about its bulk of water, was applied to the tumours on the right side of the mouth, without much, if any, appearance of pain. October 13th, tincture of iodine, rather less diluted, was renewed, and to the tumour on the opposite side of the mouth, solution of gun cotton was applied, the effect of which was to deprive it entirely of colour, making it white, and causing the veins to disappear; some temporary pain seemed to be occasioned by it.

Keloide is pronounced, by the authorities of the present day upon cutaneous diseases, to be exceedingly rare; older writers even denied its existence (Bateman). Alibert first described it. Cazenave (*Maladies de la Peau*, 1838) states that it had not, up to the date of his work, been observed in *infancy*. Youth, bordering upon adult age, is its period of election; it is almost universally seated upon the anterior part of the chest, and, by preference, just between the mammæ; it is very rarely multiple. Biett refers to only two multiple cases; in one, eight patches existed; in the other, two; in the first case, on the neck and side of chest; in the second, on the front of chest. Cazenave alone, of the authors we have been able to consult, reports the appearance of the disease upon the *face* (*Dict. de Médecine*, tome xvii. art. Keloide), and this is exceedingly rare. The same writer declares its greater frequency in females and in those of lymphatic temperament.—*Records of the Boston Society for Medical Improvement*, in *American Journal of the Medical Sciences*, July 1851.

MATERIA MEDICA.

OXALATE OF POTASH IN PUERPERAL METRITIS, AND IN INFLAMMATION OF THE BOWELS. BY DR WELTI.

In the course of some remarks introductory to the subject of his communication, the author contrasts the results of his practice, extending over an experience of thirty-two years, before and since his having recourse to the above remedy. During the last twelve years he has had twenty-five cases of puerperal fever in his practice, of which only one terminated fatally; whilst before that time he commonly lost one case in three. During these twelve years he has treated forty-four cases of abdominal inflammations (Darmentzündungen), of which forty-one were quickly cured by the simple treatment to be mentioned below, whilst two, from complications, required a modified treatment, and one proved fatal, the disease being at its height before being treated, and the patient having twice taken a glass of brandy with pepper in it. The author also contrasts his present with his former feelings, on being called to cases of puerperal fever; formerly he approached them with sadness, from the feelings of the powerlessness of his art in the presence of such an enemy; now, he goes to them with satisfaction, because he can confidently hope that the disease will be subdued at most in three days.

Dr Welti was, in the year 1836, led to the employment of this remedy by finding in a periodical an extract from an English journal to this effect,—that oxalate of potash is a substance which in the normal condition acts disturbingly (*störend*) upon the intestinal canal. What, therefore, acts in this manner in the normal condition, must, under circumstances of morbidly exalted activity, act as a sedative (*herabstimmend*)!! He made a note of this, and being an opponent of leeching, and of all greasy applications, and an admirer of all new and simple remedies, conceived that a trial of it might lead to a useful form of treatment, which the established practice in such cases had heretofore excluded. He soon

had an opportunity of trying it, in the case of a man of 34, affected with abdominal inflammation. He was called early, because all the similar cases in the same village had proved fatal; but the disease had, though only of a night's duration, acquired great intensity. He ordered an emulsion with nitre, with intermediate doses of calomel. By the evening the patient was worse, and was attacked with vomiting and other bad symptoms, The author now determined to try the oxalate of potash, and ordered ten grains of extract of hyoscyamus and ten grains of oxalate of potash in a five ounce mixture,—a table-spoonful to be taken every hour. He visited his patient four hours afterwards, with some anxiety as to the result, and found him much better, the sick man's account of the thing being, that the first dose had stopped the vomiting, the second had removed the abdominal pain, and the third had thrown him into the comfortable perspiration in which the doctor found him. The patient continued the remedy next day, and soon got well.

Soon after a case of puerperal metritis occurred to him. He prescribed the same mixture of oxalate of potash; found the patient at the end of four hours in a perspiration; repeated the mixture as a measure of precaution, and the patient was cured. These two cases occurred in 1837.

Dr Welti classifies puerperal fever cases under three heads, according to its form. The first is a mild, the second an obstinate form, requiring larger doses, or the combination of some other treatment. The third is metritis already existing before confinement, and becoming decided as soon as delivery takes place. Metritis occurs mostly three days after delivery, whether this has been natural or instrumental, and either comes on stealthily or in a decided and acute form. Yet sometimes it makes its first appearance fourteen days, or three or four weeks, after delivery.

The first of these forms [we suppose this means the cases where the uterus is inflamed before delivery] is the most important, and admits of the most speedy relief. The author's practice supplied him with three cases. The first, in 1830, under the old treatment, died. The other two, in 1837, were cured in four days. All were easy cases of labour, requiring assistance, the first turning, the two others the forceps. In these cases the uterus, already in a morbid state, had lost its contractile power, and felt very hot to the hand, when introduced into it to remove the placenta, on account of hemorrhage. He did not employ the oxalate of potash alone, but gave it in large doses along with some nitre, and calomel, and opium.

The second milder form of puerperal fever is that which occurs from one to three days after delivery, and is more frequent after natural than artificial labours. The author gives one case of this—fatal—where the patient remained *in statu quo* under the treatment with oxalate of potash; but the friends being alarmed, another practitioner was called in consultation, who had recourse to bleeding, leeching, calomel, &c. Dr Welti predicted that the patient would die, and the patient died accordingly.

The third, and often from its complications most troublesome, form of metritis, is that which occurs three or four weeks after delivery. Dr W. narrates two cases of this kind, which occurred during intensely hot weather in June 1849. In the first case all went on well till the 15th day, when hemorrhage came on suddenly, for which acids and ergot of rye were prescribed. The bleeding was arrested, and the patient kept well till the twenty-second day, when it recurred, —the same treatment was ordered, but without good results. On this same day the doctor was called to see the patient immediately. He found her sick, the hemorrhage abated, but the uterus as large as in the fourth month of pregnancy, and tender, the pulse quick and full. The breast and arms covered with purple spots; the right leg had for four days been swollen, and painful. The thirst was urgent, and the abdomen distended and tympanic on percussion. The patient stated, that since the hemorrhage had been checked eight days previously, the pains in the pelvic region had been constantly occurring from time to time, and the uterus gradually enlarging in size. Nothing was ordered at the moment.

The patient was seen again in two hours, and the disease had by this time made astounding progress: the uterus was as large as in the fifth month, the belly distended to a marked degree, and the thirst and quickness of pulse increased. The swollen leg was treated by camomile fomentations; and Dr W., regarding the metritis as the radical ailment, ordered gr. x. of extract of hyoscyamus, ℈i. of oxalate of potash, ℥ss. of nitre, ʒi. of syrup, and ʒiv. *aqua cerasorum*,—a table-spoonful for a dose; the two first doses to be given at half-hourly, and the rest at hourly, intervals. The first dose was refreshing to her; from the second she obtained more relief; and after the third the pains ceased, and she steadily improved. She fell asleep at midnight, and was in an improving condition in the morning. The medicine was continued during next day, at longer intervals; and in ten days all the symptoms had disappeared under this simple treatment, without bleeding, or leeching, or other means.

Dr W. narrates another similar case occurring during the third week, where a like result followed a similar treatment. He had also cured two cases of metritis, occurring twelve hours after delivery by turning, with no other remedy than the above mixture.

With similar good results he had administered the remedy in forty-four cases of abdominal inflammation, besides in diarrhœa in young children of most tender age, where acidity showed itself.—*Zeitschrift für Erfahrungsheilkunst*, 3 band, 3 heft., 1850.

[The statements of the author appear to be tinctured with enthusiasm for his new remedy, and the physiological grounds on which he was led to its use are not in the least degree comprehensible to us. But many a good remedy has been propounded upon a false therapeutical theory, and we have given Dr W.'s statements as we find them. An efficacious cure for puerperal inflammation would be a valuable acquisition to our resources. It is, we presume, the neutral oxalate which he employs.]

EMPYREUMATIC COAL OIL USED IN ECZEMA. BY M. LAFONT-GOUZI.

In the Transactions of the Medical Society of Toulouse, M. Lafont-Gouzi highly extols the virtues of the oil obtained by the distillation of pit-coal in the manufacture of gas, as a remedy in certain eczematous affections, and in itch. He has used the oil in the treatment of eczema impetiginodes, of itch, of prurigo, of psoriasis, of purulent ophthalmia, of keratitis, and of otorrhœa dependent on cutaneous eruption (*otorrhées de nature dartreuse*). M. Lafont mixes eight parts of empyreumatic oil with thirty parts of axunge, and spreads the ointment over the parts affected with eczema; he asserts that it is the most active of siccatives. In cases of prurigo and of psoriasis, he replaces the axunge with an oil of henbane containing opium (*huile de jusquiaume opiaciée*). If the disease proves obstinate, he uses the undiluted empyreumatic oil. The following case is given in illustration of the efficacy of the treatment.

A woman of forty-five years of age fell under his care, on account of an extensive suppurating eczema of the whole fore-arm. She had used, for a year, various internal and external remedies, without obtaining the slightest benefit. The mineral waters of Luchon had been tried in vain. The surfaces were very red and encrusted; the intolerable sensation of heat was only appeased by cataplasms of the fecula of rice, &c. Three applications of the empyreumatic oil to the whole diseased surface, made at intervals of five days, restored the skin to its normal condition.

Finally, M. Lafont states that, for the last three years, he has used the empyreumatised ointment with complete success in treating itch, a disease in which so many other remedies have been justly recommended as efficacious.—*Journal de Médecine et de Chirurgie Pratiques*, August 1851, p. 347.

USE OF NEUTRAL SULPHITES IN THE TREATMENT OF SARCINA VENTRICULI. BY DR JENNER.

(The following interesting case and observations are extracted from a clinical lecture delivered by Professor Jenner at University College Hospital, and reported in the *Med. Times* of Aug. 23, 1851):—

James Martin, now aged 66, the son of healthy parents, who died at the ages of 82 and 80 respectively, enlisted into the Artillery when 18 years of age, served in the Peninsula, was at Waterloo, and subsequently went with his regiment to North America, where he remained six years. He was discharged in 1824. After leaving the army, he worked in the Thames Tunnel, and was there when the water broke in. He was supposed to be dead when first removed, and for a year he attended as an out-patient at Guy's Hospital. In 1829, he entered the police force, in which he continued for seven years. After leaving it, he entered the Queen of Spain's service, and returned with the Legion in 1837. Though formerly of intemperate habits, he avers that for many years he has been very sober. With the exception of an attack of ague while in America, his health up to the time of the accident in the Thames Tunnel was excellent. From that event he dates the commencement of his present ailment. While in the police he suffered from intense pain at the epigastrium, and used often to retire into back streets, and there leaned with his abdomen firmly fixed against an iron post. At first, the vomiting occurred once or twice a week; afterwards, every day. It was at this time, he says, that he first vomited blood. From the time of the accident he has been subject to constipation, and long tried all sorts of quack medicines; and, to relieve the pain in the abdomen, took a teaspoonful of carbonate of soda twice a-day. While in Spain he endured many hardships, but was not so much a martyr to his old complaint; and, for the first six months after his return, he enjoyed comparative health.

In the early part of 1838 he entered University College Hospital, and, by his own account, has been unable to work since. He has been, at different times, in and out-patient at this and other hospitals, and, on one occasion, was an inmate of the parish infirmary. He was an out-patient when I was appointed to this Hospital, but too ill to attend himself. His wife came for his medicines. It was in consequence of the description she gave of the vomited matters (viz., that they frothed over the vessel in which they were received like yeast) that I examined them microscopically. I then found the bodies described by Mr Goodsir, in 1842, as sarcina ventriculi. When I first visited Martin, a year ago, this was his condition,—he was in bed, and had not been up for more than a few minutes for several days. He said that, in consequence of pain in his abdomen, he slept but little. He was free from headach, and his complexion was clear, his skin was cool, and there was no trace of œdema of the extremities. Neither the liver nor spleen was enlarged. There was decided tenderness at the epigastrium, and so far as this permitted manipulation, no tumour could be detected. He stated (and his countenance bore witness to the truth of his assertion) that he then was, and had been, suffering acutely about the umbilicus, above, below, and on either side of that spot; that the pain was "smarting and burning like a blister;" that if he sat up, he felt as though he had "a cord around his bowels, tying them back" to his spine; that he was in the habit of sitting in bed, with his knees drawn up towards his abdomen, his head and shoulders bent forwards, his arms resting on his knees; and that, while in this position, he would often get his wife to place some hard body—at one time a bundle of clothes, and at another, "the bellows"—between his knees and abdomen, the pressure of which seemed to give some relief during the paroxysms of pain. The pulse was only 72, the respirations 18 in the minute; his appetite was indifferent. His bowels had long been most obstinate, never acting without medicine, and that of the most powerful kind, croton oil, &c. He remarked, that whenever the vomiting diminished the pain increased, *so that he desired the vomiting to continue.* While an out-patient I tried for his relief nitrate of silver, hydrocyanic acid, creosote,

morphia, henbane, large doses of carbonate of soda, &c. I thought the morphia lessened the frequency of the vomiting, but, although it produced drowsiness, it seemed to increase the pain. With difficulty I persuaded him to come into the hospital, and, so satisfied was his wife that death would speedily terminate his sufferings, that, after he had been in a few days, she earnestly entreated me to discharge him lest he should die here.

He was admitted into the hospital as an in-patient March the 15th, 1851, and on the 16th the following notes of the case were taken:—Mind and special senses normal; *arcus senilis* strongly marked; can walk with facility, but feels weak; expression worn, is extremely thin, skin cool, mucous membranes pale, no sallowness of complexion, no anasarca, legs drawn up towards his abdomen and his arms around his knees, shoulders bent forwards, his head resting on the elevated inferior extremities; tongue large, flabby, indented at the edges, and thinly furred; no dysphagia, appetite generally good; had for his dinner yesterday two potatoes and some broth; dined between twelve and one o'clock; at five o'clock p.m. vomited, ejecting the whole of the matters now in the basin in about five minutes; the quantity of ejected matter reserved is two quarts.

Characters of the Vomited Matter.—Floating on the surface is a layer, from a quarter to half an inch thick, of a brownish colour, closely resembling yeast in general characters, entangling in its substance quantities of air-bubbles. It contains striated muscular fibres: sarcinæ ventriculi in great numbers, of a dark yellowish colour; torulæ very numerous, undistinguishable from the yeast-plant in size and shape; fat-globules and starch.

After eating his dinner felt comfortable and satisfied for two hours, then experienced considerable thirst; about four o'clock P.M. took a cupful of cold fluid. He thinks that cold fluids excite vomiting, hot prevent it; that cold fluids cause pain, hot fluids relieve it; from the time of drinking the fluid he felt severe burning twisting pain over nearly the whole abdomen; the burning pain was most severe about the epigastric region, the twisting pain about the umbilicus; to use his own expression, "he felt in a burning flame;" much flatus was discharged before vomiting, chiefly by belching, each eructation of flatus was attended with severe sense of burning up the centre of the sternum, called by the patient a "flashing pain;" after vomiting, the pain and burning were considerably relieved; still the pain continued during the whole night, increasing in severity at intervals. Five grains of extract of colocynth, with two drops of croton oil, were taken last night; these operated about eight o'clock A.M., producing three stools; the action of the bowels relieved a sensation of tightness. He had this morning for his breakfast, about eight o'clock A.M., a cupful of tea and a little bread and butter. At twelve o'clock complained of a nipping pain between the umbilicus and pubes, a burning pain from the epigastric region upwards over the right mammary and sternal region; no vomiting to-day; complains of tightness and fulness of abdomen. Abdomen convex; convexity less marked at epigastric and hypochondriac regions than below the umbilicus; interval between recti-muscles distinct; tenderness in epigastric and umbilical regions; slight tenderness also in hypogastric; more in hypochondriac and umbilical regions. Some sense of resistance on pressure over recti-muscles; resonance generally normal; no tumour to be detected in any part of abdomen; there is, however, greater fulness in the vicinity of the pylorus than elsewhere (from tenderness and distension the manipulation was less deep than subsequently); passes his urine freely; pulse 60, regular; lungs[1] and heart generally healthy, only the second sound at the base is murmurish. The stool contains a large amount of yellow amorphous matter, sarcinæ, and triple phosphates; its re-action alkaline.

The daily reports from this time tell of his continued suffering, and of the frequency with which he was observed in what is termed, in some of the reports, his old position.

---

[1] The case has been much abridged as regards the organs not bearing directly on the disease.

On April the 22d the following report was made:—

At half-past eleven P.M. the twisting and burning pain came on, and he brought up about eighty ounces of fluid at two efforts; then drank about half a pint of gruel, after which he continued eructating flatus and vomiting a little fluid till three A.M.; the fluid last ejected was about forty ounces in quantity. He describes the pain which preceded the vomiting to have been of a bursting kind. He says he felt "as if being torn to pieces." He complains that during the night he suffered from headach and vertigo. The abdomen is now very tender; he complains of a bitter taste in his mouth, which he says is always full of water. Bowels acted last night; pulse 76. The vomited matter was strongly acid, and resembled in all particulars that above described; and the stools were, as before, alkaline, and contained sarcinæ.

On May the 29th the report states,—Much freer from pain; all sense of "flashing burning" gone; lies ordinarily with legs extended, as if free from pain. His present state offers a striking contrast to his condition on admission. Bowels acted to-day. No stool the preceding three days.

As the hardness of the abdomen disappeared and the distension diminished, it was pretty clearly made out that the stomach was dilated, the stomach-note being audible to the umbilicus; and an ill-defined solid body was imperfectly felt about two inches to the right of the middle line, midway between the umbilicus and cartilages of the ribs.

His present state you have all witnessed; he suffers little pain; is freed from the eructation of flatus; his position is unconstrained; his abdomen is much less full than on admission; he is gaining flesh; the worn expression is comparatively trifling, and he sleeps well. The alteration in his general carriage and position strikes his wife and friends most forcibly. It is, he affirms, many years since he enjoyed the same state of comfort. I must briefly advert to the condition of his urine. It is acid when first passed, but becomes alkaline rather more quickly than healthy urine should. One of my excellent clinical clerks, Mr Simpson, has been kind enough to examine it daily for some time, and he notices no difference in the degree of acidity, as manifested by litmus paper, whether Martin is vomiting or not. An analysis of the matter vomited before the man's admission to the hospital, was kindly made by Professor Graham. It contained a large excess of free hydrochloric acid, a little acetic acid, alcohol, and sugar; the gas disengaged was carbonic acid. It was identical, in all essential particulars, Mr Graham says, with the matter vomited by a man suffering from sarcina ventriculi, under the care of Dr Bence Jones, in St George's Hospital.

Now, did the primary disease in the case of Martin, consist in the presence of the sarcina in the stomach, or was the sarcina (as Schlossberger from his cases concludes it invariably is) merely an epiphenomenon?

* * * * * *

I have read to you some notes, which illustrate the improvement that has taken place in our patient's condition since his admission into the hospital. Let us now see by what means that improvement has been effected.

It appears from the notes, that, from his first admission till the 5th of April, he vomited almost daily an enormous amount of thick fluid in a state of fermentation, loaded with sarcinæ and torulæ. To give you an idea of the quantity, I may mention, that on March 10th, he vomited two quarts; on the 17th, one quart; on the 19th, a pint and a half; on the 20th, one quart; on the 22d, five pints; on the 24th, two pints and a half; the drugs given during the same three weeks were hydrocyanic acid, acetate of morphia, and sulphate of zinc. You will remember he had proviously taken creosote, nitrate of silver, &c.

At this time, guided by the knowledge that sarcina is a vegetable, and having had my attention called by my friend Mr Marshall to the fact, that Professor Graham had suggested the employment of sulphurous acid in cholera, when that disease was supposed to depend on the presence of a fungus in the intestinal canal, as the agent the most destructive to vegetable life, and, at the same time, one

that might be involved in the human stomach with impunity, I administered the sulphite of potash.

The power exercised by the remedy has been most remarkable and most unequivocal.

Before taking it, he was vomiting daily from 40 to 100 ounces of the fluid, loaded with sarcinæ and torulæ; thus, on the 1st of April, he vomited 100 ounces; on the evening of the 2d, between 70 and 80 ounces, although he had the same morning, from the action of sulphate of zinc, vomited 50 ounces. No vomiting on the 3d. On the 4th, in consequence of the extreme pain and sense of distension, sulphate of zinc was again administered.

On the 5th, he took ℥ss. of the sulphite of potash early in the morning: in the evening he vomited. On the 6th the dose was increased to 1 drachm. On the 7th, he vomited 12 ounces only of acid fluid, on the surface of which there was but a small amount of scum; it contained perfect sarcinæ, but no torulæ.

From the 8th, he took 3 drachms of the sulphite daily, each dose being given in ℥iss. of water. At 6 A.M., on the 9th, he ejected from the stomach 4 ounces; the sarcinæ were now decidedly less numerous, and there were no torulæ. In the evening of the 6th, he again vomited, but the vomited matters were now free from sarcinæ and torulæ, and there was appearance of fermentation. Between the 9th and the 18th he vomited three times, but on neither occasion did the vomited matter contain sarcinæ or torulæ.

On the evening of the 18th the sulphite of potash was omitted; and on the 19th, he vomited a fluid in a state of fermentation, containing torulæ, but no sarcinæ. On the 20th, he vomited 9 ounces of a similar fluid; these specimens were not examined by myself, but by a most trustworthy observer, Mr Morris, the physician's assistant. On the 21st, there was a thick scum on the surface of the vomited matters, which contained sarcinæ and torulæ in abundance.

The sulphite of potash, in drachm doses, was again administered; no further vomiting occurred till the 27th, when, without any apparent cause, it again commenced. At the dispensary we learned that a fresh stock of the drug had come in on the 20th. On submitting a specimen to Mr Graham, he found that it contained no trace of sulphurous acid. At his suggestion, I prescribed from this time the sulphite of soda, a more stable salt, and one less liable to be decomposed either in the preparation or by keeping.

On the 29th of April, half a drachm of it was given at bed time, and on the 31st, half a drachm three times a-day. On the 1st of May, our patient vomited 40 ounces of a fluid covered with a thick scum; the vomited matters were not examined microscopically.

On the 2nd, the dose was increased to a drachm. There was no vomiting till the 5th; the vomited matters were in quantity about 50 ounces, had no scum floating on their surface, and were entirely free from sarcinæ and torulæ.

On account of the burning pain which he occasionally complained of, I now ordered him a mixture containing sodæ carb., two scruples in each dose. On the 13th, 15th, and 16th, he again vomited sarcinæ in quantity; and as I suspected that soda prevented the evolution of sulphurous acid, I omitted the carbonate. From the 16th of May to the 19th of June, the vomited matter contained no trace of sarcinæ or torulæ, and had lost its yeasty odour and appearance. *During these thirty-three days he ejected about* 230 *ounces of acid fluid; during the first eight days of his residence here he ejected* 380 *ounces.*

On the 19th of June, the vomited matters contained a considerable amount of sarcinæ. On inquiry at the dispensary, I found that the stock of sulphite was nearly exhausted; and on testing it by the addition of a stronger acid, it gave off but little odour of sulphurous acid.

I increased the dose to ℥iv., and on the next day to ℥iss.

From the 19th of June to the 1st of July he vomited once only, and the ejected matter contained no sarcinæ. * * * * *

Perhaps it may be supposed that diet has assisted in removing the sarcinæ.

Not at all; for Martin has had a pretty liberal supply of arrow-root, beef-tea, mutton, milk, bread, and potatoes, with two ounces of rum daily. Sometimes the meat has been replaced by fish; sometimes the diet has been chiefly animal, sometimes vegetable; but this has exerted, apparently, no influence on the formation of the sarcinæ. It is true the sarcinæ return directly the sulphite of soda is omitted; but then, even though it were necessary for this man to continue its use for life, it would be no great trouble. Sulphite of soda is not more disagreeable to the palate than carbonate of soda, and is less injurious, and many take the latter daily. But I hope, by steadily pursuing its use, by the employment of unfermented bread, as to-day suggested by Mr Graham, and by some other alterations in his diet and drugs, that ultimately he may be able to omit the sulphite entirely.

I cannot conclude without observing, that considerable benefit may be anticipated from the employment of sulphurous acid in all diseases attended with the development of parasitic plants. I would mention porrigo especially. That it would prevent the growth of the epiphyte there is little doubt, and, should it do so and yet not cure the disease, the question of the relation between the two would be settled.

In some forms of dyspepsia, attended with the discharge of large quantities of flatus, I think I have seen considerable benefit from its use.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

DEDUCTIONS REGARDING PUERPERAL ECLAMPSIA, AND ITS RELATIONS TO ALBUMINURIA IN LYING-IN-WOMEN. BY M. DEPAUL.

1. Convulsions, of an epileptic character, are rarely seen in the first four months of pregnancy. M. Depaul, however, had seen a case at the end of three months, in a person who previously had not been subject to epilepsy, or any other nervous affection.

2. It is erroneous to state, that the prognosis of eclampsia is more favourable in proportion as gestation is less advanced, or according as labour, if it have commenced, is distant from its termination.

3. This opinion is founded upon an inference not drawn from facts, viz., that the paramount indication of treatment is depletion of the uterus.

4. The dangers which threaten the fœtus consist in the modifications which the maternal blood undergoes, and on the disturbance of the uterine circulation; the death of the fœtus not unfrequently occurs in the course of a convulsive paroxysm. The fœtus resists the causes of destruction more certainly in the earlier than in later months, when it is nearer to its perfect state.

5. The best treatment of these cases is full general depletion.

6. Neither paleness of countenance, smallness of the pulse, nor the presence of albumen in the urine, are contra-indications of depletion.

7. Albuminuria is more frequently met with in pregnancy than in any other physiological state.

8. Without denying the possibility of nephritis in a pregnant woman, the presence of albumen in the urine must generally be regarded merely as a functional derangement. This is confirmed by the results of autopsies.

9. This albuminuria cannot be regarded as the cause of the convulsion, since the latter has often preceded the former.—*Medical Gazette.*

REMOVAL OF A LARGE FIBROUS TUMOUR OF THE UTERUS, FORMING AN IMPEDIMENT TO THE PROGRESS OF LABOUR. BY M. DANYAN.

The woman, thirty-one years of age, had already born naturally three children. She was nearly eight months gone with her fourth child. For three weeks she had been losing a little blood per vaginam, and the waters had been discharged when M. Danyan was called to see her.

He found a large tumour nearly immoveable, and filling up almost the whole cavity of the pelvis. In front of the tumour, the os uteri could be felt dilated in form of a crescent with its concavity turned backwards.

M. Danyan, in consultation with M. P. Dubois, decided to enucleate the tumour. Manipulating carefully, he made a long incision in its anterior superior surface. Some hemorrhage followed. By the aid of the fingers, the tumour was partially enucleated and dragged down by the forceps of Museux. When the tumour projected from the labia, its bulk was so great as to prevent further manipulation with the fingers. Its lower half was therefore cut off, and the further enucleation then completed. The hand was then introduced, and the head, hand, and foot found presenting. By traction on the foot and some slight incisions in the os internum uteri, which was contracted, M. Danyan completed the delivery. The woman did well.—*Rev. Med. Chirurg.*, May 1851.

EXTRA-UTERINE PREGNANCY OF TWENTY YEARS' DURATION.

A woman, forty years of age, was suddenly seized with symptoms of internal strangulation, under which she sank. Dr Christian, under whose care she was, discovered in the abdomen a large tumour, situate in the right iliac fossa and lumbar region. This tumour had first appeared some months after her marriage, twenty years before, at which time labour had come on at the sixth or seventh month, without any result; the movements of the fœtus had suddenly ceased, and the labour itself was arrested. After this period she had born eight children. The autopsy showed a tumour of the size of a large ostrich's egg, enveloped in a cyst with ossified walls, except at its superior part, and enclosing a fœtus of six or seven months, as well preserved as if it had been artificially enclosed after the labour,—still having its umbilical cord fixed to the interior wall of the envelope which represented the placenta, and which was completely ossified.—*Philad. Med. Examiner*, in *L'Union Médicale*, 15th July 1851.

CASES OF VESICO-VAGINAL FISTULA TREATED BY OPERATION. BY DR. GEO. HAYWARD.

Dr Hayward's first operation for vesico-vaginal fistula was performed in May, 1839, and an account of it was published in the No. of this Journal for August of the same year. He has since operated frequently, and has met with such satisfactory results as to be induced to give his experience on the subject. He has operated twenty-six times on nine patients—one being operated on six times, another five, two twice, and five once. In three cases the operation was entirely successful; in five the patient obtained great relief, so that the urine could be retained for a number of hours without any escape through the fistulous opening; and in the remaining two, no benefit was derived from it. This is a degree of success highly creditable to the skill of this distinguished surgeon.

Dr H. gives the following details of his mode of operating, and of managing the patient afterwards:—

"Before the discovery of the anæsthetic powers of ether, I found that the most difficult and painful part of the operation consisted in bringing the bladder down to the os externum. It is now done with comparative ease, and without causing the slightest suffering to the patient. I have administered the ether in the three last operations of this kind, and have been able to bring the bladder down, pare the edges of the fistula, introduce the ligatures and the catheter, and restore the bladder to its place, in twenty minutes; when in all the cases before, in which I did not use it, the same process required an hour, and during the most of that time the patient was suffering severely. Besides, the fistula is sometimes in such a situation, as when it is near the fundus of the bladder, that without this agent, or some similar one, it would be impossible to bring it in view.

"The patient being thoroughly etherized, the bladder can be brought down by introducing a large-sized bougie (one made of whalebone, highly polished, is to be preferred) into the urethra, to the very fundus of the bladder, and carrying the other end up to the pubis. In this way the fistula is readily brought into

sight. Its edges can be pared with the scissors or a knife, though usually both these instruments are required; and this part of the operation is much facilitated by holding the edges by means of a double hook. In all the cases that I have examined, these edges are thick, hard, and usually of a white colour. It is not difficult, therefore, to dissect up the outer covering from the mucous coat of the bladder to the distance of two or three lines. The needles are then to be passed through the outer covering only, and as many stitches must be introduced as may be found necessary to bring the edges of the fistula into close contact.

"Since my first operation, I have used a short needle, with the eye near the point, made to fit on to a long handle. The instrument, when the two parts are together, looks not much unlike a tenaculum, though not so much curved, and considerably broader near the point.

"As soon as the needle is passed through one side of the fistula, it is immediately seized by a forceps, the handle is withdrawn, and the needle is then carried through. It is to be then again fitted to the handle, and carried through to the other side in the same way. As many stitches as may be thought necessary to bring the parts into close contact can in this way be made with great ease. One thread of each stitch is to be cut off; it is convenient to leave the other, as it enables the operator and patient to know when the ligatures have separated from the bladder.

"A large-sized female catheter is then to be introduced into the bladder, and secured there by means of a T bandage. The patient should be laid on her side, with the upper part of the body somewhat raised, so as to facilitate the flow of water through the catheter. This should be removed at least once in every twenty-four hours, as it is very likely to be obstructed by mucus, coagula of blood, and occasionally calculous concretions. In three days I think it safe to remove it altogether; but then it should be introduced at least once every three hours, for ten or twelve days more, so as to prevent any accumulation of urine in the bladder, and consequent strain on that organ.

"The diet should consist entirely of liquid mucilaginous food; such as an infusion of slippery elm, gum-Arabic and water, flax-seed tea, arrow-root, and milk and water. This diet, in my opinion, should be continued till the ligatures come away.

"The bowels should be opened by some mild laxative a few hours before the operation; but it is desirable that they should not be moved again till some days after.

"I think it best for the patient to use the catheter once or twice a-day for several weeks, and at any rate during that time to avoid making any strong efforts to expel the urine by the contraction of the bladder.

"It may be proper to add, that I have never had any troublesome hemorrhage from the operation, nor any alarming symptoms after it. In some cases the pain has been severe for two or three days, and once or twice it has run down the limb, apparently in the course of the sciatic nerve. When performed in the way that I have recommended, I believe it to be attended with very little if any danger, as the bladder is not subjected to any considerable degree of violence, nor any part injured to a great extent."—*Boston Medical and Surgical Journal*, and *American Journal of Medical Sciences*, July 1851.

A NEW TREATMENT OF PROLAPSUS UTERI.

M. Desgranges, surgeon to the Lyons Hospital, has laid before the Academy of Medicine of Paris a new mode of remedying the uterine displacement. It consists in pinching up the mucous membrane of the vagina, after reduction, with ittle forceps, or "serres fines" of Vidal. The forceps produce ulceration, and fall off from the fifth to the tenth day; and M. Desgranges repeats this operation eight or ten times, taking care always to leave as many of the forceps as possible. At the commencement of the treatment, he introduces as many as nine; later, from six to four; and at last, towards the end, only one of the little instruments. The treatment lasts in general two months and a half, or three months.

It will be seen that this method rests on the fact that the narrowing of the passage may prevent any further descent of the organ, the loss of substance and cicatrization being instrumental in the result. M. Desgranges adduces three cases in support of his views, and the operation might at first sight be looked upon as holding out a fair chance of success; but it should be borne in mind, that by the coarctation of the canal, none of the weight of the displaced body is diminished; and that constant pressure from above may certainly conduce to the elongation of the cicatrices.

For assisting the memory of our readers, we shall just transcribe the various analogous operations which have been proposed, as given in a very good article of the *Bulletin de Thérapeutique*:—

"1. Excision of a few folds of the vaginal orifice, as practised by Dieffenbach, in imitation of Dupuytren's mode of excising some folds of the margins of the anus in prolapsus of the rectum.

"2. Excision of a broad longitudinal flap of the mucous membrane of the vagina, either elliptical (Dr Marshall Hall) or square (Dr Ireland), and the immediate apposition of the lips of the wound by suture.

"3. Cauterization of the mucous membrane, so as to harden the tissues by suppuration, and obtain union between the walls of the vagina and the surrounding organs (Laugier).

"4. Excision of a semicircle of the vaginal orifice, and immediate apposition by suture, in acting either on the anterior or posterior circumference.

"5. Episioraphy, practised by Fricke, of Hamburg. This consists in paring the inner surfaces of the labia majora, and bringing them together by suture.

"6. Complete obliteration of the vagina, proposed by M. Romain Gérardin. This operation has never been practised upon the living subject, and could hardly be thought of before the cessation of the catamenia."—*Lancet*, 9th Aug. 1851.

NEW OBJECTION TO CHLOROFORM.

M. Chailly Honoré made the following communication to the Medico-Chirurgical Society of Paris, at its second quarterly meeting of this year:—

A woman who had been delivered for the first time by craniotomy, by M. Paul Dubois, and whom I had delivered by the forceps, in her second accouchement, of a living daughter, fell pregnant a third time. Notwithstanding our urgent advice, she objected to art being had recourse to until the full period of this her third pregnancy had come round. The pelvis of this woman not being more than eight centimetres in diameter, it was not possible that this delivery could be effected so easily as her second; for then labour came on somewhat before the full time. The induction of premature labour could alone offer the best chance of safety, both to the mother and to the child. Consequently, we gave it as our advice that this should be resorted to at the proper time. But this woman, having been under the influence of chloroform during the first and second operations, and not having experienced the pains of labour, had not calculated upon the sufferings likely to accrue in her state. She therefore preferred waiting her full time, confident of being delivered without pain, at the sacrifice of her child, We could therefore, under these circumstances, only yield to the wishes of the patient. I applied the forceps, in order to assure myself that it was not possible, as in the second labour, to extract the infant alive. This time we did not make use of chloroform, in order that the maternal sentiment might be aroused, she having refused the resources of art at a time when it would have conduced to the well-being of both mother and child. A short use of the forceps soon convinced us of the inutility of their application. The lesson was sufficient; we now gave her chloroform, and delivered her, as she had been in the first instance, by craniotomy. This circumstance revealed to me an inconvenience of chloroform of which I had never dreamt. But I hope that in future this woman will understand her duty better, and not, for her personal interest only, compel us to sacrifice a third infant.—*L'Union Médicale.*

OVARIAN CYST—DEATH FROM AN UNUSUAL CAUSE. BY DR VAN BUREN.

The patient, a widow, aged 29, first discovered a hard moveable tumour in the right iliac fossa five years since. At the time of her admission into the hospital (May 30th, 1850), the existence of an ovarian tumour was clearly ascertained. She was very subject to colicky pains in the bowels. The tumour was very moveable, and was thought a favourable instance for ovariotomy.

August 28th.—Was seized with symptoms of peritonitis, and was treated with calomel and opium and a blister.

On the 1st of September all evidence of peritonitis had disappeared, but the symptoms of a more alarming character supervened, such as pinched and sunken face and thready pulse. She sunk exhausted.

Post-mortem examination twelve hours after death.—The abdomen was opened by crucial incisions. The flaps thus made were found to be adherent, by their peritoneal surfaces, to the tumour, which was very dark in colour, almost black, and at points greenish. The peritoneum in contact with the tumour was stained with the same colour by imbibition. The omentum, small intestines, and colon, wherever in contact with the tumour, were adherent to it, and stained of the same colour. These adhesions were torn through without difficulty, being evidently of recent deposition. There was no fluid in the cavity of the peritoneum, and no evidences of peritonitis except in contact with the tumour. On separating the tumour from its adhesions, which had thus fixed it in its position, it was found to be connected with the uterus by the right broad ligament, and, in fact, to consist of the right ovary. In the broad ligament, which was somewhat elongated, and the sole pedicle to the tumour, was a short very tight twist, which was found to have been produced by the revolution of the tumour upon its axis one and a half times. On the distal side of the twist a mass of very much distended (ovarian) veins was visible. Internally the tumour was deeply stained with blood, with which it was yet intensely congested; its structure was evidently fibrous, with irregular cavities throughout its substance (fibrous overgrowth of the ovary).

Dr Van Buren attributes the peritonitis and enteritis to inflammation originally set up in the pedicle of the ovarian tumour, which by some accident had become twisted.—*New York Journal of Medicine*, March 1851; and *Provincial Medical and Surgical Journal*, July 9, 1851.

ON OVARIAN IRRITATION. BY DR FLEETWOOD CHURCHILL.

This affection resembles most closely sub-acute ovaritis, but differs from it in not being inflammatory. Age appears to have little influence in the production of the disease, and it is most frequently found in women of delicate nervous temperament. The principal symptom is an uneasiness, amounting in most cases to pain—in a few instances to very severe pain, in one or both iliac regions, but most frequently in the left, which Professor Simpson attributes to the propinquity of the left ovary to the rectum. It is accompanied with a sensation of fulness about the iliac region, considerable tenderness, occasionally hysterical paroxysms, and the irritation sometimes extends to the bladder. Nothing unusual is detected by a vaginal or rectal examination—the ovaries are certainly not enlarged, but uneasiness is produced by moving the uterus to the side affected. The different circumstances under which the attacks occur, are the following:—

I. Occasionally from amenorrhœa.
II. The sudden suppression of the catamenia.
III. Dysmenorrhœa, secondarily affecting the ovaries.
IV. Menorrhagia.
V. Congestion and erosion of the cervix uteri.
VI. In hysteria, both in the normal and abnormal states of the catamenia; and,
VII. In sterility accompanied with a highly nervous temperament.

As to the pathology of the disease there can be no doubt, from the peculiar

and fixed locality of the pain, and its frequent connection with menstruation, that the ovaries, one or both, are the seat of the irritation; nor from the various symptoms can the conviction be resisted, that the affection is essentially neuralgic, and not inflammatory. It does not appear that ovarian irritation, although it may disturb the menstrual function in various ways, is the cause of menstrual disorders; it is rather a concomitant symptom.

Cold, excess in sexual intercourse, and in some instances deprivation of that stimulus, and any of those causes calculated to disturb the functions of the uterus or ovaries, may be considered as among the causes of ovarian irritation.

It is to be diagnosticated from acute ovaritis, by the absence of enlargement, and by the tenderness present being much greater than that resulting from inflammation, being, in fact, of a nervous character.

It is to be distinguished from pelvic abscess by the want of tumefaction at the brim of the pelvis.

In the treatment of this disease, leeches to the ovarian region, followed by poulticing, and even warm poultices without leeches, small blisters, anodyne liniments, anodyne plasters and enemata, tincture of aconite, applied freely to the iliac region, have all been more or less followed with relief. But that which can be most confidently recommended to the profession, is opium applied to the upper part of the vaginal surface, made up in the form of medicated pessaries, similar to those used by Dr Simpson, each ball about the size of a large marble, containing two grains of opium, half a drachm of white wax, and a drachm and a half of lard. This has been tried in a considerable number of cases, and with almost invariable success.—*Abridged from a paper read before the Association of the College of Physicians of Ireland.*

Part Fourth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING V.—*July* 5, 1851.—Professor BENNETT, P., in the Chair.

REPORT.

1. *Mr Drummond* reported on the urinary sediment shown by Dr Bennett at last meeting, that chemical analysis showed the presence of a large quantity of uric acid in combination with ammonia. He had not been able to detect any other important ingredient in the matter subjected to examination.

ORIGINAL COMMUNICATIONS.

1. *Dr Bennett* showed a series of preparations illustrative of the development of the chick in ovo. The larger proportion of them had been given to him by Mr Jones, and had gained the prize in his physiological class; but a number of them were presented to him by Mr Drummond, particularly one showing very well the structure of the amnion.

STRUCTURE OF NEW VESSELS IN FALSE MEMBRANE.

2. *Dr W. T. Gairdner* exhibited under low microscopic powers a portion of a false membrane, dried on glass, so as to show vessels filled with blood running

within it. In regard to this preparation, Dr G. made the following statement:—

The membrane now presented to the Society is one of a kind not very uncommon. It was found within the arachnoid cavity, in contact with the inner surface both of its visceral and parietal layer, and so disposed as to enclose between its folds a large clot of blood which had been extravasated (probably from the longitudinal sinus) into that cavity. The case is of some interest; but as its details do not involve any new scientific principles, it is not necessary to allude to it farther than to say that the fatal disease was distinctly referable to an injury received five weeks before death, and by which the patient, a man sixty years of age, fractured his humerus. The head symptoms came on gradually in the form of coma, with typhoid febrile symptoms, but no delirium. There is, therefore, no reason to doubt that the membrane was in this instance of hemorrhagic origin, as in various other cases related by authors, and referred to in an excellent paper by Mr Prescott Hewett, of St George's Hospital, in the twenty-eighth volume of the "Medico-Chirurgical Transactions." The existence of vessels in such membranes has also been shown by injection, in a preparation by Mr Holmes Coote, in St Bartholomew's Hospital, as well as by others in St George's Hospital Museum. Although, therefore, there is nothing new in the facts now visible or demonstrable in regard to this membrane, the Society may find the following observations, made while the specimen was recent, not devoid of interest:—

The portion of membrane examined was about two inches square, being all that could be preserved. It was removed from the surface of the dura-mater and falx cerebri, and had at one place a transverse irregularly varicose streak, of purple colour, quite obvious to the naked eye, but scarcely so defined or sharp in the edge as to have the character of a vessel. Crossing this, however, on a different plane, were a great many smaller branching vessels, still visible to the naked eye or a simple lens, and with a very defined outline.

On submitting a portion of the membrane at this part to a power of forty diameters, the walls of the smaller vessels could be very distinctly seen as a well-defined clear space on each side of the red streak which marked the *channel* of the blood. In the larger and varicose blood-channel, however, no distinct wall of any appreciable thickness could be traced; nor could any be decidedly said to be visible even under higher magnifying powers.

A small portion of the membrane, containing some of the smaller and more distinct vessels, was dissected out on glass, and treated with acetic acid. Examined under a power of two hundred and fifty diameters, it had the general structure of fibrin. In most parts there were traces of elongated nuclei, but these were in no great numbers, and not well-marked, except at the site of the vessels. Here the nuclei were disposed in a longitudinal series in the axis of the vessel, showing the wall to be altogether composed of longitudinal fibres. The nuclei were in some parts very much elongated, forming twisted wavy fibres.

I squeezed some blood out of the largest vessel, and examined it under two hundred and fifty diameters. It showed many well-formed blood corpuscles; but it appeared to me that I could trace them distinctly from the very smallest appreciable size up to that of the normal blood corpuscle. Except in size, I did not see much difference between what I suppose to be the young and the perfect blood corpuscles; but the number of those of the inferior or intermediate sizes was, I think, decidedly greater than in normal blood.

There were few or no white corpuscles.

Besides these elements, there were very many irregular-shaped coloured granules of various sizes, up to $\frac{1}{100}$th of a line, such as are found in many circumstances along with disintegrating blood. They were easily distinguished from the blood-corpuscles by colour, form, and re-action with acids, which they resisted. These granules were scattered throughout various parts of the membrane, to which they appeared to communicate the rust-coloured mottling. Although, therefore, I think they were also floating free in the blood of this large

vessel, I am not *positively* able to demonstrate that they were so in a manner free from possible fallacy.

Another element found in the matter squeezed from the vessel was a certain number of those very delicate hyaline cells, which frequently occur in pathological exudations, and which I have generally called simple transparent or soap-bubble vesicles. They were invariably non-nucleated, and had all varieties of size up to $\frac{1}{150}$th of a line.

This observation presents several points of great interest bearing on the still unsettled questions of the origin of new blood and blood-vessels. In regard to the blood taken from the most considerable vessel, it is to be observed that it differed from normal blood—firstly, in the very varying size of the red corpuscles, and in the great number of such corpuscles which were much smaller than the normal type; and secondly, in the absence of white corpuscles. It is difficult to understand on what principle these differences can be explained, except by supposing the blood in this vessel to be *young blood*, formed within the false membrane in a vessel as yet unconnected with the circulation. This inference would have been still farther probable had the observation that coloured granules of disintegrating hematin existed in this blood been free from a possible fallacy; for it is obvious that had the new vessels been in contact with the general circulation, no such granules could have existed in their blood. Admitting for a moment the inference correct, that this was actually young blood, it is clear that this observation would directly disprove the development of the red corpuscles from the white, and would tend to show that the latter are not in any way necessary to the development of the blood.

A somewhat similar observation has been published by Vogel,[1] in regard to small points which he found in a granulation on the end of a bone, and which he supposed to be newly-formed blood. But in this case neither was the blood enclosed so distinctly in a new vessel as in the present instance, nor was it apparently so characteristic a specimen as regards the different stages of growth of the red corpuscles.

With respect to the new vessels, it is worthy of observation that the smaller ones had in this case more perfect walls, and were evidently more advanced in development than the larger varicose tubes from which the blood was taken for examination. This fact appears to be quite at variance with any theory of the formation of new vessels by peripheral extension from the older ones; as, according to such a theory, the smaller capillaries would be first formed, and some of them would gradually become developed and widened into more considerable channels, acquiring, at the same time, thicker and more complex walls; so that in all cases the progress of development in the walls of a vessel would necessarily be in exact proportion to its size and its proximity to the original tissue. Such a relation undoubtedly prevails to a certain extent in the new vessels formed in granulations on a free surface; but in observing the vessels of false membranes, I have repeatedly been struck with the fact that no such relation can be traced. On the contrary, the new vessels in a false membrane appear to be channelled out, as it were, in the direction of its fibres, and without any reference to the vessels of the adjacent structures; and may be occasionally seen in regularly arranged groups of five or six long parallel vessels, all of them of considerable diameter, but of most rudimentary structure, and totally destitute of transverse fibres or nuclei, throughout their entire length. From the present preparation it appears that the vessels of most considerable size may even have the most rudimentary structure, and from an examination of other specimens I am satisfied that the development of such vessels may be quite as far advanced in the more distant parts as in those close to the parent membrane. From these circumstances it would appear probable that the vascular structures in a false membrane, instead of being the result of a series of developments from the capil-

[1] Pathological Anatomy. Dr Day's Translation, p. 549.

laries of the parent tissue, are independent formations, and are as much involved in the ideal plan (if I may use the expression) of the membrane, as the chimneys of a house, or the streets of a city.

John Hunter supposed that, in the organization of extravasated blood, vessels were formed in the same way as in the membranes of the chick, where they take their origin independently of each other, but following a harmonious arrangement, and afterwards combining into a system. Although the proofs of this doctrine were at the time, and perhaps still are, deficient, I must confess that the results of observation, as well as considerations of analogy, incline me strongly to adhere to it, and even to extend it to the case of granulations, which that great physiologist, as well as many modern observers, have viewed as receiving their supply by an extension of the subjacent vessels. It is difficult to conceive of any direct evidence being led on this subject, except by the multiplication of observations like the present, under perhaps even more favourable circumstances. If it can be established by repeated and independent observations, that the newest vessels of false membranes are often of considerable size from the first, and contain a blood having all the characters of a new formation, the conclusion will be irresistible, that both the blood and the vessels containing it are developed from germs within the new structure itself. Without wishing to rest so important a conclusion upon the above observation alone, I have thought that these facts ought to be laid before this Society, where they may probably be the means of attracting additional attention to the subject.

ON THE TERMINATION OF THE SMALLEST SPLANCHNIC NERVE IN A CASE WHERE THE CORRESPONDING KIDNEY WAS SITUATED IN THE PELVIS.

3. *Mr Zaglas* mentioned to the Society the following details of a dissection he had lately performed, illustrating an abnormal distribution of the nerves of the abdomen.

It is not a very rare occurrence to find a kidney situated lower down than usual, and repeated observations have been published in which it had descended even into the small pelvis. In a case which, in the course of the last winter, presented itself in the dissecting rooms of the Edinburgh University, the left kidney of a male middle-aged subject, was found lying across the common iliac vessels, and bending over so as to apply its lower half on the contiguous parietes of the small pelvis. An artery from near the bifurcation of the aorta, and another from the common iliac, supplied it with arterial blood.

In this case I was anxious to ascertain its supply with nerves, and chiefly to see how the smallest splanchnic nerve, which usually joins the renal plexus, would terminate. The nerves surrounding the above-mentioned arteries had contracted connections with the aortic and hypogastric plexus, and with corresponding sympathetic ganglia of the main cord. The splanchnic nerve did not join them nor any other abdominal plexus. It emerged from amongst the fasciculi of the left crus of the diaphragm, and was seen stretching some distance downwards along the crus and the psoas muscle, which latter it then entered. I now followed the nerve within the substance of the psoas, in a direction downwards and backwards towards the spinal column, and was satisfied that it gave no off-set to the muscle. When near the column, the neurilemma of the nerve began to form an aponeurotic expansion, which then gradually coalesced with the tendons of origin of the psoas muscle and with the periosteum of the lumbar vertebræ (I think of the third and fourth), and thus became effaced.

The man died dropsical, but with no appearance, at least no prominent structural alteration, of the kidney concerned here, which on the contrary, seems to have perfectly fulfilled its office in the economy for a long period without a connection with the nerve. Beyond this statement, perhaps, nothing can be said with certainty in the present state of obscurity in the anatomical relations of the sympathetic system.

With regard to the genetic process of this abnormity, the following may appear tolerably correct. Whatever the cause was that produced the displace-

ment of the kidney, it also caused the separation of the kidney from the nerve, and this in a primordial stage of development, when the muscles had not as yet acquired their distinctness. The nerve then became blended with the spinal column, and afterwards (when the psoas had advanced in its development) was overlapped by that muscle and comprehended between its fasciculi.

4. *Dr Bennett* introduced the subject of the nature and character of cells, normal and pathological, with a view especially to the determination of the grounds on which it might be argued that specific distinctions existed between the different varieties of cells known. The questions touched upon by Dr Bennett led to an animated and protracted conversation, in which Mr Zaglas, Mr Cobbold, and Dr W. T. Gairdner took part.

Meeting VI.—*July* 11, 1851.—Dr Bennett, P., in the Chair.

COMMUNICATIONS.

1. *Dr Bennett* exhibited a microscopic demonstration of the blood from a patient in the clinical wards. The white corpuscles were in excessive numbers, though not to so considerable an extent as in other specimens shown to the Society.

COMPLETE CONGENITAL FISSURE OF STERNUM.

2. *Dr Bennett* showed a preparation of a sternum from a female, in whom the bone was cleft throughout perpendicularly in the mesial line, owing to malformation. The xiphoid cartilage was also cleft. The two halves of the bone were in contact at the lower end, and diverged to the extent of several inches superiorly.

Mr Zaglas was appointed to report on this case.

RETENTION OF OVUM AT FULL PERIOD IN THE UTERUS OF A COW.

3. *Mr Barlow* exhibited twin fœtuses taken from the uterus of a young and apparently healthy cow, which, when killed a few days ago, was not known to be pregnant. Judging from the state of their dentition and development of the hoofs and bones, they must have died in utero about the expiration of the natural period of gestation. Their heads, necks, and extremities were doubled upon the bodies, so as to occupy comparatively little space. The skin, muscular tissue, and viscera, were dried and shrunk into less than one-fourth of their natural bulk and weight, and formed a tough, brown, almost homogeneous substance, adhering closely to the bones, which could be plainly felt and separately distinguished. They were closely invested by dried membranes, which were smooth, shining, slightly hardened, moistened with a mucous secretion, but unattached on their uterine surfaces. Each fœtus was accurately notched or fitted to the other, and adhered to it by means of the membranes and an intervening glutinous substance, so that the two were moulded into one ovoid mass (quite destitute of external inequalities), about the size and shape of a large gourd. As no history of this case could be arrived at, it was impossible to speak as to the period during which this dried or mummy-like fœtal substance had been retained in the uterus. Comparing, however, the appearances here exhibited, with those seen in instances wherein the records of gestation were known, it is likely that the fœtuses in question have been contained in the uterus at least six months beyond the natural time of parturition. Cases of death of the fœtus and its subsequent retention in utero, are far from unfrequent in domestic ruminants, and in some instances the periods of retention have been known to extend so long as twelve and even eighteen months beyond the natural gestatory period. It is also somewhat remarkable that cows and ewes, in this condition, will often fatten with rapidity, and evince all the usual indications of good health.

ON THE EXISTENCE OF CILIA IN THE VITELLARY MEMBRANE.

4. *Mr Drummond* read the following communication, which was illustrated by drawings:—

When the ovum of batrachians, after it has been deposited by the female, is examined with the naked eye, it is found to consist of a dark-brown spherical mass, of about $\frac{1}{12}$ of an inch in diameter, surrounded by a layer of a gelatinous or mucus-like substance, $\frac{1}{8}$ inch in thickness. The internal dark mass corresponds to the vitellus; the outer gelatinous substance is not analogous to the albumen which we find surrounding the egg and yelk of the birds, inasmuch as it does not appear to be albuminous in composition, but presents more the re-action of gelatine or mucus. It does not coagulate when boiled; on the contrary, it dissolves almost completely. It is probable, therefore, that it is only a layer of mucus serving as a protective covering to the vitellus. When the egg is examined more carefully, and with a power of about thirty diameters, we find that the outer or mucus-like layer is not in contact, by its inner surface, with the vitellus, but that there is a space left between the two. This space is limited internally by the membrane of the vitellus, and externally by the inner surface of the mucus-like layer, which latter presents here a very distinct well-defined margin all round, as if it were lined by a membrane. In this space, between the vitellus and mucous layer, which is filled with fluid, I observed that the particles of dust which were contained in the fluid, were moved about with a constant and rapid motion, and that sometimes the whole vitelline mass was rotated around its axis. On examination with a power of 250 diameters, these movements were found to be produced by the action of vibratile cilia which covered the whole outer surface of the vitellus. I endeavoured to ascertain the exact time at which these cilia made their appearance; but as the season was somewhat advanced, I could not find eggs young enough for the purpose. With respect to what becomes of these cilia, as the outer membrane of the vitellus is developed into the skin of the frog, the cilia which clothed the vitelline mass I found remained as appendages to the skin of the animal for a considerable time after it left the egg stage. At first they clothed the entire skin of the animal just as they had done the vitelline membrane, but disappeared by degrees; those covering the branchia remaining longest.

It is interesting to observe that Bischoff has noticed a somewhat similar appearance in the ova of the dog. He observed that the vitelline mass in the ovum of that animal, whilst passing along the Fallopian tube, presented a series of rotatory movements; and on examination with high powers, found these to be produced by the action of cilia which clothed the surface of the vitelline membrane. He was not able to ascertain what became of these cilia afterwards, or when they disappeared, and he believes that their office was to assist the ovum in its passage to the uterus.

On consulting the paper of Lebert and Prevost on the development of eggs of the batrachians, I find that they also had already noticed the vibratile cilia; but state that they could not ascertain that the cilia were attached to any particular series of cells. That they are, however, attached to the cells forming the covering of the vitellus, is, I think, without doubt; because, when the vitellus is broken up, I have seen the cells which composed its membrane moved about over the field of the microscope by the action of the cilia attached to their surface.

5. *Dr W. T. Gairdner* showed a drawing, illustrating a remarkable case of melanosis of the skin. It occurred in the integuments of the right leg, in the form of dark-coloured tubercles of the usual form, closely aggregated together, and at some points ulcerated. A similar diseased condition was found in the inguinal and lumbar glands, lungs, and pleuræ. The melanotic matter presented the structure described at a former meeting.

ON A PECULIAR FORM OF CYSTIC DISEASE.

6. *Dr W. T. Gairdner* showed a drawing of a very singular elongated serous cyst, found in the anterior layers of the great omentum of a woman who died of fibrous tumour of the uterus. The cyst was preserved in the University Museum. The patient died very unexpectedly, with symptoms resembling peritonitis; but neither this, nor any other disease sufficient to explain death, was found on dissection, the details of which are given below:—

Large Fibrous Tumour of Uterus, containing Cysts; Cysts in Omentum, in Cellular Tissue, and in Velum Interpositum (Pineal Gland?)

The dissection (*April* 4, 1851) was unavoidably hurried and imperfect.

A woman, æt. 37, pale, but not emaciated.

On opening the abdomen, a very large resisting tumour appeared, having nearly the size and relations of a uterus in the seventh or eighth month of pregnancy. The surface was tolerably smooth, not nodulated, and was covered by peritoneum. The peritoneal sac contained only an ounce or two of very slightly turbid fluid, without any flakes of appreciable size. The surface of the serous membrane not abnormally congested. The tumour could be readily lifted out of the abdominal and pelvic cavity, when it was seen to be attached inferiorly to the uterus. It had no adhesions, except one at its lower third, to the edge of the omentum, which presented a singular arrangement. It did not cover the tumour in front except at the lower part, and when drawn out, was found to be stretched over the right side and back part of the tumour, from which it emerged into view at the lower and right margin. The omentum was remarkably thin and elongated, so much attenuated at most parts that its individual layers presented rather a fine network than a continuous membrane.

To the anterior layer of the omentum was attached a remarkably long and narrow cystic structure, of generally tubular form, slightly rounded and closed at either extremity, and presenting at some points partial constrictions with intervening gibbosities, in some degree like the external surface of the distended colon. The length of this cyst was between three and four feet, its breadth varying from half an inch to an inch. It was apparently covered by the anterior layer of the omentum, as its external surface showed numerous branched vessels, which communicated with an artery and vein, the size of a large goose-quill, running within the omentum, at the upper border of the morbid structure. The walls of the cyst, as seen externally, were therefore composed of the special membranous cyst-wall and the thin omental vascular layer; they appeared semi-transparent, and allowed the contained clear fluid, with numerous floating flakes, to be readily seen. The upper end of the cyst, which was the larger, was situated near the middle point of the omentum, where it was attached to the stomach. From this point it descended, passing towards its left border, and from this towards the right, keeping near, but not in contact with, the free border of the omentum. No trace of any other solid contents than the above-mentioned flocculi could be seen in any part; the cyst was not, however, cut open through its whole length (being preserved for the University Museum by being injected with spirits); notwithstanding this circumstance, the transparency of the parietes permits of its being said, with confidence, that no considerable organic structure could have existed in the interior or attached to any part of it without being observed. A small portion of the cyst was cut open, when it was seen to be lined internally by a very thin and very smooth membrane, precisely similar to that commonly found in simple serous cysts; the external constrictions corresponded to slight membranous projections like very imperfect septa; but it could be ascertained that fluid communicated freely between all parts of the cyst, which was nowhere subdivided by any approach to a complete partition. Some of the flocculi from the interior of the cyst, on examination by the microscope, presented a filamentous appearance, with some imperfect nuclei and cell-structures, but no trace of any more perfect organisation.

The intestines appeared healthy, but were not examined. Liver, kidneys, spleen, pancreas not examined.

The uterus was considerably elongated (to three or four inches), and at its fundus the above-mentioned tumour was attached to it. Imbedded in its muscular substance were several smaller tumours, none of them exceeding the size of a hazel-nut. All of them, as well as the larger one, presented on section the usual appearance of the uterine fibrous tumour. In the largest growth there were several irregularly-formed cysts, containing a matter like recently coagulated fibrin, containing in its meshes much clear fluid. (Under the microscope, some cell-structures were visible in it, but nothing characteristic or approaching in character to pus.) The large tumour was fed by numerous very large vessels, some of them not less in size than a goose-quill. The interior of the uterus contained some grumous blood, without coagula; but the mucous membrane presented nothing abnormal.

The ovaries were both enlarged to the size of walnuts, and congested. They contained likewise a few simple cysts.

Bladder and urethra not examined.

The thoracic organs (heart and lungs) were free from disease, except that the latter were of small bulk, flaccid, and scarcely crepitating posteriorly. The bronchi contained a slight excess of mucus, and their membrane was very vascular.

In both groins, immediately above Poupart's ligament, there was present a cluster of cysts in the cellular tissue, of irregular form, and containing watery fluid, with numerous fibrinous flocculi of a yellowish colour. The lymphatic system in the neighbourhood appeared quite normal. The chain of femoral lymphatics was examined in the left limb, and found normal.

The brain and its membranes were carefully examined. The nervous tissue was mostly very firm and natural in appearance, but rather pale. Fluid on the surface moderate; in ventricles very small in amount. The choroid plexus and velum interpositum remarkably pale; in the folds of the latter, occupying as nearly as possible the situation of the pineal gland, was a semi-transparent cyst, the size of a wild cherry, and oval in form. The lower wall was somewhat thicker than the upper; it had a small cellular pedicle, which connected it with the surrounding vessels, and adhered to them also by its entire surface, which was slightly flocculent; it appeared to contain a watery fluid. In the neighbourhood of this body was a slight thickening of the velum interpositum, which may have been the atrophied pineal gland; it was not particularly examined. The base of the brain and the arteries normal. The interior of the cranium unusually smooth, and free from all trace of osteophytes.

Dr G. remarked that he was not aware of any thing in the records of morbid anatomy at all resembling the peculiar elongated cyst in the omentum in this case. Its length, and the peculiarity of its form and situation, suggested irresistibly the idea of its being a parasitic structure; but if it was to be regarded as an entozoon, then it must be one of unusually simple organisation, and more analogous to the ordinary rounded serous cysts than had yet been described. The conjunction of it with other forms of cystic disease, which, though not unique, were sufficiently uncommon, made this case one of unusual interest both to the scientific pathologist and to the comparative anatomist.

LAW AND QUACKERY.

John Stephens, of Ratcliff Highway, grocer, surrendered to take his trial at the Central Criminal Court, on August 23, 1851, for the manslaughter of Harry Stewart Davis. It transpired on the trial that John Stephens carries on the business of potato salesman on one side of his shop, and that of grocer on the other; and that he also practises as a quack doctor. The powder administered to the deceased is sold in large quantities under the name of "Composition Powder." The enema contained some of this powder, with the addition of lobelia and valerian.

One witness (the shopman who had sold the powder) stated that he had taken pounds of it himself, and it had never done him any harm. He had also taken lobelia. The "composition powder" was said to consist chiefly of "bayberry bark and hemlock spruce bark." Dr Letheby stated that he detected chiefly cayenne pepper and ginger. Being a vegetable compound, the exact composition could not be ascertained by analysis. It was, however, proved to be a powerful stimulant.[1] The intestines of the deceased were found to be in a very inflamed state, but there was also organic disease of other parts, which gave the prisoner the benefit of a doubt.

Mr Justice Wightman in summing up, explained to the jury, that the principal question for their consideration was, whether the prisoner had been guilty of such gross rashness or exhibited such gross ignorance in his treatment of the deceased as occasioned his death, and in that case only would they be justified in finding him guilty of manslaughter. It would be very hard indeed upon a medical man, or any other person, if, because the patient he was attending died, he was to be subjected to a charge of manslaughter; and if it were to be so, very few men would like to incur the responsibility of practising the science of physic. Did the evidence in the first place satisfy the jury that the medicine was of a poisonous and dangerous character? They had heard the testimony of the witnesses on this point, and it also appeared that the medicines had been taken by some of the parties, who had derived great benefit from them. He was bound to tell them, that the fact of these having failed in one instance, was not sufficient to make out a case against the defendant; although it might appear that it would be improper to administer such medicines in a case where the stomach was already in a state of inflammation, yet there did not seem to be any evidence to show that the prisoner was aware of the fact. His lordship then read over the evidence, commenting on the material parts as he proceeded.

The jury immediately returned a verdict of NOT GUILTY.

The Editor of the "Pharmaceutical Journal" remarks:—"In accordance with the charge of the judge they could not have returned any other verdict: nor indeed, on the principle laid down, could a verdict of guilty be returned in any case of death under the treatment of a quack. In the first place, the same allowance is conceded which would be made in the case of a medical practitioner, who, possessing full qualification, and using due care, skill, and attention, had failed to cure his patient: and then the fact that the quack was ignorant of the condition of the patient is admitted as an excuse for the administration of improper medicines. While this maxim is adhered to, there is more protection and encouragement given to quacks than to qualified practitioners. We maintain that in the case of a qualified practitioner, where the propriety of the treatment is called in question, the *onus probandi* should rest with the accuser, because it is fair and right to infer, in the absence of evidence to the contrary, that a qualified professional man has availed himself of his skill and experience in the treatment; but in the case of the quack the *onus probandi* should rest with the accused, who having undertaken that for which he is not legally qualified, cannot be expected to possess the requisite skill and experience."—*Pharmaceutical Journal*, September 1851.

TREATMENT OF THE INSANE IN THE HOSPITALS OF CONSTANTINOPLE.

Two wards were devoted to insane,—one for males, the other for females. When I first entered the office, and expressed a wish to examine the charity, permission was given, and a mere boy sent to show the way. On expressing a wish to enter where the lunatics were confined, some reluctance was manifested, but the young

---

[1] Dr Coffin, in his book, gives two formulæ for "Composition powder"—Bayberry, 4oz.; pinus canadensis, 2oz.; ginger, 2oz.; cayenne, ¼oz.; cloves, ¼oz.; all finely pulverized and well mixed. Ordinary dose a teaspoonful in a cup of hot water sweetened.—Or bayberry, 4oz.; Pinus canadensis, 4oz.; ginger, 4oz.; golden seal, 2oz.; sassafras, 2oz.; cayenne, ½oz.; cloves, ½oz.

man yielded to earnest importunity, though he positively refused to lead me to the wing where the females were located. Not more than forty in all were under the care of the hospital. All the males were lying on their matresses upon the floor, wearing iron collars about their necks, from whence a chain extended to a strong staple in the floor. They were far enough asunder to prevent interference with each other. A boy of about fifteen, heavily laden in this manner, in one corner, was imagined to be a furious maniac, as an apology for his confinement. A wound on the side of his head might have been an injury inflicted on himself in a paroxysm of fury. On returning to the office, two of the directors being present, the inquiry was made why we wished so urgently to see the house. I then made known my profession, and further observed, that having come so great a distance it would be a subject of deep regret not to be permitted to examine one of the best conducted hospitals in Constantinople—a privilege never denied a medical traveller in any part of Europe or America. Having discovered that I was from the latter country, they were extremely polite. Sherbet was brought in, soon followed by coffee, and they then, with apologies, accompanied me to the wards of the female lunatics. Their treatment was similar to the others. The improved system of kindness and indulgence has not yet been introduced anywhere in this country. The Greeks will do it first, if it is ever sanctioned at all, for their intelligence and ambition to avail themselves of the wisdom that distinguishes foreign countries are apparent. Although out of place, I will here remark, that in the neighbourhood of Beyroot the monks have possession of a cave in which maniacs are shut up, while the holy impostors exorcise the devils who have possession of their unhappy patients. Such is the reputation of the place, that lunatics are invariably carried there from great distances. If the efforts are unsuccessful, they are then sometimes disposed to consult physicians.—*Boston Medical and Surgical Journal*, August 1851, p. 440.

PUBLICATIONS RECEIVED.

Homœopathy: Report of the Speeches on Irregular Practice, delivered at the Nineteenth Anniversary Meeting of the Provincial Medical and Surgical Association. London. Churchill. 1851.

On the Local Origin of the Yellow Fever Epidemic of British Guiana. Letter from Daniel Blair, M.D., to John Davy, M.D., with Appended Documents. Lond.: 1851.

On the Preservation of the Health of Women at the Critical Periods of Life. By E. J. Tilt, M.D. Lond. Churchill. 1851.

The Anatomy and Diseases of the Prostate Gland. By John Adams, F.R.C.S., Eng. London. Longman. 1851.

On a Peculiar Form of Thrombus occurring during Labour. By W. F. Montgomery, M.D. From Dublin Quarterly Journal of Medicine.

Collection of Facts Illustrative of the Morbid Conditions of the Pulmonary Artery. By Norman Cheevers, M.D., &c. Lond. 1851. (Reprinted from London Medical Gazette.)

Dr W. T. Gairdner on Bronchitis. (Reprinted from this Journal.)

Dr Alexander Christison on Indian Hemp. (Reprinted from this Journal.)

Report of the Alleged Lunatic Friends' Society; with Pamphlets. Lond.: 1851.

A History of Epidemic Pestilences, with Researches into their Nature, Causes, and Prophylaxis. By Edward Bascome, M.D. London. Churchill. 1851.

The Prevention and Cure of many Chronic Diseases by Movements. By M. Roth, M.D. London. Churchill. 1851.

TO THE EDITOR OF THE "NEDERLANDSCH WEEKBLAD VOOR GENEESKUNDIGEN."

We have received the three specimen numbers, and proposal for exchange, which is accepted. Dr Gildemeester will oblige us by transmitting to us, through the house of *Messrs Williams and Norgate*, Henrietta Street, Covent Garden, London, the parcel of back Numbers, from 1st July to the present date, and by sending through the same channel future Numbers as they appear. The Monthly Journal will be sent to Dr Gildemeester in the same way, commencing with the July Number,—*i.e.*, No. 1 of the Current Volume.

THE EDITOR OF THE "VERHANDLUNGEN DER PHYSICALISCH-MEDICINISCHEN GESELLCHAFT IN WUZBURG."

We accept the exchange. Numbers 14–22 of volume I., and 1–5 of volume II. have been received. Will the editor kindly send, with his next parcel, through *Messrs Williams and Norgate*, Nos. 1–13 of vol. I.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Present State of Medical Evidence.* A Lecture delivered before the Royal College of Physicians, Edinburgh, on 10th March 1851. By ROBERT CHRISTISON, M.D., Professor of Materia Medica in the University of Edinburgh.

[THE following observations were delivered at one of the monthly meetings of the Royal College of Physicians last winter, when the College was honoured by the attendance of several members of the Bench and many of the Bar. They were not originally intended for publication; but the author has been requested in various quarters to make them generally accessible. He is conscious that views, which might be appropriately presented to a select audience of the legal and medical professions, would be the better for greater development and more copious illustration when offered to the public at large; but the necessary extension of them would require more leisure than he can at present command. In their present shape, however, they may attract the attention of others to the subject; and if this be accomplished, their object can scarcely fail to be attained sooner or later; for no one can study carefully the existing state of medical evidence without perceiving, both that great defects exist in it, and that medical science may supply a remedy.]

MR PRESIDENT AND GENTLEMEN,—The subject which I propose to bring before you this evening is one which some time ago engaged my earnest attention, soon after I was appointed Professor of Medical Jurisprudence in our University.

At that period—more than five-and-twenty years ago—an acquaintance with medical jurisprudence was confined in my own profession to a few studious amateurs. And, if the necessities of practice involved a more frequent knowledge of it among lawyers, that knowledge, nevertheless, could not be very deep or very sound, so long as it was necessarily derived in a great measure from the medical sciences, which at that time were not specially studied in their medico-legal relations by their own proper followers.

Hence in this department of law the principles of action were vague, and the practice of courts fluctuating. Medical evidence was usually loose and inconclusive, or rash and contradictory, limited in its range, and held in low esteem. And what else could

be expected, when the physician or surgeon, in his novel position of medical witness, did not discover till too late that an important branch of medicine had been left out in his education; and the lawyer that, instead of having simple matters of fact to deal with in his examination, he was unexpectedly plunged in the depths of sciences unknown to him?

The first step taken towards an improvement of these things was the admission of medical jurisprudence in 1825 into the curriculum of medical study in our own University. The professorship had existed for eighteen years, but languished under great discouragements. At last the subject was partially recognised in Edinburgh as a branch of the graduate's education in the autumn of 1825. In 1830 a thorough recognition in the regulations of the Royal College of Surgeons of Edinburgh placed it on a par with the other branches of professional study for surgeons. In 1833 the example of that College was followed by the Senatus Academicus of the University; and other boards of medical education in the three divisions of the empire also concurred in the same measure at a later date. A professorship of medical jurisprudence too was founded at Glasgow and in the University-Colleges of London, a lectureship in each of the Universities of Aberdeen, and others in the London Hospital-Schools, as well as in the University and Royal College of Surgeons of Dublin. Thus everything which could be done by the patrons of education in the way of encouragement has been at last, although somewhat tardily, accomplished.

Another important element of progress was, that about the commencement of the same period, medico-legal inquiries, for the first time in this country, began to be frequently chosen as the subject of express original research and practical observation. The present sketch is not the place for a history of medical jurisprudence, otherwise many familiar names might be noticed in connection with these researches. But I hope that, without incurring the charge either of egotism, or a neglect of others, it may be allowed me to refer briefly to my own endeavours; because, unless this be done, it may be difficult to find favour for the views which will be submitted by and by for your consideration.

On my appointment as professor of medical jurisprudence in 1822, it was natural for me to feel sensibly its low estate, and my duty to raise it if I could. For this end it was desirable to choose a particular branch only of a subject so comprehensive as to extend both its roots and ramifications into all the other medical sciences. My choice fell upon toxicology,—in itself indeed no narrow field, but one almost untrodden by British cultivators, yet at the same time more full perhaps of varied interest than any other, better fitted at all events to attract general notice, and also more likely to yield good fruits in the hands of a young labourer.

Fortunately, at the same time, a wise innovation—for which, as

for many more important improvements, our criminal law is indebted to the late Sir William Rae, when Lord Advocate—placed the professor of medical jurisprudence in direct contact with the practice of his science, and taught him to preserve a practical bearing alike in his lectures and in his private inquiries. The professorship had at this period existed for nearly twenty years; yet, though founded by the Crown, its occupants had seldom or never been consulted by the Crown officers in medico-legal questions. On the contrary, when any of them appeared in the arena of a law court, his weight was thrown into the opposite scale, and so far as may be judged of from hearsay, rather to the obstruction of justice than the discovery of truth. Sir William Rae, perceiving probably the evils of this arrangement, and the growing influence of medical jurisprudence, consulted its professor on all important criminal cases, especially in questions of poisoning, and made him a witness, when such testimony seemed advisable.

The consequences to the practice of our criminal courts, resulting from all these changes, were soon apparent through every branch of medico-legal science; but in none so manifestly as in the most difficult department of all, that of toxicology. In trials for poisoning prior to this time, the proof of the fact of death by poison was looked forward to by the public prosecutor with more anxiety and distrust than any other part of his case, and by the opposite counsel as the most promising outlet for the prisoner's escape. But a total change gradually took place in this respect; and at length, after a hard forensic conflict in 1827, on the occasion of the trial of Mrs Smith for murder by poison,—when the medical evidence, based on the most modern improvements in medico-legal analysis, was vainly assailed by all the skill of the most eminent pleaders of the Scotch bar, with the aid of able and zealous medical advisers and medical witnesses,—the fact of death by poison became an article of proof on which the crown lawyers could confidently rely in every case which they judged fit to send to trial; and it is now no uncommon thing for the prisoner's counsel to admit that part of the case altogether, and to look in quite another direction for the chance of a successful defence.

The conclusion I would venture to draw from this brief narrative, and my reason for placing it before you, is, that such an advancement having been attained in the most difficult department of medical jurisprudence, there seems no reason why every other should not be placed in the same favourable position, if similarly cultivated and similarly fostered. It may seem surprising, indeed, that this object has not been already accomplished; but an untoward occurrence, which I would willingly here evade, if it were possible, soon afterwards put a stop to all further progress, by undermining the confidence of law courts in medical evidence.

The general cultivation of medical jurisprudence naturally inspired medical men, when they had now to appear as witnesses,

with a reliance on themselves which they had been very far from feeling during their previous imperfect acquaintance with the subject. Instead of shunning criminal and civil trials as formerly, many now rather courted the notoriety of the witness-box. Some even carried their liking so far that, if not consulted on one side of a case, they had no objections to be consulted on the other,—thus attempting to engraft the part of counsel upon that of witness. The example of medical controversy on the occasion of the trial of Mrs Smith seemed to engender an epidemic contentiousness in the profession, and this at a time when, by an unlucky coincidence, medical polemics were in the ascendant in this city. Hence it was apparently enough that certain parties were cited on one side of a civil or criminal trial, for certain others to appear, by a sort of instinct or custom, on the opposite side. The grounds of procedure on these occasions were probably not always well considered by the actors; at all events, by both bench and bar they were thought not wholly free from suspicion. The confidence of lawyers in medical testimony was thus severely shaken; and the estimation in which medicine had been previously held received a blow from which it has not even yet recovered. But unjustly. For, while the fault was that of a few, not of the many, and still less of medicine itself, nevertheless the obloquy, which might have been rightfully visited on those who erred, fell not upon them, but was extended to our profession at large by a hasty and undiscerning generalisation; and those, who might have cured the evil by a little judicial discipline, preferred to censure medical evidence in the abstract, and to moralise over the uncertainty of physic and the discordances among physicians.

But in truth the fundamental cause of such misadventures, and of the present imperfections in medical testimony, as well as in the practice of our law courts respecting it, lies deeper and elsewhere than would seem to have been generally imagined. No branch of medical jurisprudence has been so little studied systematically by either lawyers or medical men as the most essential of all for its healthy development—namely, that which relates to the principles and practice of medical evidence. Lawyers have taken little pains to penetrate into the intimate structure of medical evidence. No adequate attempt has been made by them to instruct medical experts and medical witnesses in their medico-legal duties. Nor, on the other hand, has any competent authority in physic laboured hitherto with success to throw light on the medical side of the subject.

Most of the standard legal authorities on the law of evidence barely touch upon that part of it which rests on medical science. They do so, some more and others less particularly, in order to mention the practice of law courts in receiving medical evidence; but no one has attempted a critical analysis of it, which certainly seems surprising, if we consider either the vast importance of such

evidence in the majority of cases in which it is called for, or the frequent complaints of these writers as to its imperfections.

In *Baron Hume's* work on crimes, where he treats of the proof in criminal proceedings, no notice is taken at all of the duties or evidence of the medical witness. *Burnett*, in his work on criminal law, may or may not have had it in view, when he says in a single line, that "witnesses must in general speak to facts, and not to opinions either of their own or of others" [P. 609]. *Phillipps*, an esteemed author on the special subject of evidence, merely observes that, as a witness should speak to facts only, his opinion is in general not evidence; but that on questions of science or trade persons of skill are allowed to give both facts and opinions; and that medical persons may thus give their opinions on facts stated by others, as well as those observed by themselves [i. 288]. In *Glassford's* "Principles of Evidence," the sum of his statements on medical testimony amounts almost exactly to what has been stated by Phillipps; but there are also little detached passages throughout his book which show, that he had penetrated into its nature and peculiarities further than most other authors on this subject, and which raise a regret that he had not investigated its structure more expressly. *Tait*, copying the statement of Phillipps, adds the lamentation, that "it is found by experience that scientific men often differ in opinion, and that their testimony is often very uncertain"; but he makes no attempt to discover why it is so. *Starkey*, an English author of high estimation, remarks more precisely than his precursors, that "the testimony of medical men is constantly admitted with respect to the cause of disease or of death, * * * and the general state of the mind of the patient, as collected from a number of circumstances;" and that "such opinions are admissible in evidence, although the professional witnesses found them entirely on the facts, circumstances, and symptoms established in evidence by others, and without being personally acquainted with the facts" [i. 154]. *Peake*, developing somewhat the latter observation, says, that a physician, who has not seen a particular patient, may after hearing the evidence of others, be called to prove on his oath the general effect of the disease described by them, and its probable consequence in the particular case [P. 195]. *Chitty*, in his work on medical jurisprudence, goes no further in his exposition; but seems to doubt the admissibility of such evidence in one extensive field of practice,—namely, in questions of lunacy [P. 359]. *Greenleaf*, a recent American authority, of great reputation both in the United States and in this country, simply transcribes the observations of Starkey. *Mr Alison*, in his "Practice of the Criminal Law of Scotland," has gone more minutely, as will be seen hereafter, into the practice of Scotch courts, relative to the management of medical evidence; and, resting apparently on his extensive opportunities of observation while he was an advocate-depute in the criminal courts, he

gives a more precise shape to the disparaging opinion of Mr Tait, and complains "that medical men, even of character and information," should have been found by experience to be "generally so prone to contradict one another, and to adhere to the side on which they are cited" [P. 545]. I must here again be allowed to express my regret, that one who has written so much and so well on the practice in regard to medical evidence, should not have been led to investigate its principles, and that he should have wound up his statement with a severe, though not unmerited censure, without being tempted to inquire, by an analysis of medical evidence, into the source of defects which must have struck him so forcibly. Lastly, *Professor Amos*, of University College, London, in his lectures published in the "London Medical Gazette," for 1830-31, has learnedly expounded the English practice as to the reception of medical evidence, and laid down many excellent practical rules for the conduct of medical men in their medico-legal capacity; but, like Mr Alison, he has not made any attempt to analyse medical evidence; while, like him too, he proclaims it as "a great reproach to the medical profession that, on the occasion of celebrated trials, the medical witnesses on one side and the other have contradicted each other in such a point-blank manner in their opinions delivered on oath" [*Med. Gaz.*, vii. 613].

Can nothing then be done to remove the opprobrium thus fixed upon our profession and the sciences we cultivate? I am not vain enough to suppose that I can accomplish this. But it may at all events contribute something to so desirable an end, that we shall all see clearly what there is in the constitution of medical evidence, as well as in the practice of law courts, which leads to the uncertainties and contradictions thus so generally complained of by lawyers.

We turn in vain for this information to the medico-legal writings of members of our own profession. For, if legal writers on evidence have dropped the subject at the confines of medicine, as little have medical authors taken it up there. In the "Treatise" of *Dr Paris* and *Mr Fonblanque*, the remarks made on the law of evidence have only a general application, and do not bear on the specialties of medical evidence. The elaborate "Elements" of *Dr Beck* are scarcely less meagre on the subject. In the more recent "Manual" of *Mr Taylor*, the general topic of medical evidence has been omitted altogether. In 1824, however, a separate volume was published expressly on this theme by *Dr Gordon Smith*, afterwards for a short time professor of medical jurisprudence in University College, London. But with much literary research, sundry judicious suggestions as to the rules of evidence observed in courts of law, and some sound advice to medical witnesses, this rather bulky work fails nevertheless in developing the principles and structure of medical evidence, and wants that practical character, which its learned author would undoubtedly have given to it, had he been more

engaged than he actually was in the practice of medical jurisprudence.

It is possible that these writers, in thus passing by the intimate nature or structure of medical evidence, did so because they regarded it as too elementary and self-evident to require notice from them. But if this be the explanation of their silence, they have been in error. Not indeed that anything very new or recondite can be said on the subject. On the contrary, it will be, I trust, no slight test of the truth of the succeeding statements, that they will appear, to every well-informed member of the medical profession, as propositions which either actually have, or ought to have, suggested themselves before to his mind. But I apprehend nevertheless, that they have been both too little studied, and too lightly weighed, otherwise it is difficult to account for the continuing imperfections of medical evidence, or for the prejudices which have in recent times prevailed against it.

The general doctrine in law, that witnesses must speak to facts only, and that their opinions are not admissible as evidence, rests on two grounds. The first is, that the forming of opinions is the proper province of the jury. This is usually the only reason assigned. But another, not less important, is that an opinion can seldom be formed on any branch of a case, however subordinate, without the witness exercising his judgment as to the truth of the facts deposed to,—a function which belongs even more peculiarly to the province of the jury.

But the general doctrine must be violated when questions of science arise in the course of the evidence; because the jury do not possess the requisite scientific knowledge, either to determine the truth of the facts, or to draw from them sound conclusions. Hence, in questions of law occurring upon trials, the judge is interposed between the facts and the jury; and his opinion is imperative. For the same cause, in questions of medical science, or of physical science generally, professional or scientific men are interposed, who, though nominally witnesses, really discharge the function of judge; but with the material difference, that the jury is not bound to adopt the judgment. Notwithstanding this difference, however, it stands to reason, that the medical witness should be placed as far as possible in the same favourable circumstances with the judge for forming an opinion. It will be seen by and by, whether or not courts of law have studied to provide him with that advantage.

Medical evidence consists partly of facts and partly of opinions.

I. Medical facts are derived by the witness in part from the evidence of sense, but in part also from that of testimony,—that is, from his own observation, or from that of other witnesses.

The facts derived from his own observation are thus obtained as

evidence, sometimes because he has been incidentally cognisant of them in the ordinary exercise of his profession, and sometimes because he is employed, in criminal cases, on the part of the crown or the prisoner, in civil cases, by either party in the cause, expressly to conduct professional inquiries relating to it.

In the former case,—viz., when deposing to facts occurring to him incidentally to the ordinary exercise of his profession, a medical witness is usually considered more trustworthy than one who deposes as a witness employed expressly to investigate the facts; for the facts observed in the former way, having been observed without any relation to the questions arising in the cause, are more likely to have been noticed without bias. For it is a principle in law, as well as in reason, that "evidence is more or less deserving of credit, as it is more or less undesigned" [*Glassford*, p. 308]; or according as it was prepared or not for the case.

This principle, however, has been allowed too wide an influence in the appreciation of medical evidence in courts of law. An exception has indeed been tacitly admitted in the instance of "experts" employed on the part of the crown to conduct medical inquiries in criminal proceedings; because, as the public prosecutor, at least in Scotland, can very rarely have any other than a public interest in any such case, so the medical expert, as his functionary, can have no conceivable bias, except indeed that universal bias from which no man, whether lawyer or physician, witness or judge, can be entirely exempt,—the desire to support his opinion when it has been once dispassionately formed. In civil trials, on the other hand, a decided preference is usually shown in courts to incidental medical evidence, over that which is the result of express investigation undertaken at the request of parties or their agents. And this is in general right, so long as the present objectionable mode of obtaining such evidence is adhered to. At the same time I cannot help remarking, that frequently in civil, and sometimes even in criminal law, too decided a preference is given to medical facts incidentally observed over those ascertained expressly for the case. For scarcely any medical man determines the whole particulars of a medical case for the purposes of ordinary practice with the same care which is taken when he knows that his observations are to be made the subject of review in a court of law. And besides, the medical witnesses, who conduct inquiries expressly for the cause, are selected always for their skill, and commonly also for their character; while incidental witnesses must be taken as they come, and indeed are on many occasions, especially in criminal trials, and for an obvious reason, of no great professional standing. On these accounts, the objection arising from possible bias on the one hand is counterbalanced by that depending upon probable loose observation on the other. And consequently, if the risk of bias could be excluded, as it might in general be by a modification of the present mode of admitting the evidence of "experts," I apprehend that in that case the general

principle now under consideration might be reversed in the instance of medical evidence.

The facts of medical evidence are derived not only from the personal observation of the witness, but likewise from the testimony of others. And herein lies much of its weakness.

In legal inquiries, in truth, it is but a small part of the facts constituting the basis of medical opinions that rests on the evidence of the medical witness' own senses. Always a large part, often the whole, is supplied by the observation and memory of others, who being unprofessional persons, or even although members of his own profession, may be little competent to observe, to appreciate, or to recollect.

Of the facts derived from testimony, some are deposed to in court in the cause, and others have been collected previously out of court, and with more or less, or no, legal formality. These circumstances must variously affect their validity, both in law and in reason and common sense.

A preference is always given to those facts of testimony which are directly sworn to in the course of the trial, as having been observed by the witness who deposes to them. The reason for this is, that they are subject to the test of cross-examination [*Amos*, Med. Gaz., vii., 548]. But I may observe, that medical facts of this kind, though subject to the test, are far from being always subjected to it; for neither counsel nor judge may be always aware when they require it. Hence I have often had occasion to notice that they are allowed more weight than they deserve, when they come to be applied as the basis of medical opinions.

There is no more important principle of the law of evidence, than that it ought to be founded only on facts sworn to in court. A well-known exception, however, exists in regard to the statements of a deceased person, made in relation to the cause while aware that he is upon death-bed. But a far wider exception has also been long tacitly allowed in law courts, and is now expressly admitted by some legal authorities on the law of evidence,—viz., the history of his own case by a deceased person, although not aware he is dying, or though not upon death-bed at all, given to his medical attendant, or even to an unprofessional person, who deposes to the statement in the cause. Thus *Mr Amos* observes, "Are we then to infer, that what a patient says to his medical attendant can rarely become matter of judicial investigation? By no means. Inquiries by medical men, with the answers to such inquiries, are often received as evidence of the state of health of the patient at the time" [vii., 548]. So too *Mr Alison* says, that in English practice "the account given by a deceased person of his own bodily state during illness, or immediately after injury has been received," is admitted in evidence, although hearsay [Practice, 521]. It is remarkable, however, that this statement should be restricted by

the author to English law, since nothing can be more certain than that the same rule is constantly followed in Scotch courts, where, in charges of murder or culpable homicide, and on trials for reduction of wills on the plea of insanity, or of death-bed, as well as for nullity of assurances on lives, both medical and other witnesses are daily allowed to depose to medical facts of extreme consequence, which they obtained from the deceased patient though unaware that he was dying.

It is quite plain indeed that medical evidence in many cases could not exist without such a rule and practice; for in that way we obtain not only the best, but even the only attainable evidence. On the other hand, it is equally clear that the rule should be applied with circumspection, and if I may trust my own experience, not without more circumspection than is usually shown. It is not to be supposed that even a medical man, and much less an unprofessional person, while not at the moment aware of the probable future importance of his inquiries, will be so observant of what a deceased person says of the particulars of an illness, of their order of occurrence, of their progress and dates prior to the witness seeing him, as the witness would subsequently wish that he had been, when he finds himself deposing to all these facts in a court of justice. Nevertheless, I do not think I have often seen adequate circumspection observed in receiving such hearsay evidence,—an omission which can scarcely be referred to any other circumstance than that, by education, neither judge nor counsel can be prepared effectually to test and check it.

I may here notice, from the trial of Mrs Smith for poisoning, an apt illustration of an important imperfection thus left in the proof. The prisoner was seen to administer a potion to her maid-servant in a suspicious manner on a Tuesday evening; and in three days the latter died of poisoning with arsenic. A fellow-servant, however, who slept with the deceased, did not observe her to be ill at all on Tuesday night, or particularly so till the evening of next day; on that day indeed she was even able to eat a hearty dinner. But her medical attendant, who, visiting her for the first time on Friday evening, found her very ill and evidently dying, and who did not then suspect poison, but thought she had cholera, deposed that she told him she had been exceedingly ill with burning pain, vomiting, and purging all Tuesday night and all Wednesday. Had this fact been clearly established, the proof of administration would have been very strong. But it rested on hearsay evidence; and the witness who supplied it, neither at the time of collecting it, nor I suspect even at the time of delivering it, was aware of its extreme importance. Nor did the court seem to be aware of this, nor to observe the discrepancy between his evidence and that of the fellow-servant of the deceased until it was too late; when another professional witness, who did notice the discrepancy, explained that the operation of arsenic could not be suspended for so long a period

as from Tuesday evening, when the suspicious potion was administered, until the evening of Wednesday, when the symptoms of poisoning were by all accounts undoubtedly developed. Which of the two opposite statements was correct? The direct evidence of the fellow-servant,—or the hearsay evidence of the surgeon? A very grave question. For the proof of administration was the only weak point of the case against the prisoner. A more searching examination might have cleared up the apparent contradiction; and had it done so, the prisoner would have scarcely escaped with a verdict of "Not Proven."

As facts derived from testimony constitute often so material a part of medical evidence, it is essential that he who has to form opinions from them shall be placed in favourable circumstances for detecting them, for judging of their truth, and for appreciating their bearings and their force. In England this is easily accomplished; because witnesses are allowed to be present during the whole trial, so as to hear one another's evidence. And in criminal trials there is additional access to information through the publicity given to the preliminary proceedings, both of the coroner's jury, and of the police magistrate. It is only in very special circumstances, and on rare occasions, that the witnesses at an English trial are excluded from court, and not unless this be requested by the counsel, for reasons which are assigned. It is very difficult in Scotland; where the practice, whether in civil or criminal courts, is a rigorous separation of the witnesses at the precognitions, and exclusion of them from court upon trials until the examination of each begins. Speaking of precognitions, *Mr Alison* says, "It is a proper precaution in magistrates to separate witnesses as much as possible during their examination" [Practice, 489]. On trials too, as he observes, "witnesses should not remain in court to hear the depositions of the other witnesses who precede them" [Ibid. 542]. And in point of fact this precaution is now followed in both circumstances much more scrupulously than might appear from Mr Alison's mode of enunciating the rule.

The rule in question, however, has been by no means invariably observed. On the contrary, until a rather recent date, it had been very much relaxed in the instance of medical witnesses. My own experience will sufficiently show how unsettled really has been the practice. On the first occasion of my being cited professionally for the crown, viz. in 1825, I had not been concerned with the medico-legal proceedings at the precognition; no information was communicated to me about the case in any other way, and I did not hear the evidence at the trial. As it was eventually found unnecessary to examine me, I know not how counsel proposed to do it. For the trial was one for murder by a blow on the head; and the main question, which related to the connection of a fatal erysipelas with the injury inflicted, was to be cleared up principally by medical facts, obtained throughout the evidence of the general wit-

nesses, and such as no unprofessional man could accurately collect and arrange. Next year, on the trial of David Kennoway before the High Court of Justiciary for parricide, a partial improvement was admitted, by allowing the medical witnesses to hear all the evidence of the general witnesses. But they were excluded as soon as the medical examinations commenced, even as to facts merely. In the autumn of the same year, on the trial of Clark at the Perth circuit for the murder of an infant by locking it up in his writing-desk, the medical witnesses were allowed to be present during the whole trial. And this was at the recommendation of the judge himself, the late Lord Gillies; who justly remarked, that "in such a case, where much depended on the state and appearance of the child's body when first seen by unprofessional persons, and on the nature and position of the articles in the desk which had compressed and marked the body, nobody but a medical man could tell what were or were not medical facts in the evidence of the general witnesses." In the High Court of Justiciary the somewhat undue latitude of this arrangement soon afterwards underwent some restriction. And in 1827 on the occasion of the trial of Mrs Smith, it seemed to be decided, for the first time with some degree of form, that the medical witnesses should be admitted to hear all the general evidence and all the facts of the medical evidence, but not the statement of the case by counsel, or the medical opinions of one another. In conformity with that rule, the precognition was often put before the medical witnesses prior to the trial, to facilitate their inquiries as medical experts, or for their opinion in consultation. This wise and reasonable adjustment, suggested I believe by the late Sir William Rae, when Lord Advocate, and sanctioned by the present Justice-General when Lord Justice-Clerk, and at the head of the criminal court, became to all appearance so firmly fixed, that it has been quoted by good legal authority as part of the criminal practice of the country. Thus *Mr Alison*, speaking of the usual practice of excluding witnesses in general from the court, remarks: "To this there is an exception in the case of medical witnesses; who should remain to hear the deposition of the witnesses who depone to the facts of the case" [P. 542]. "To this rule," he repeats, "there is an important exception in the case of a medical witness called on to give a professional opinion; who not only may, but ought to, remain in court when the examination of witnesses *in causa* goes on." "In all the late trials for murder, accordingly, * * * the medical gentlemen on both sides, who were to give a professional opinion, were present during the whole time." "But," continues he, though they "should hear the whole *facts* of the case detailed by the other witnesses, whether professional or ordinary, who are examined in the cause, yet it is usual, when one medical man begins to give *an opinion*, to cause other medical men to retire" [P. 535]. And so of precognitions, where he speaks of the usual practice to keep the

witnesses ignorant of one another's declarations, he excepts "the case of a medical witness who is called to give an opinion merely professional; for he should see the precognition, or hear the declarations of the witnesses, as well as the subjects on which he is called to speak, on the same principle on which he is directed to remain in court during the trial, till the medical *opinion* of the witnesses begins" [P. 489].

Mr Alison's work was last published in 1833; and for some years afterwards the rules he thus inculcates continued to be observed in all courts. But within nine years more a change was again made, and the original exclusive practice was suddenly restored in all its rigour; and so it now remains. In civil as well as criminal trials, medical experts, and medical men consulted for their opinions, are not now allowed to see the precognition. Medical witnesses are not permitted to remain in court, to collect for themselves the facts on which their opinion is to be founded. In the preliminary proceedings they are told only what the counsel or agent may consider to be medical facts, and sometimes very little even of them. And upon trials, they are dependent for the elements of their opinions on the counsel and the judge; who indeed do their best to collect and arrange the medical facts for them, and put questions bearing on the case, but as much as possible in general terms, and without reference to its specialties. One or two deviations from this rule have occurred on the circuits, but it is rigorously observed in the High Court of Justiciary.

The reason for this retrograde step has never been publicly assigned. But it cannot be any indefeasible principle in Scotch law; for the preceding narrative shows, that the law is sufficiently pliant in this matter to admit of almost any rule. I may therefore be allowed to point out the objections which exist on the side of medicine to the present system;—objections which every medical man must have felt, who has been engaged in medico-legal practice.

The first objection to be stated against it is, that it tends to suppress and conceal medical facts. It is impossible for judge or counsel to know always what facts in the general evidence are medical, and what not. No one will doubt the aptitude with which the lawyer acquires for the occasion a knowledge of the particular art or science concerned in a law case. But it is not to be supposed that any aptitude or any practice of the kind will enable him to single out from the testimony of the general witnesses the whole facts which bear upon medical opinion. It has been already mentioned, that one of the shrewdest lawyers who ever occupied the Scottish bench once made this admission in court. I may now mention a remarkable illustration of its justice. In 1831, on the trial relative to the disputed insurances on the life of the late Earl of Mar, which it was maintained had been vitiated by concealment of the vice of opium-eating, the presiding judge, the late Lord Chief Commissioner Adam, told the jury in his charge, as a fact in the

case, that although there was evidence of the Earl having purchased laudanum at the rate of nearly three ounces a-day for some years, there was no evidence of his having actually taken it to that excess. There was indeed no direct evidence of his having done so. But the judge, not being a medical man, had not perceived the medical bearing of a most pregnant little fact which was sworn to by the Earl's housekeeper; who said she gave him every morning before he got up so large a dose as a tablespoonful of laudanum at once. As he was also proved to have taken habitually several other doses during the day, the whole quantity was approximatively accounted for. But—which is still more pointed—the enormous dose, and the time of day when he took it, were to every medical man irrefragable proof that Lord Mar must have long been a practised and slavish opium-eater [Med. and Surg. Journal, xxxvii. 130].

Another objection is, that facts are liable to be misstated and misrepresented, owing to errors of language. One of the chief reasons for the preference we give to the direct testimony of our own senses over the testimony of others, is the risk of error from the necessity of the intervening use of language. The medical witness is subjected by the present rule of Scotch courts to a twofold risk of this kind, inasmuch as he receives the facts, not directly from him who witnessed and describes them, but through the intervention of a third party. And the risk of error is all the greater, that many words have a double meaning, one general, and the other scientific or professional. Some years ago, on an important trial in the High Court of Justiciary for assault, the public prosecutor attempted to prove, that the person assailed had been wounded to the effusion of his blood; which is held in law to be an aggravation of guilt in such cases. When the principal medical witness was examined as to the injuries inflicted, he was asked whether any blood had been effused; and he replied that a good deal must have been effused. But he meant that there was effusion of blood under the skin, constituting the contusion he had described; while the counsel and court at first received his answer as implying that there had been considerable loss of blood from a wound. The latter view was on the point of passing to the jury as a fact, when one of the judges detected the equivoque, and set the matter to rights. But the incident is not the less illustrative of the risk of serious error, in circumstances in which scarcely any medical man could have gone wrong, had he been duly pre-informed as to the bearings of his evidence.

A third objection is, that the medical witness has no opportunity of judging of the truth of the facts, on which he is asked to found his opinion. The same professional skill, which enables him to collect the facts better than another, makes him also a better judge of their authenticity. I am aware of the theory in law, which holds that the jury alone are to judge of the truth of the facts. I apprehend, however, that this doctrine may be meant to apply only to

those facts whose truth depends on the veracity of the witness. But at any rate, both in regard to such facts, and all others indiscriminately, the rule is violated by the present practice of Scotch courts in examining medical witnesses. The facts do not pass to them with the stamp of authentication from the jury. For the questions put to the witnesses are framed from the facts, and put generally by the counsel, and occasionally by the judge, but very seldom by the jury. In truth, the rule must be inevitably violated, whenever evidence consists of opinion from facts deposed to by others; because no man in such circumstances can either form an opinion as a witness, or shape questions to elicit an opinion from a witness, without exercising his judgment as to the authenticity of the facts. I do not here refer so much to facts whose authenticity depends on the veracity of the witness who deposes to them. His evidence as to facts, whence medical opinions are to be deduced, may be morally true, and nevertheless scientifically false. And it is mainly their authenticity in the latter respect, which is referred to at present, as a matter which must be determined before an opinion can be safely formed from them. According to the existing practice of Scotch courts, this is done by the counsel and the judge. Is it not more rational that it should be done by the witness himself, who alone is qualified by professional education to exercise such judgment?

No one can hesitate how to answer that question, who is acquainted with the nature of medical facts,—the acuteness required for observing them,—the experience for appreciating them,—the judgment for distinguishing fact from opinion. I would beg in particular to remark, that few are aware, and no unprofessional person can be adequately so, of the great risk that opinion shall be mistaken for fact.

Facts being mostly cognisable by the external senses, they are received in evidence much more unreservedly than opinions; which, besides the exercise of observation, involve that of many other faculties of the mind, all much more liable to error. Hence opinions, if taken for facts, will be apt to be admitted as such with undue facility, because not duly tested by the judgment. Spurious facts of this sort abound in pure medicine. How much more then in medical jurisprudence, if supplied by unprofessional persons! Suppose that in a trial on account of a disputed assurance, an ordinary witness deposes that the party insured had at a certain date palsy in the limbs. To a non-medical eye nothing could be liker a simple fact. But it is no such thing. The affection may have been lameness, left by an old rheumatism. It may depend on stiffness of one or more joints from local disease or injuries; it may be part only of a general debility, affecting, as it often does, the limbs more than the rest of the body; it may be a mere awkwardness of gait. In calling the affection palsy, then, the witness does not state a fact, he forms a diagnosis, he delivers an opinion, and an opinion very

likely to be wrong; for it is sometimes no easy matter, even for a medical man, to make up his mind as to the existence of paraplegia in its early stage. How often upon trials does it happen, that in describing the appearances in suspected poisoning, the witness says he saw inflammation of the stomach! and this is received as a statement of fact. But is it really such? Several characters are necessary to constitute inflammation: some of these may be imitated by the state of the stomach at certain stages of digestion; others by pseudo-morbid appearances arising spontaneously a short time before or after death; others by the action of extraneous agents subsequently. The witness therefore does not describe a simple fact: he forms a pathological opinion. And were any farther proof necessary to establish this, I have but to remind you how often he is discovered to be wrong. Some years ago a man in London was tried for murdering his mistress, and throwing the body into the Thames. His defence was, that they determined to drown themselves together; but that his resolution gave way in the water; and that, while he with difficulty saved himself, he failed in his efforts to save also his companion. But three surgeons, in this matter no wiser than three ordinary observers, swore at the coroner's inquest, that they found contusions on the head and face of the girl's dead body. This passed for a fact, and to most unprofessional persons would seem a very simple fact. The man was accordingly tried for his life. But on the trial a fourth surgeon swore, that he afterwards dissected the alleged bruises, found no traces of congested or extravasated blood, and that in his opinion the appearances were no more than the livid marks which are apt to arise in many bodies after death. A somewhat similar case occurred in this city some years before, in which the prisoner justly escaped through the evidence of the late Mr John Bell and Mr Fyfe.

If I may trust my own experience of proceedings in criminal and civil trials, the confounding of medical opinion with medical fact is a matter of very frequent occurrence, and one serious cause of the imperfections of medical evidence. Nor is it rashness of criticism to express a fear, that errors originating in this cause must be very apt to happen under the present system, which requires medical witnesses to form their opinion on the facts, without a previous opportunity of sifting them.

A fourth objection is, that the medical witness can have no correct idea of the bearings of his evidence on the case. This is too evident to need illustration. The proposition indeed may with reason be stated more strongly. For he will be very apt to take up an incorrect idea of the bearing of his evidence; inasmuch as questions framed upon general propositions, or supposed conditions, cannot fail to give him some insight, and yet a very imperfect and probably incorrect one, into the ordinary facts and nature of the case.

It will probably be replied, however, that according to a principle in evidence, testimony is in general of most weight, when the

witness does not know how it affects the cause. "It is not necessary," says *Mr Glassford*, "that a witness should understand the purpose of his examination, or the inferences to be drawn from his answers: on the contrary, his want of opportunity, and even his incapacity, for so doing, may in some cases confirm the truth of his information" [P. 53]. This may be all very sound doctrine in respect to evidence consisting of facts only. But in matters of opinion, any little strength thus given to testimony is far more than counterbalanced by the great risk of the court, in ignorance of medical science, or misled by the terms of the opinion, giving it a force neither designed by the witness, nor borne out by scientific principles. And this consideration is all the more cogent, that it is often the interest and the aim of counsel to bring about such a result. I do not know how medical witnesses now usually feel under the new practice of excluding them from the general evidence and medical facts deposed to in court. But I must confess for my own part, that having often, as a witness under the older and more open practice, seen the necessity of an explanation or condition, to prevent an erroneous conclusion or application, which would otherwise have been deduced from my answer, and which I was able to anticipate only because I knew the general facts and the bearing of my evidence on them,—I should entertain now a reluctance to appear as a medical witness, and a fear of error in that capacity, to which I have hitherto been a stranger.

The fifth and last objection I have to notice is one which applies as much, if not more, to the practice of withholding the precognition from the medical witness, as to his exclusion from the evidence of the other witnesses at the trial. This is, that the range of medical evidence is thus circumscribed, to the serious obstruction of the ends of justice.

Not many medical men, perhaps, and certainly few lawyers, are aware how comprehensive would be the scope of medical evidence, under a better judicial practice, and in the hands of able medical witnesses. To substantiate this statement here would require considerable details, otherwise it could be accomplished with no great difficulty. It may perhaps suffice, however, to mention, that there are not two opinions on the subject among well-informed medical jurists. And their convictions on this head refer to all kinds of medico-legal inquiries, whether civil or criminal; but most of all to those connected with trials for homicide, assault, child-murder, and other heinous offences against the person. For it is not only the mere cause of death or injury, which may be cleared up by medical evidence. Equal light may be thrown by it upon the intent with which the injury was inflicted, the time of infliction, the means employed, the force exerted, the number of persons concerned, the profession of the assailant, his very person even, with sundry other minor points which it would be tedious to enumerate. Nor should

the important consideration be overlooked, that medical evidence may supply a direct and impartial test of the veracity of the ordinary witnesses in circumstances when their truth might be justly doubted.

This last object is of such consequence, that I may be permitted to illustrate it by a single instance, among many which I could supply from my own experience. On the trial of Mrs Mackinnon, a brothel-keeper, who was executed here in 1823 for murder, it was stated in evidence by the companions of the deceased, that on their going into the place on their way from an evening entertainment, a squabble arose between them and the inmates; which ended in the deceased receiving a fatal stab in the chest. At the examination of the body, at which I was present, though not officially, I remarked that the wound, which was situated over the cartilage of the second left rib, penetrated towards the left, backwards, and very much downwards, into the lungs. On then asking one of the deceased's companions, who happened to be present at the inspection, how the injury was inflicted, he said he saw the prisoner approach the deceased with a long table-knife held dagger-wise, and drawing a blow from her left ear, strike him downwards, forwards, and to her right side. But the prisoner alleged in her declaration, that she merely held the knife before her, sloping upwards, to deter the deceased from attacking her; and that he, being drunk, stumbled forward as he advanced, and fell upon the point of the knife; and this statement was in some measure confirmed by her lodgers. Here were two very contradictory accounts,—and an impartial test of their respective truth was much to be desired; for the whole individuals in the scuffle were more or less intoxicated; one party was disreputable, and the other not absolutely the reverse; so that the evidence of neither was quite to be depended on. Such a test was supplied by the particular direction of the wound; which was wholly incompatible with the declaration of the prisoner, and perfectly conformable with the evidence of the companion of the deceased.

Now, what I have to urge is, that unless the medical experts and witnesses are made well acquainted with the precognition, and hear the testimony of the general witnesses at the trial, evidence of this kind must be very apt to be overlooked, and the range of medico-legal inquiry consequently circumscribed, to the sacrifice of truth, and the obstruction of justice.

II.—In thus treating of medical facts, as a branch of medical evidence, so much has been necessarily said also of medical opinions, that little remains to be stated under that head, which would not be mere repetition. But there are nevertheless some additional considerations which must not be lost sight of.

In matters of science opinion depends on the possession of accurate powers of observation, habits of close reasoning, and extensive

well-digested experience. On the first two of these qualifications, it is unnecessary to say anything here. But the third, or the possession of experience, deserves to be attentively considered; because its true nature, as concerned with medico-legal opinions, is often misapprehended both in and out of our profession.

Experience is of two kinds; direct, or knowledge acquired by personal observation; and derived, or knowledge obtained from the observation of others, that is, learning. In ordinary medical practice both acquisitions are highly esteemed. But in the practice of medical jurisprudence an undue preference is apt to be shown for direct experience; and learning has often been unjustifiably undervalued. So much indeed has this precious possession been sometimes underrated in courts of law, that medical witnesses have on several occasions been expressly forbidden to make use of published authorities, and have been enjoined to confine themselves strictly to their own personal knowledge. Nor is this legal prejudice even yet so entirely exploded as to be safely passed by in the present exposition.

The objections in law to the use of derived experience are, that the facts have not been sworn to, and that the witness is made judge of their truth instead of the jury. These objections have been felt so strong by some judges, that they have pronounced such evidence altogether inadmissible. Thus, at the trial of Mr Donnall at Launcestown in 1817, for poisoning his mother-in-law with arsenic, which was proved to exist in the stomach after death by means of two liquid tests only,—a medical witness for the prisoner stated that he had found phosphate of soda to be similarly acted on by these tests as arsenic; and that phosphate of soda might well exist in the stomach, because the contents were bilious, and this salt had been found in all sorts of bile by Thénard. But Mr Justice Abbott here stopped him, observing that "the Court cannot take the facts from any publication, or as related by any stranger" [Paris and Fonblanque, iii. 291]. Nevertheless, the authority of Thénard, in a question of chemical science, was of course very much superior to that of the witness, who thus appropriately referred to him. So too, at a prior trial, that of Miss Butterfield, in 1775, for poisoning Mr Scawen—a case which turned mainly on the question whether mercurial salivation may recommence six weeks after its cessation without mercury having been again taken, —one of the medical witnesses for the prisoner proposed to read a passage from the treatise on poisons of Dr Mead, who was at that time the highest English authority in toxicology, and who mentions a case of the kind with an interval of six months. But Sir Sydney Smithe, the presiding judge, forbid him to read it, and told him to confine himself to his own personal experience. Other English judges, however, have taken a different view of this question. On the trial of Spenser Cowper for murder in 1699, Dr Crell, a principal witness, was allowed, after a brief altercation with the Bench,

to "quote the fathers of the profession," and availed himself of the permission somewhat unmercifully. At a trial in London for child-murder in 1802, a witness, who ignorantly swore at the coroner's inquest, that the child had been born alive, because the hands of the dead body were clenched, a thing which he had never witnessed in still-born children,—retracted that opinion on the trial, because a friend, whose name he mentioned, had assured him that he had seen clenched hands in a fœtus which undoubtedly had never breathed; and this reference was allowed by the presiding judge, in face of one of the best established rules of evidence. But the most decisive opinion that has hitherto been delivered on this subject in England, is that of the late Chief-Justice Dallas; who, when a medical witness attempted to disparage the evidence of written medical authorities, by saying that "the writers of books would advance anything," severely censured this impertinence, observing that he "would not sit in a court of justice, and hear science reviled, and the recorded researches of the medical world represented by ignorant tongues as leading only to uncertainty" [Amos in *Lond. Med. Gaz.* vii. 613].

The practice in Scotland has never, to my knowledge, been so irrational as that enjoined by the opinions of Justice Abbott or Sir Sydney Smithe. But I doubt, on the other hand, whether sentiments have ever been expressed from the Scotch bench so strongly on the side of learning as by Chief-Justice Dallas. Nevertheless, his views are both philosophically and practically sound. Facts in science, as laid down by authors of standard reputation, will bear comparison for truth and stability with any body of ordinary facts sworn to in a court of justice. Nor can any one be so partial to that particular form of enunciating facts as to suppose, that the cause of truth in science would be a gainer were every book sworn to be true by its author; or that this precaution would bind him more strictly than the solemn paction of sincerity which he tacitly makes with the world by the very act of publication.

Let me not be supposed to undervalue direct experience. But let us nevertheless see what it exactly amounts to in matters of medico-legal inquiry. The value of any one's direct experience on a particular point must depend, not on direct experience alone, however large, but mainly on his having compared and combined it with the vast stores of derived experience, amassed by his predecessors, and generalised as it has accumulated. Were a witness, in a trial for poisoning with arsenic, to recommend his evidence to the court, as I have known some do, by stating that he had seen some two or three cases of the kind before, and that the symptoms and morbid appearances in these cases corresponded with what were observed in that under consideration,—this direct experience would really mean nothing more, than that he had an opportunity of verifying, by a few personal observations, the facts and principles derived from the classified testimony of hundreds of prior

observers. A man of true learning, thus applying the process of verification, may draw his conclusions as soundly at the first as at the third observation; and is likely to do so much more soundly at the first, than will ever be done on any occasion by one who, affecting to despise learning, boasts that he relies only upon his own narrow opportunities of direct experience. A few years ago a surgeon in the north was called to a patient with obstinate stomach complaints and colic; which the means he employed relieved from time to time, but did not remove. Puzzled with the inveteracy of the case, he was one day pondering at the patient's bed-side what could be the cause, when his eye fell upon a water-bottle which was lined inside with a brilliant, white, pearly, crystalline film. He instantly saw that he had to do with a case of slow poisoning with lead. He had indeed never seen a case of lead-colic before, but he had read of it. He had never seen a water-bottle so silvered over until then, but he had read of that too, in an account of the only parallel incident on record. He applied a test to the water in the bottle, and detected lead in it. He went to the cistern, which he found lined like the bottle, with white carbonate of lead. He found lead in water fresh drawn from the supply-pipe, which brought the water from a spring at a great distance. Finally, the water was analysed, and found to be one of those which act on lead. Now, suppose this incident had led to a legal inquiry,—were another of the kind to occur indeed, the engineer would well deserve to be made an example of,—and this surgeon had been asked, according to a prevailing practice with counsel, whether he had ever seen such a case of poisoning before, would his evidence have been invalidated by his reply? Assuredly not. This gentleman, Mr Johnston, of Peterhead, had read as if he had seen. He drew his conclusions from experience, though not personal. And since nothing could have been wanting to the conclusiveness of his opinion as it would have stood, so nothing could have been added to it in that respect, had he witnessed, instead of merely perusing, the previous case. Some years ago I was requested to inquire into a mysterious case of poisoning with arsenic. I had not previously seen any one during life who had taken that poison. But by frequent reading, lecturing, and writing on the subject, the varied forms of arsenical poisoning were as familiar to me as if they were all marshalled before my eyes. There was arsenic in the dregs of a tea-cup, from which the poison was said to have been drunk. Nevertheless, I was convinced from the symptoms, that the case was one of pretended poisoning; I pronounced it such; and such it was proved to the satisfaction of all, by an analysis of the first matter vomited, in which there was not a trace of arsenic. Now, of what avail here would have been the opinion of one acquainted with arsenical poisoning by personal observation only. He might very well have seen even a dozen cases, and yet none but its common forms, and thus been ignorant of the only forms which there is any difficulty in

distinguishing. Half instructed merely, and all the more full of prejudice on that account, he would have given an opinion, with confidence indeed, as such men usually do, and winning very probably the confidence of a court, but on which no man of true experience would have placed any reliance.

There are some things which a medical man encounters so often in practice, that in regard to them learning is next to a superfluity. There are some practitioners of so vast opportunities of personal observation, that with them direct experience might almost take the place of learning in most things. Were medical evidence concerned only with matters of this kind, or with men of this stamp, it would truly be of the highest order. But unhappily for such a consummation, rare incidents in medicine have a strange luck to turn up in medico-legal inquiries; and medico-legal incidents are most apt to arise in a station of life which naturally throws them often into the hands of not the most instructed members of our profession.

Hence in all probability it was that courts of law felt the necessity of submitting upon trials the primary medico-legal proceedings to the revision of witnesses of learning and skill, though not personally cognisant of the facts. This practice has prevailed systematically only from a rather recent date; and even still its usefulness, and in some instances its admissibility also, are occasionally denied.

All writers on the law of evidence agree in the general proposition, that "opinions are admissible in evidence, although the professional witnesses found them entirely on the facts, symptoms, and circumstances established in evidence by others, and without being personally acquainted with the facts" [*Starkie*, i. 154]. *Mr Chitty*, however, observes in 1834, that Justice Park doubted the admissibility of such evidence in a not uncommon case,—viz., when a medical witness is called on to say, without having seen an alleged lunatic, and merely from hearing the facts sworn to by others, whether he ought to be deemed insane [353]. And more recently this kind of evidence was pronounced inadmissible in such a case by the whole English judges on a remarkable occasion. In consultation on the case of Macnaughten, who shot Mr Drummond by mistake for Sir Robert Peel, they stated as their opinion, that "a medical man conversant with insanity, who never saw the prisoner previous to the trial, but who was present during the whole trial and examination of the witnesses, cannot in strictness be asked his opinion as to the state of the prisoner's mind at the time of commission of the alleged crime; or his opinion whether the prisoner was conscious, at the time of doing the act, that he was acting contrary to law; or whether he was labouring under any or what delusion at the time,—because each of these questions involves the determination of the truth of the facts deposed to, which it is for the jury to decide" [*Greenleaf*, ii. 303]. This statement is in

answer to a very special question, and relates to a class of cases in law proceedings connected with lunacy, as to which English judges have lately shown a strong propensity to appropriate medical opinion entirely to themselves and their juries. But it is quite plain, that if the rule of practice thus laid down be sound, and the reason assigned for it the true and only one, then the rule, with its reason, applies equally to all medical opinion whatever, except that very limited class which rests entirely on personal observation of the facts of a case; and therefore that this decision strikes at the very root of medical evidence as now understood.

In Scotland there has never been, so far as I am aware, any opinion delivered like this. In fact, I have known the English rule disregarded quite recently in a Scotch court in the very case supposed by the English judges. In criminal cases at large great deference is usually shown to the opinions of physicians and surgeons of known skill; who, though not directly cognisant of the facts, are cited on the part of the crown, to speak to the import of facts deposed to by the primary witnesses. And if those who occasionally appear on the side of the prisoner are not always so favourably considered, the fault has lain much more seldom with the court than with the witness,—who has too often attempted palpably to play the part of advocate, endeavouring to perplex and shake evidence, with the conclusions of which he nevertheless at bottom substantially concurred.

But on civil trials, such as those relative to lunacy, life-assurance, damage from injuries, death-bed deeds, and the like, a different rule seems to prevail. All medical witnesses, not fundamentally and directly concerned with the case, are looked upon with jealousy. The evidence even of the highest in professional standing is sometimes represented by judges as of no consideration whatever. And it is thought all in all by many lawyers, if the witness who states his opinion saw the case, however low may be his position in professional estimation. Not many years ago, namely in 1832, on a jury trial respecting the settlements of a gentleman who was alleged to have been upon death-bed at the time he disposed by will of his landed property,—when the parties on both sides produced various well-known physicians in this city to give their opinions on the facts of the case deposed to by the medical attendant of the deceased and other witnesses, the presiding judge observed incidentally, not without marks of impatience, "that it had always been his practice to tell the jury, that more attention is to be paid to what the medical gentleman says who attended the case, than to all the theoretical physicians in the country." And in his charge to the jury, he passed by the evidence of the latter as of no account, repeating in more measured language, that "he would place more confidence in the opinion of a medical man who saw the case, whoever he might be, than in that of any physician, who judges only from facts stated on questions put, perhaps im-

perfectly, by us in court, and not by persons well acquainted with the subject."

With great deference, this is a rather arbitrary way of administering a very arbitrary law. As a general rule, it appears to me an unreasonable dogma, founded on a hasty view of the nature of medical evidence. In one limited class of cases indeed it is a sound proposition. If a medical man, who personally witnessed the facts of a case some considerable time before, gives an opinion which he clearly recollects having formed on the occasion, but has forgotten in a great measure the fundamental facts—which is no very improbable supposition—no other man's opinion, how eminent soever he may be, is so worthy of credit, because he has no elements of safe judgment on which to found it. It is very remarkable, however, how few cases of this kind occur,—how seldom a medical practitioner's memory is found so far at fault in medico-legal proceedings. In another limited class of cases, a witness who did not personally observe the facts is scarcely more favourably circumstanced,—viz., when the opinion to be formed involves facts which cannot be fully communicated by language, such, for example, as the appearance and expression of a sick man. But if the facts are well-remembered, and they can be conveyed correctly in language, I really cannot see what should render the opinion of a man of experience and skill, who did not witness the case, less entitled to credit in a court of law, than in similar circumstances in ordinary medical practice. And should the primary medical witnesses be men of only moderate note in their profession—an incident of no unfrequent occurrence—I cannot see why their opinions should not be liable to revision by persons of superior skill, as in ordinary professional practice, merely because the opinion is to be followed by certain results in law, instead of in medical treatment. It appears to me, that an opinion in such circumstances is entitled to weight in proportion to the rank and acknowledged qualifications of him who delivers it; and as a general rule, to the same comparative weight with that of a consulting physician or surgeon in ordinary practice, who is consulted without an opportunity of seeing the patient. Nay, the opinion of such a man may fairly be held to be all the more weighty in comparison, that in medico-legal questions he has to form a judgment as to what has happened, or has been done; while in ordinary practice, he has also the more difficult task imposed of deciding what is to happen, and is to be done.

The real history of the very case which drew from the judge the opinion now under review would have been a startling reply to it. The medical attendant, whose evidence went to nullify the deed which was questioned, had stated in his precognition that his patient died of diabetes. Had this statement been adhered to on the trial, it would have been controverted by general testimony of such strength as no judge could have impartially resisted. But having previously met with one of the medical witnesses, who was to give an opinion

on the facts as deposed to in court, he discovered his error, and modified his evidence at the trial, so that the defenders of the validity of the will were completely thrown out in their intended line of defence. This evidence was declared by the judge to be unassailable; and so indeed it was. But the confirmatory evidence of him who had made it so was pronounced unworthy of notice.

Enough, perhaps, has now been said to show, that there has prevailed too long in this country a want of proper understanding between Law and Physic in respect to medical evidence; and that neither lawyers nor medical men have attempted all that might have been done to bring it towards the exactness of which it is susceptible. Were the bench and the bar to look more narrowly into the nature of medical evidence, they may discover that practical rules—resting on sound principles, established by long usage, and applied with singular success, for the development of evidence consisting of ordinary matters of fact—have been subsequently extended without due consideration to evidence composed of scientific reasoning and opinion, to which they are not applicable either in point of reason or convenience; and were medical men to turn their attention more earnestly to the bearings of medical science on law and its administration, they would escape many of the errors of opinion, as well as conduct, which have lowered their profession in the eyes of a class of men exercising more influence perhaps than any other on the sentiments of society; and they would facilitate the ends of justice by giving to medical evidence greater concordance, force, certainty, and comprehensiveness.

In conclusion, then, it may be asked,—What is to be done for the attainment of these objects? I have not the vanity to suppose that I can singly answer that question. Nor, though I had, is there time now for developing all the views which arise out of the preceding considerations. The following sketch is all that can be attained, without encroaching too far on the patience of this meeting.

And first, as to what lies within the province of the administrators of the law.

Let them bear in mind that medicine is a difficult art and science,—more difficult even than their own, as being less tied down to fixed rule and precedent; that medical opinion is not the simple matter which it might appear to be thought by them, judging from the present practice of law courts respecting it; that in medico-legal questions a medical man is no mere witness, but in some measure as much a judge as he who sits on the bench. Let him not, then, be placed as now in circumstances so unfavourable for sound judgment. Why should he not be allowed the same advantages for deliberate inquiry and reflection as judge or counsel when called on for an opinion in law? But instead of that,—possessing only an imperfect acquaintance with the facts, having often little or no

warning as to the opinions he may be asked, allowed no leisure to collect his thoughts and recall his experience, deprived of an opportunity of referring to the written authorities of his profession, destitute of support from consultation with his brethren, questioned by those whose aim it often is to lead him into contradiction,—he is expected, nevertheless, to answer trustily on all points at the moment. Had it been an express object to render medical evidence vague, uncertain, and contradictory, I do not know what more effectual plan than this could have been devised for the purpose.

The measures which seem best fitted for putting an end to such a state of things are the following, which have this to recommend them to impartial consideration, that they are not entirely new, having been already occasionally adopted in one department or another of law practice:—

1. In all criminal cases involving the inspection of a dead body, the medical men employed should be taught to proceed according to prescribed rules put into their hands by the legal authorities, so as to provide against material omissions, and to insure the detection of errors. In 1839, at the request of the Lord Advocate, the present Lord Murray, "Suggestions for the Medico-Legal Examination of Dead Bodies," were prepared by Dr Traill, Professor Syme, and myself, for this purpose. I fear, however, that his Lordship's laudable object has not been carried through, and that the document has been since lost sight of. There is an important technical obstacle against the public and authoritative sanction of these or any other instructions, as containing the only correct mode of procedure; but there is no objection to the mere recommendation of them by the authorities, and therefore it may be regretted that they should be allowed to fall into neglect. Additional force might be given to them advantageously, were they to be issued with the revisal and sanction of the Royal Colleges.

2. In all criminal cases, involving important medical evidence, the whole proceedings bearing directly or indirectly on that evidence should be subjected, before the trial, to the review of competent men, the higher in their profession the better; who may be made witnesses, if necessary. This would be a mere extension of a practice not uncommon until a few years ago, and still occasionally followed.

3. The medical witnesses should be furnished in good time with the whole facts of the precognition, as well as the main questions of opinion which they are expected to answer. This would be merely a return to a former rule.

4. The witnesses should have an opportunity of consultation together. It will be objected, as a consequence, that differences of opinion may in this way be concealed; and that the court ought to know what differences of opinion exist. But the present practice creates far more differences than it merely discovers. It seems to me

much on a par with a foolish practice of some patients, who, instead of obtaining a conjunct opinion on their case, prefer to wander from physician to physician, bewildering themselves with what they call "independent opinions." Lawyers would be surprised, could they learn the great unanimity of medical sentiment throughout the profession, after trial, on all important medico-legal questions, which for some years past have elicited so much contrariety of opinion in court. The inference is evident and irresistible that the witnesses had been ill-informed.

5. The witnesses should be present in court, so as to hear all the facts before delivering an opinion on them; but they should be excluded when the evidence as to opinion commences. This would be a simple restoration of a practice formerly established under the administration of the present Lord Justice-General when constantly at the head of the Criminal Court as Lord Justice-Clerk.

6. In civil actions, when it is thought right to add the opinions of consulted witnesses to the primary medical evidence, the court should appoint one or more such witnesses, for its own protection against partial testimony. The most crying evil of the present system of medical evidence,—that of which I have heard both bench and bar complain most bitterly,—is the contrariety of opinion obtained in civil actions. It is not necessary to look far for the root of this evil. What else can be expected, when the witnesses are informed of the facts by one party only, whose interest it is to communicate as little as possible of anything not favourable to his own side, and who, by frequent partial communings of this kind, succeeds in converting them into a sort of witness-counsel, with a strong, though it may be unconscious, bias in favour of early one-sided opinions? Why should not a portion of such testimony be placed in an independent and impartial position, by the court nominating men of known character to give their opinions as witnesses, after acquaintance with the medical facts on both sides? Something like this is occasionally done in questions of lunacy. There seems no reason why it should not be extended methodically to all important medico-legal questions of a civil nature. It would test the present contradictions of civil medical evidence, and in time supersede the present party medical evidence in a great measure, and convert medical witnesses into medical advisers.

These are the principal measures which, so far as I am acquainted with the subject, might be adopted without much difficulty by courts of law for the improvement of medical evidence. But the medical profession may also contribute largely their share towards its elevation.

If comparatively little has been said in the preceding remarks of the defects arising from the deficient knowledge, prejudices, and errors, of medical witnesses, it is not because these defects are not flagrant enough, or because I am not quite sensible of the discredit

they have brought upon medical science; but because I could not go more amply into the subject without frequent allusions, which might be applied, possibly erroneously, or, if with justice, nevertheless to individuals, who after all were perhaps not so much to blame as the negligence of our courts, which left the door too open for error and ignorance to enter. Unfortunately there can be no doubt that there is much room for reformation in the habits and even principles of medical men as medical witnesses.

1. In the first place, a more general and accurate acquaintance with the facts and principles of medical jurisprudence is desirable. There is no greater or more common mistake among medical men than to suppose, that an experienced practitioner is necessarily a skilful medical expert or a safe medical witness.

2. Secondly, in order to make him so, we require better medico-legal text-books. I am far from undervaluing such works as the Elements of Beck, or the Manual of Taylor; but it is no disparagement of them to say, that no single individual can treat profoundly of the medico-legal relations of surgery, obstetrics, toxicology, insanity, and the general practice of physic. Nor shall I be content with the position of medical jurisprudence in this country until three, at least, of these departments shall have been illustrated by an eminent surgeon, a scientific accoucheur, and some admitted authority in mental diseases; who shall each add to his own practical experience the study of the records of medico-legal literature, especially the fertile literature of Germany and France.

3. Thirdly, when a medical man is necessarily and unavoidably concerned, in the ordinary exercise of his profession, with what in legal phrase is called the *res gestæ* of a cause, he must of course make the best of his position by informing himself to the utmost on the subject of it.

But when asked in other circumstances to appear as a witness, his appearance as such being optional, there are some considerations he should weigh well before giving his consent. Among these the most important are, that he shall possess the requisite ability, and preserve his independence. On the former of these points nothing need be said in the present summary. On the latter, however, I may be permitted to conclude with a few words of caution, because there is no error more common among medical witnesses, and none more injurious, alike to the cause of medical jurisprudence and to the character of the medical profession, than the sacrifice of independence and impartiality to the views of the party by whom they are cited.

A few years ago I was consulted by a solicitor, in the interest of a person of good station in the north of England, who was accused, in suspicious circumstances, of murder by poison. And I was requested to give my opinion particularly on the validity of the chemical evidence, and if it appeared valid, to furnish "suggestions for shaking it." This incident will illustrate the object of a party's

agent, when he looks out for medical evidence. When a medical man is asked on the part of the Crown his opinion, either as an expert or in consultation, on a criminal case in Scotland, he is expected by the Crown officers to maintain rigorous impartiality and independence; nor in general is there any risk of incidental temptation to the contrary. But when he is asked to be a witness for the prisoner in a criminal case, or for either party in a civil action, he may rest assured, that although the agent may not own it, even perhaps to himself, his expectation or wish is in most instances much the same with that of the English solicitor. There are few, it is hoped, who would not recoil from the broad request to "shake" evidence in which they themselves believed. But on the other hand, every man is not proof against the compliment conveyed in the more ordinary form of approach. There is, however, one simple rule, which will invariably preserve him from sacrificing his independence: viz., never to embark as medical witness in a cause, when he has his option, unless satisfied that the purpose of his evidence is compatible with truth and justice. If he consent to appear for the mere purpose of contradicting the medical details of a case, while he cannot deny the main inference to which they conjunctly lead,—for the purpose of embarrassing and shaking medical evidence, in the general conclusions of which he may be forced to concur,—he perverts medical science and forsakes the province of witness. I remember a witness of this sort, who, after doing his utmost to undermine the chemical evidence in a charge of poisoning, by assailing this, that, and another petty fact as unworthy of reliance, was compelled, on being asked his opinion on the whole chemical evidence, to admit that it was irrefragable. And on another occasion, after the witness had been made to assail for nearly an hour the evidence of poisoning from particular symptoms and individual morbid appearances, the flimsy web, thus tediously spun, was torn to pieces in a moment, when he was asked the simple question, whether, from the whole medical facts he had heard in evidence, he entertained after all any doubt, as a medical man, that death had been caused in the way charged in the indictment. He had none, and could never have had any. These gentlemen were not witnesses, but counsel; and very sorry counsel too. Their conduct escaped the censure of the court; but not that of their own profession. Let us hope that their example will serve as a beacon, to be avoided by their successors.

4. I wish I could add here the suggestions of Dr Gordon Smith and Mr Amos for the conduct of medical witnesses on trials. But I must be content with referring for this information to their writings; because I have already detained the College and its guests too long. For this I beg to apologise, pleading that the Council and I together selected rather too wide a subject for a single lecture. I have also more particularly to beg our friends of the legal profession, who have honoured us this evening with their presence,

to excuse me if I have used too great license in criticising professional rules and practices, with whose bearings I may be inadequately acquainted. I cannot help feeling their injurious influence on medical evidence; and I cannot help expressing on this occasion what I believe to be the sentiments of my profession generally, as much as my own, upon the subject. If I have gone too far,—if I have meddled too freely with an institution, whose principle is stability, and its ruling passion resistance to change, I must plead as my apology, that I am open to the prejudices of a restless profession, whose history is constant innovation and continual discovery, and its pride that it cannot for a moment stand still, as the world advances in science and civilization.

ARTICLE II.—*Contributions to Legal Medicine, being Notes of some Trials which took place at Glasgow during the Autumn Circuit of* 1851; *with cursory Remarks.* By J. A. EASTON, M.D., Medical Officer to the Glasgow Police, one of the Physicians to the Glasgow Royal Infirmary, &c.

IN the following article, I propose to lay before the readers of the Monthly Journal, brief notes of, and cursory comments on, some of the more important medico-legal cases in which I was engaged as a medical witness at the Autumn Circuit, which has just finished its sittings at Glasgow. The general calendar was unusually heavy, there having been no less than 101 cases tried altogether, of which eleven, if not twelve, required medical testimony for their elucidation and support. Of these I was engaged in nine, and had the pleasure and benefit of having, as occasional coadjutors, my friends Drs Robert Macgregor, John Crawford, Andrew Anderson, Mr Joseph Fleming, and Mr Wm. Scott. These nine cases embraced one of murder, three of child-murder, one of culpable homicide, one of rape, two contraventions of what is called Lord Ellenborough's act, the species of assault in both having been committed by stabbing; and lastly, one case where a son inflicted seven wounds with a poker on the head of his mother, whereby the skull was laid bare in five different places.

Out of this dark and appalling catalogue I select the following:—

CHARGE OF CULPABLE HOMICIDE.—*Marks of Violence on Head; Softening of Right Lobe of Cerebellum, and of a portion of Middle Right Lobe of Cerebrum; Apoplectic Extravasation in both of these Situations.*

James Jack was charged with the crime of culpable homicide, in so far as, on the 6th September, while in his house in Tontine Close, Trongate Street, Glasgow, he struck his wife with his fists about the head and face, and otherwise abused her, in consequence

of which, or of apoplexy, or some other diseases occasioned thereby, she died on the same day.

The evidence, which was very short, brought out the following facts:—On the evening of deceased's death, she was observed by several of her neighbours going about in apparently her usual state of health, and quite sober. While prisoner and his wife were in their own house, words were heard between them, and a noise was also heard as of some person falling; shortly after which prisoner came out of his house, and said to some of the neighbours he had never seen his wife so drunk as she was that night. He was accompanied into his house by some of the people on the stair, and the deceased was found lying in bed nearly dead, but still breathing. There was an appearance as of a scuffle having taken place. The police were sent for, and the prisoner taken into custody. There was no direct evidence as to the assault, the most important part of the case being the medical testimony, which was embodied in the following report, in which (I may here remark once for all), as also in those that follow, it will be observed that I have endeavoured, as much as possible, to avoid the use of technicalities, in accordance with the advice I have already ventured in this Journal[1] to tender to the junior members of the profession.

"On the left side of the throat, parallel to the commencement of the windpipe, there was a deep, recently-inflicted abrasion, which had all the characters of such a mark as would be caused by the forcible application of the nail of a full grown man or woman. There was a recent bruise also on the ring finger of the right hand. These were the only injuries that existed on the *surface* of the body.

"But on dissecting the scalp from the skull, we found, on the under side of the former, in that part of it which covers the right temple, a dark ecchymosis, considerably larger than a shilling; and the corresponding portion of the right temporal muscle, immediately subjacent, was bruised, pulpy, and loaded with coagulated blood to the extent of at least a half-crown piece. About two inches higher up than this injury, and towards the left side, the pericranium presented a deeply ecchymosed appearance, oblong in shape, and an inch and a quarter long; while towards the left side of the upper part of the occiput, there was a third ecchymosis, similar in form and size to that just described, but more superficial and much less severe. These injuries could only have been caused by violence applied externally, such as blows, kicks, or falls.

"In examining the brain, we found that the main posterior artery of that organ was in a diseased condition, and that—as the consequence of this—the right lobe of the lesser brain, and a portion of the right lateral ventricle of the larger, were in the morbid state technically called *ramollissement* or softening. Into those diseased

[1] See vol. ix., p. 590.

portions there was an extensive apoplectic extravasation of coagulated blood. This diseased state of the arterial trunk, which is to be regarded as the primary cause of the disorganisation of the cerebral tissue, must have been in existence for some time before death; and in consequence of such a deviation from sound structure, the circulation of blood in the brain would be irregularly performed, and a peculiar susceptibility to apoplexy engendered.

"In endeavouring to give an opinion as to the circumstances under which the death took place in this case, we think that the following conclusions may be submitted, as having been established by the medical examination:—

"I. There can be no doubt that severe external violence had been applied to the head.

"II. There is equally little doubt that there existed disease of a portion of the brain, rendering the patient peculiarly susceptible of apoplexy.

"III. It is equally certain that this woman died of apoplexy; but

"IV. The application of external violence must have increased this apoplectic tendency, or rather, to express ourselves more correctly, must have rendered the supervention of apoplexy inevitable."

The deceased was above 60 years of age; and the basilar artery presented that yellow atheromatous degeneration to which this vessel appears to be peculiarly liable. The *ramollissement* had not proceeded to an extreme degree, the affected portions of the brain having merely lost their cohesion, without having become discontinuous or diffluent. Though it was proved, by the testimony of several witnesses, that this woman was of sober and industrious habits; yet, as it was surmised that she had partaken of a small quantity of whisky on the evening of her death, the prisoner's counsel, fairly enough—on the *dolus an virtus* system—attempted to establish, that the fatal apoplexy had been occasioned by the stimulus of ardent spirits; and in answer to a question put by him, bearing on this point, which was afterwards more fully referred to by the presiding judge (Lord Colonsay), I stated, that in a person in whom the brain was diseased, as in this woman, *any* stimulus, mental or physical, would be likely to superinduce apoplexy, qualifying the opinion, however, with the additional observation, that while whisky was capable of producing such a result, so, *a fortiori*, was violence applied to the head. In cases of this description, barristers—or counsel, as they are called in Scotland—are extremely anxious to elicit from the medical witness a general acquiescence in some general abstract fact in medicine, without the slightest reference to the specialities of the case under consideration. In these circumstances, it is in my opinion clearly the duty of the medical witness, while assenting to the abstract proposition, the truth of which may not be gainsayed, to call attention at the same time to any peculiarities that exist in the individual case under trial, and to explain

how far these peculiarities may modify, or even alter, the opinion which, but for their existence, he might otherwise have expressed. It may be perfectly legitimate, in a dexterous lawyer, to construct *his* hypothesis on conjectural causes and bare possibilities, but the medical witness is bound, by the duty which he owes to society, as well as by respect for his own reputation, to present the facts of each case precisely as they stand, in all their extent and bearings; and to found his opinion, not so much on abstract, isolated doctrine, as on the individual circumstances of the case under investigation.

The jury unanimously found the prisoner guilty, and he was sentenced to imprisonment for two years.

CHARGE OF MURDER.—*Abrasions on Face; Numerous Contusions on Legs and Feet; Lacerated Wound in Vulva; Tuberculous Deposit with Small Vomicæ in the Lungs.*

Peter Brown was charged with the crime of murder, in so far as, on the 10th of July, while in the dwelling-house in Great Hamilton Street, occupied by him, he wickedly and feloniously attacked the now deceased Margaret Wilson or Brown, his wife, and with his fists, or some other instrument (to the prosecutor unknown), struck her on the head or face, and repeatedly kicked her on the feet and legs, and private parts, or lower part of the belly, and thereby inflicted a severe wound on the lower part of the belly, to the great effusion of blood, by all of which she was mortally injured, and died thereof on the day following.

The evidence led in support of this charge disclosed the melancholy but too prevalent fact, that the prisoner and his wife had been of intemperate habits, and had led consequently a very unhappy life. On the day specified in the indictment, the son of these unhappy parents, who was the principal witness on his father's trial for his mother's murder, came home shortly before eight in the evening, and finding the front door locked, he thereafter forced the back-door, and discovered his mother lying behind it on the floor, bleeding, faint, and scarcely able to speak. No other person was in the house but his father, who was in bed. Blood was found on the floor where the deceased was lying, and the under part of her clothes was saturated with that fluid. The following is the medical report:—

"On the middle of the right cheek there was a deep, recently-inflicted abrasion, of a semicircular form, and fully an inch in length; and a much longer abrasion, but more superficial and evidently not so recent, existed over the left side of the lower jaw.

"On the front and sides of each leg and foot there were at least a dozen livid marks of recently-inflicted bruises. These blue marks varied in shape and extent, some of them being oblong, and fully three inches in length, while others were circular, and ranged in

size from a shilling to a fourpenny-piece. The number and situation of these bruises precluded the supposition that they had been the result of falls, or any species of accident, and left no doubt that they had been caused by repeated blows or kicks.

"But in the private parts was found an injury, of a far more serious kind than any of those which were noticed elsewhere. Immediately above the orifice of the urethra, or urinary passage, there was a ragged wound, of an irregularly triangular form, and sufficiently large to admit the point of the finger. The delicate structure surrounding this laceration, which penetrated upwards and forwards to the pubis or share bone, was deeply contused. In consequence of the extreme vascularity of this part of the body, such a tear could not fail to have been attended with profuse loss of blood.

"The brain and the contents of the belly were healthy. The upper part of each lung exhibited the morbid alteration called tuberculous deposit, which is well known to be the cause of the disease popularly known as consumption. But though this deviation from natural structure existed, as well as one or two very small cavities, and though it is extremely probable that this disease would have had a fatal termination at some future and indefinite period, yet we are decidedly of opinion, that the state of the lungs at the time of death was in no way connected with the fatal result.

"Under these circumstances, therefore, we have no doubt that the death of this woman was occasioned by the loss of blood, unavoidably consequent on the tear in the private parts."

From the above report, it will be observed, that the face, legs, and feet of the unfortunate deceased furnished too ample proof that she had been the subject of a severe, protracted, and violent assault. This circumstance is thus specially adverted to, because it was maintained for the defence that the laceration in the vulva, and the consequent hemorrhage, of which it was admitted that the woman died, *might* have been caused by her having fallen on the corner of the handle of a tub; and the prisoner's counsel, proceeding on the theory of abstract possibilities already referred to, asked the medical witnesses if the fatal injury *could not* have been so produced, and they at once stated that such an occurrence was *not impossible.* Care was taken, however, in giving this answer, to explain to the court and the jury that, on the assumption that the laceration had been caused in this way, the particular part that was torn must have come in contact with an angular projection on the tub, and not on a smooth even surface. Unfortunately, however, for the accused, it was proved that the tub, *on which he himself affirmed in his declaration* that his wife had fallen, had no such projecting point; and, though another utensil of the same kind, having such a projection was found in the house, yet the fact of the prisoner having spoken only of what I may call the smooth tub, neutralised the ingenuity which sought to connect the cause

of death with the tub having the angular projection. In his very luminous and impartial charge, Lord Colonsay completely demolished the abstract possibility theory, and set before the jury, in perspicuous and convincing terms, whether, with a medical opinion explicitly given that a kick was the most likely means of inflicting the mortal injury, and with the evidence before them of violence having been inflicted by repeated kicks on *other* parts of the person, they could entertain any reasonable doubt that the injury on the private parts had not been inflicted at the same time, and in precisely the same manner,—urging them, however, if they felt such a doubt, to give the benefit of it to the accused. The jury returned a verdict of culpable homicide; and the prisoner was sentenced to transportation for life.

In reference to lacerations of the vulva, I may remark, in concluding this case, that my situation as medical officer to the Glasgow Police has enabled me to see seven cases of this description of injury, and singularly enough, in connection with the foregoing case, two of these, which were fatal, were occasioned by an accidental fall on the projecting part of a tub. But then the laceration in these cases was on the external, lateral, and posterior parts of the genital organs,—chiefly on the labia majora,—and not, as in this case, in the centre, and high up between the clitoris and the orifice of the urethra—a situation comparatively so internal, so central, and so high up, as to be almost beyond the reach of accident.

CHARGE OF CHILD-MURDER.—*Death by Suffocation (?) Marks of Violence, as if by forcible compression of Mouth and Throat.*

Elizabeth Morrison was charged with the crime of child-murder, in so far as, on the 18th or 19th of May last, within a house in M'Alpine Street, Glasgow, she did compress the throat or neck, or press upon the mouth or lips, of her child, in consequence of which the child was suffocated or mortally injured, and died on the said 19th of May.

As this case broke down before the period when the medical witnesses are usually called, and was consequently given up by the public prosecutor, I shall merely, before submitting the medical report, give a short outline of such facts as were brought out by those witnesses who were examined in court, premising generally, that evidence of a very disgusting character came out, which showed that the moral status of some of the witnesses was of the lowest degree, and that their testimony was not entitled to implicit credence. This was the point on which the case broke down. The accused, it appeared, was unmarried, while the father of her illegitimate child was a married man. She had denied her situation during the whole period of her pregnancy, and was delivered alone, though another female, ignorant of what was going on, was in an adjoining room. The child survived twenty-five hours

after birth; no injuries were observed on it till about twelve hours after it was born, about which time the injuries on the mouth, described in the following report, were first perceived,—those on the throat some time after,—and the child subsequently lay gasping for breath, and taking what were described as "nervous turns." No medical aid was solicited. The medical report was as follows :—

"From the length, weight, and general condition of the body, it was evident that this child had undergone the full period of utero-gestation, while the dry and shrivelled state of the still adhering navel-string indicated that death had taken place in a day or two after birth.

"On the left side of the upper lip there was a deep and recently-inflicted abrasion, which extended fully half an inch in the transverse direction, and the eighth part of an inch in the longitudinal. The cuticle, or scarf-skin, had been completely detached in this situation, the wound felt hard when touched, and exhibited the dry, dark, leathery appearance which is produced by forcible compression. The anterior portion of this lip, throughout its whole extent, was of a livid colour. A small and very superficial abrasion existed on the lower lip, exactly opposite the injury on the upper.

"On the left side of the throat, near the projecting part of the windpipe, there was a deep abrasion, nearly half an inch in length. This wound was peculiar,—it was in the form of an indentation, which had deeply divided the skin, was nearly vertical in direction, convex in shape, with the concavity toward the right side, and precisely such an injury, in all respects, as would be produced by the forcible and persistent application of the nail of a full-grown man or woman. Immediately beneath this wound, there was another pretty deep abrasion, somewhat circular in form, and rather larger than a split pea. On the right side of the upper part of the windpipe the skin was red and ruffled, and this appearance, indeed, was presented, more or less, on the greater part of the front of the throat.

"These injuries on the mouth and throat must have been inflicted during life, and were exactly such as would be caused by the grasping of an adult hand.

"The vessels of the brain were very much congested; the lungs were of a deep violet colour, and distended with bloody serum; the right cavities of the heart contained fluid blood.

"The contents of the belly were healthy.

"As there was no trace in this case of any of the diseases which are induced by natural causes, and as the lungs, heart, and brain, presented the alterations which result from suffocation, and connecting these alterations with the injuries on the mouth and throat, we have no hesitation in certifying, that this child died in consequence of the vital air having been excluded from its lungs by artificial means applied externally."

As the prisoner was acquitted by a jury of her country of the crime charged against her, I consider it unfair to make any comments on this case, and shall therefore leave the reader to draw his own conclusions as to the manner and circumstances in which this child died.

CHARGE OF CHILD-MURDER, OR OF CONCEALMENT OF PREGNANCY.—*Marks of Violence on back part of Head; Extravasation of Blood on Brain; Signs of Suffocation (?)*

Elizabeth Cowan or Coan was charged with the crime of child-murder, in so far as she having, on the 3d day of May 1851, within or near the house in or near Paterson Street, of Kingston, in or near Glasgow, then and now or lately occupied by David Garrioch, accountant, then and now or lately residing there, in whose service she then was, given birth to a living female child, she did then and there wickedly and feloniously, with her fist or hand, or with some instrument to the prosecutor unknown, strike the said child a blow or blows on or about the head, or did knock its head against some article of furniture, or the floor, or some other hard substance, and did place it between two matresses in a bed in said house, or did, in some other manner to the prosecutor unknown, mortally injure the said child; in consequence of all which, or part thereof, the said child immediately, or soon thereafter died, and was thus murdered by her, the said Elizabeth Cowan or Coan: and the prisoner was further charged with the alternative crime of concealment of pregnancy. As the evidence in support of the former of these charges was believed, I understand, to be deficient, the plea of being guilty of the minor offence was accepted, and the prisoner sentenced to nine months' imprisonment. Of course, no evidence either medical or otherwise, was led in these circumstances; but the following was the medical report which was prepared, and which I give *ad longum*:—

"*5th May* 1851.

"We hereby certify, that we this day inspected and carefully examined the body of a female child in one of the apartments of the Police Office, Maitland Street, Glasgow.

"This child had evidently been recently born, for the navel string and after-birth were perfectly fresh, and both adhering, the usual separation by the tying of the former having been omitted. The body was free from the slightest putrefaction, was well-formed, well-conditioned, weighed rather more than seven pounds, and was nearly twenty inches in length. The nose was slightly flattened, traces of blood were observed at the anterior nostrils and on each cheek, the lips were blue, and the tongue protruded beyond the gums. Around the anus there was a large quantity of the feculent matter of new-born infants—technically known as meconium—with which substance the feet likewise were smeared.

"The chest was well arched, and emitted a clear sound when percussed. The lungs were of a dark colour, and very full of blood; they were fully expanded, and when compressed in several points between the finger and thumb, the crepitating sound indicative of the presence of air in the air-vesicles was distinctly audible. The whole contents of the chest,—that is, both lungs and a portion of the windpipe, the heart and its bag, and the roots of its large vessels, as well as the structure peculiar to fœtal life, called the thymus gland, having been removed from the body, were thrown into water, in which they immediately floated considerably above the surface of that fluid. The lungs were thereafter cut into about twenty separate pieces; and though these were subjected to pretty strong pressure with the view of expelling if possible the contained air, every one of them, when placed in water, rested on the surface.

"Both cavities of the heart, but particularly the right, contained a considerable amount of fluid blood.

"The contents of the belly were healthy.

"On removing the scalp, it was found that the under side of that portion of it which covered the back of the head was in a deeply ecchymosed or bruised condition; and on the upper and back part of each hemisphere of the brain there was a copious extravasation of coagulated blood.

"Having carefully and maturely considered the whole of the circumstances furnished by the medical examination of this case, we think ourselves warranted in drawing the following conclusions:—

"I. That this child had undergone the full period of utero-gestation. This is established beyond the possibility of doubt, by the plump condition of the body, but more especially by its length and weight.

"II. That it had fully and perfectly breathed. This is evident from the arched appearance of the chest, the largeness of the lungs, their spongy texture, the existence of crepitation when they were slightly squeezed, their readily floating in water, even with the superadded weight of the heart and the thymus gland, and their continuing to do so when cut into many pieces, and subjected to strong pressure.

"III. That it had suffered a violent death. This, we think, is established both negatively and positively;—negatively, by the absence of all trace of any of the usual diseases which terminate existence, the body, on the contrary, being well-formed and well-conditioned;—positively, by the marks of violence on the scalp, and the extravasation of blood on the brain, as also by the gorged condition and dark colour of the lungs, and the amount of fluid blood in the heart. The former of these unnatural conditions would lead to the supposition that the death had been caused by violence applied directly to the head; the latter, that asphyxia had been induced by suffocation, a conclusion supported to some extent by

the appearance of the nose, by the bloody cheeks, the livid lips, and the protruding tongue. But having expressed in general terms our conviction, that the death was a violent one, we must refrain, in the meantime, from giving an opinion as to the precise kind of violence which had been resorted to,—whether such had been an injury to the head, as a blow or a fall, or compression of the mouth and nose, or both of these causes combined.

"All this we attest to be true on soul and conscience, &c."

It has already been stated that the prisoner pleaded guilty to the charge of concealment of pregnancy, and therefore I shall not advert specially to the other more serious charge of child-murder, except in so far as the details connected with that charge bear on the general subject of infanticide, with a few general observations on which I shall now conclude this article. It scarcely requires to be stated, that every charge of this nature presupposes the previous living of the child which is said to have been murdered; and this appears to be such an obvious truism, and the fact of previous living existence seems so susceptible of demonstration, that most people imagine, that on such a question any attempt to make plainer what surely is plain enough already, must be unnecessary, because superfluous. Yet the questions, what constitutes live-birth, and how can live-birth be substantiated, are possibly the most disputed of the numberless disputed questions in medical jurisprudence. Thus, let us suppose that it has been proved that a particular child has been heard to cry while still in utero, a second while still in vagina,—occurrences the existence of which rests on undoubted authority,—or, that loud and vigorous sounds have been emitted from a third child, whose head only protrudes from the person of its mother—a circumstance which no one practising midwifery will regard as a very unusual event—surely every one of these three children must be regarded as having breathed if it cried; and if it breathed, the uninitiated would say, surely it has lived. But the law inculcates a very different doctrine, and tells the unsophisticated practitioner of medicine that though the protruding head had been mortally injured while the rest of the body was attached to the mother, no murder had been committed because live birth had not been effected. It matters not though the lungs contain air and float in water, the legal definition of live-birth remains unaltered by this demonstration of the existence of air in the lungs. That this is a correct interpretation of the law of England[1] on this sub-

[1] I have used the words law of England advisedly, for I am not aware whether the law of Scotland adopts the same or a different view. In order to ascertain this point, I have made a cursory examination of the celebrated work of Baron Hume, and of the justly popular treatise of Mr Alison on the Criminal Law of Scotland. In neither, however, have I been able to observe any statement bearing directly on the question under consideration. No doubt at page 73 of the 1st vol. of the latter work, the learned author, while insisting on the fact, that no considerations arising from age affect the charge of murder, states,

ject might be established by many examples. I shall only adduce one or two, which are taken from the first edition of that invaluable work, Taylor's Medical Jurisprudence,—"In a case tried a few years since, in which a child had been found with a ligature firmly tied around its neck, the medical evidence showed clearly, that it had breathed; and the whole of the appearances in its body were such as to leave no medical doubt that it had died by strangulation. The judge, in charging the jury, said, 'if they were of opinion that the prisoner had strangled her child before it was wholly born, she must be acquitted of the murder.' The prisoner was acquitted."[1] Again, at the Norfolk spring circuit of 1837 "Mr Justice Coltman held, that to justify a conviction for child murder, the jury must be satisfied that the entire body of the child was actually in the world in a living state when the violence was offered to it."[2] Lastly, in 1841, Mr Baron Parke thus charged the grand jury at the Herts Lent assizes,—"With respect to all these cases (of infanticide), there is a degree of doubt whether the infant has been *born* alive. The law requires that this should be *clearly proved*, and that the whole body of the child should have come from the body of the parent. If it should appear that death was caused *during delivery*, then you will not find a true bill!"[3] In order, then, to constitute live-birth in the eye of the law, the child must be proved to have been fully born, to have been separated from its mother, and to have maintained an independent existence, while no violence inflicted on it before the consummation of that event—even though such violence should render subsequent living impossible—can be regarded as murder. This, then, being the accepted legal view of what is meant by a child having been born alive, the next point for consideration is, does medical science, or physical science generally, supply any test whereby the legal definition may be established in all its comprehensiveness and exactitude? Most unhesitatingly I answer, they do not, simply because they cannot. In vain do we point to the hydrostatic test as proving that these rigid requirements have been fulfilled. Under two restrictions, and two only,[4] the hydrostatic test can demonstrate that a child has breathed,

---

illustratively, that the crime may be committed on "the infant which has breathed for one minute," &c.; but it is obvious, that this expression does not meet the question under review, because, as already adverted to, *mere breathing* may take place under several circumstances,—and yet live-birth, as defined by the law of England, may not happen after all. Those remarks, however, are made with great diffidence, as my reading of law of any kind has been very limited; and as nothing is more likely than that some express precept on this point, which I had either not seen or not understood, may exist in the very works I have named.

[1] P. 471. [2] Op. cit., p. 472. [3] Op. cit., p. 472.

[4] These are—decomposition, causing evolution of gas in the lungs as in other parts of the body; and, secondly, artificial inflation of these organs. The former will of course be obvious to the senses, and therefore can be no source of fallacy; the latter is so unlikely to be practised in cases of this description, as scarcely to

but *where* that breathing took place,—whether in the womb or in the vagina, whether by the body wholly detached from the parent, or but partially protruded,—on these points it reveals nothing; and in objecting, as many do, to this test simply because it does not satisfy them on these matters, they are merely displaying their own ignorance in expecting a kind of information which the hydrostatic test is incapable of supplying. The hydrostatic test, be it remembered, is only significant of a general law in physical science, and is the exponent neither of the accidents of parturition nor of the atrocities of the murderer; it merely proclaims, in short, the existence of air in the lungs, but is silent regarding the source of that air, and equally silent regarding the place where, and the manner in which, it was introduced.

In the foregoing medical report in the case of Coan, it will be observed that the second general conclusion refers to the fact, that the child "had fully and freely breathed"—not, had been born alive—and the remarks which I have just made, rather fully, I fear, for the patience of the reader, have been prompted by the persuasion, that considerable misconception prevails among many members of the profession regarding the duties expected of them, and the exact amount of assistance they are able to afford, in the elucidation of cases of infanticide. Hence it is, that, in conducting the examination of infants supposed to have been murdered, many medical men look to the hydrostatic test as affording decisive proof of living-birth, and that in preparing their reports they express an opinion, founded on that supposed proof, that the child had been born alive. Now, I maintain that by that test they are able to establish nothing beyond the mere fact of breathing—whether perfect or imperfect is not the present question—and that in using the current phraseology to which I have just referred, they are certifying what they are unable to substantiate, and are exposing themselves, consequently, at least to ridicule, perhaps to censure.

There are many other points connected with the medical jurisprudence of infanticide on which I had intended to enlarge, had space and time permitted; but the consideration of these must be reserved for another opportunity.

---

be admitted even as "an abstract possibility." I make this last remark in the full knowledge that Orfila has suggested that the lightness of the lungs, which causes them to float in water, may have been occasioned by a newly-delivered woman putting forth une œuvre de la tendresse maternelle to re-animate her still-born child by blowing into its lungs! And also, that he has gravely urged that such a result may have been effected by some malignant wishing to criminate an innocent person, towards whom he or she was malevolently disposed! —On insuffle de l'air dans les poumons d'un enfant mort-né pour faire croire qu'il a vécu, et qu'il a été tué par sa mère!—"They manage things better in France;"—and it is perfectly evident, that the French demon must excel all other demons in being profoundly versed in the subtleties of legal medicine.

ARTICLE III. — *Tubercular Tumour of Cerebellum.* By J. P. BRAMWELL, M.D., Perth.

I WAS called about eighteen months ago to visit Janet Alexander, a sickly strumous-looking girl of ten years of age. I was informed by her parents that some four months before she had suffered from discharging sores in her neck, arm, and leg, that these had dried up suddenly, and that after this she began to complain, now and then, of pain in her head, to relieve which she would whirl round rapidly while standing in the room. Her stomach had also become very irritable of late. Some days before I was called to see her the pain in the head became excruciating, so much so as to cause her to utter at intervals piercing cries. Her vision also was becoming impaired, the pupil of both eyes was slightly dilated.

Treatment.—To have the head shaved, and freely rubbed with the Ungt. Tart. Antimon. morning and evening; to be purged freely with calomel and scammony.

As soon as the ointment began to take effect, and a copious discharge was thrown out from the irritated scalp, a decided diminution of the pain in the head took place, and this continued until the headach was almost gone; but the vision became more impaired, and the pupils were dilated enormously, and insensible to light. She now began to complain of diminished power on the left side. The intellect was quite clear, and the sense of hearing acute.

I directed the irritation of the scalp to be renewed from time to time, and prescribed the Ol. Jecoris Aselli in teaspoonful doses twice a-day—to be increased in quantity as soon as possible.

August 15, 1850.—Up to this period she has kept almost entirely free from headach; she sleeps and eats well, and has become quite plump, but the vision is not improved. For some time past she has shewn a peculiar uncertain gait in her attempts to walk, and though able to move freely and steadily any of her limbs individually, she does so collectively with great difficulty and without co-ordination or harmony.

To have Hydrarg. c. Creta with Pulv. Doveri three times a-day. This treatment she continued to follow for some three weeks, but with no benefit whatever.

July, 1851.—I am informed that up to this period her general health has been remarkably good, and that she has never shown any tendency to epilepsy; she has for some time past, however, again begun to suffer from the old pain in her head, and has had an issue in the neck with little benefit. When talking with her parents, she will suddenly stop, scream, and cry for her head to be

firmly pressed—and soon after will resume her story as before. She begins also to suffer, in a certain degree, from dysphagia.

Her symptoms became more aggravated, and she died on the 18th of this month, remaining sensible to the last.

Sectio Cadaveris forty-eight hours after death.—The skull-cap was very thin, membranes non-adherent and but slightly congested, brain large in proportion to the age of the subject, and somewhat softer in texture than natural. The ventricles were considerably distended, and about ʒvi. of serum escaped when an opening was made into these cavities. The optic thalami were attenuated, and softened. The cerebellum was remarkably softened, so much so, that after the tentorium was removed, it required much care and attention to lift it out of its situation for examination. Toward the crus it was quite pultaceous, but posteriorly somewhat more consistent. A dense tumour was felt imbedded deeply in its centre, and stretching across from lobe to lobe; by washing away the cerebellar substance, this tumour was found to be about the size of two pigeons' eggs, connected by a broad commissure, and measured about three inches in length; it was elastic to the touch, surrounded with a fibrous capsule, and botryoidal on its surface. When cut into, it presented two distinct substances—the containing substance or cyst semi-cartilaginous, semi-gelatinous, and its contents yellow in colour, homogeneous in appearance, and of about the consistency of putty; the contents could be easily removed from the capsule by the knife, leaving its walls entire.

Remarks. — This case is certainly one highly favourable to Flourens' theory of the function of the cerebellum,—viz., that this organ is commissural, and the co-ordinator and harmoniser of muscular action. Assuming this to be the case, we would be led to expect that all lesions of the cerebellum, whether traumatic or otherwise, would, to a greater or lesser extent, interfere with co-ordinate muscular motion.

When Flourens removed a portion of the cerebellum in pigeons, the result was feebleness and want of harmony in muscular movement; and Longet found that when one-half of the cerebellum was removed from birds, they next day exhibited the unsteady and uncertain gait of drunkenness; again, in Andral's Clinique Médicale, tome 7th, there are several cases recorded where a drunkenlike gait existed in connection with a diseased state of the cerebellum.

In this poor girl's case, there was the most marked want of power of muscular adjustment; she could not stand in one position for any length of time, and when she attempted to walk, her gait was unsteady, she tottered from side to side, and if not laid hold of would speedily have fallen to the ground. From all this it is pretty clear, that a very important connection exists between a due regulation of muscular movement and a healthy condition of the cerebellum.

It is difficult to say at what time this cerebellar tumour began to

grow. It is certain that no head symptoms showed themselves before the sores in the neck, arm, and leg had ceased to discharge, and it is not at all improbable that its origin may be dated from that period. There can be little doubt that the nature of the disease was essentially strumous, for the contents of the capsule exhibited, under the microscope, all the characters of true tubercle.

Did the tumour in the cerebellum and the concurrent hydrocephalus both derive their origin directly from the cachectic state of the system? or was the effusion into the ventricles the result of the cerebral irritation produced by the tumour in question? Symptoms of hydrocephalus began to show themselves some four months after giddiness and pain of the head were complained of, and from the sudden loss of sight and continued blindness, there is reason to believe that effusion, to a greater or lesser extent, existed from the date of the first acute symptoms up to the period of her death. The pressure of the fluid upon the optic thalami and their attenuated and softened condition, account satisfactorily for the loss of vision.

PERTH, August 15, 1851.

ARTICLE IV.—*On the Treatment of Aneurism.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

LET it be supposed that the Dublin surgeons should at length accomplish what they so ardently desire, and succeed in establishing pressure as the universal remedy for aneurism of the extremities; and let it be further supposed, that through lapse of time the operations of Hunter, Desault, and other surgeons of the present as well as past century, who have perfected the application of ligatures for obstructing arterial trunks, should be buried in the thick obscurity of oblivion. Patients labouring under the disease would then have no means of escape from suffering and impending danger except the "CLAMPS," with their tedious confinement, averaging in duration a period amply sufficient for crossing the Atlantic, visiting the chief wonders of the western world, and returning, with their prolonged agony of compression, submitted to only through the authority of military discipline, or some equally powerful influence, and tolerated only through the frequent administration of opiates, in addition to careful regulation of the screws, and with all those risks of failure, or worse consequences, in the way of mortification, which, if prevented by ignorance of his situation from disturbing the patient, must at all events deprive the attendants of tranquillity, and too frequently reward their anxious solicitude by disappointment or the loss of limb, if not of life. Such a state of things being, if possible, supposed, let the reader, if he can, imagine with what surprise, delight, and gratitude the following relation, from

the hospital book, would be received, more especially if the operator were able to say, that upon this occasion he had tied the femoral artery for the *twentieth* time, without any bad effects from the operation.

"Donald Monro, æt. 26, a tall muscular man, by occupation a provision dealer in Glasgow, was admitted on the 23d of September 1851. At the lower part of the left thigh, on the inner side, is a pulsating swelling, the size of a large orange. It is of an oval shape, its long axis being in the direction of the limb. Inferiorly, it encroaches slightly upon the upper part of the popliteal space. There is a general dilatation of the whole tumour with each pulsation of the femoral artery. By arresting the circulation at the groin, and at the same time making pressure upon the tumour, it is considerably diminished in size. He states that he always enjoyed good health until six weeks ago. At that time he felt slight numbness and a pricking pain in the left leg and foot, which have continued with more or less severity ever since. Three weeks ago he found a small swelling on the inner side of the thigh, and since this it has been daily increasing in size. During the last fortnight he has been entirely incapacitated from following his ordinary occupation, being unable to put his foot to the ground, or to extend the knee-joint, without severe pain in the tumour, and in the limb beneath. He is unable to assign any cause whatever for its origin; and he cannot recollect having had any blow or other injury of the limb.

"*September* 29*th*.—Since his admission, although absolute rest and a low diet have been rigorously maintained, the size of the tumour has very considerably increased, chiefly in an upward direction. The patient complains of sleepless nights and great uneasiness and pain in the limb. His expression is anxious, and he is earnestly desirous that the operation should be performed without delay. The next day, *September* 30*th*, the man being under the influence of chloroform, Mr Syme placed a ligature round the femoral artery about four inches below the groin. No difficulty was encountered, and upon the patient awaking from his sleep he expressed himself as comfortable and perfectly free from pain. He has never suffered in any way since. He has slept soundly at night, and in the day time has amused himself with reading, &c. His expression is happy and contented, and his general appearance greatly improved. On the eighth day the wound was dressed for the first time since the operation, and was found to have healed in great part by the first intention, there not being more than a few drops of pus under the dressings. On the fourteenth day the ligature separated. The tumour is now fast disappearing, not being more than half its previous size."—E. R. B., *Resident Clerk*.

The reader is probably aware that for persevering in the perpetration of such cruel and unjustifiable acts, as the one just related,

I have incurred the bitter reprobation of my brethren in Dublin, who indeed have not conceived any epithet of scorn, contempt, or indignation too severe for expressing their opinion of my conduct in doing so.

"To attempt to argue with Mr Syme upon surgical subjects is a fruitless and unprofitable task; it were far more dignified to allow one, whose mind seems deplorably dark alike to logic and philosophy, to remain in tranquil possession of his own sentiments, no matter how erroneous these may be, than to waste time in any endeavour to dispel them by proof or reasoning."—*Dublin Quarterly Journal of Medical Science*, August 1851,—p. 137.

To such philosophers it would be obviously useless for me to say a single word upon the subject. Indeed I think it prudent to retire from the field in good time, lest they should resort to the "*argumentum baculinum*," for which figure-of-speech Irish logicians are alleged to entertain a partiality, and which, judging from the language applied to me, there seems no want of disposition to employ in my own case.

I will, therefore, for the future, confine my efforts to this side of St George's Channel. It appears that the great advocate of pressure, Dr Bellingham, has been trying to make converts in London by personal exposition, and apparently with some success, which is not surprising, if the preference of Mr Skey and Sir B. Brodie for "*blunt knives*" be generally adopted in that city. For my own part, with all due dread of the pain and danger of compression, I should certainly not hesitate in submitting to the torture of the clamps rather than encounter such deadly weapons as silver or copper knives. But when a keen edge of sharp steel, a well constructed needle, and a single thread of small silk ligature applied without disturbance of the neighbouring textures, are the means employed, I suspect it will be found difficult to make modern surgery retrace its steps so far as to afford entire satisfaction in Dublin.

ARTICLE V.—*Injury of the Head.* Communicated, through Professor SYME, by Dr JOHN TRAILL, Arbroath.

March 5, 1851.—LEWIS EVANS, labourer, æt. 40, a stout muscular man of dissipated habits, admitted for wound of head. This forenoon, whilst employed on board a ship, a heavy bucket, filled with coals, and said to weigh four hundred weight, in consequence of a chain breaking, fell from a considerable height, the sharp iron-bound edge striking him on the head. He was at first insensible, but in a few minutes recovered so as to be able to walk to the Infirmary, with but little assistance.

There is a lacerated wound two and a half inches long, commencing at the upper part of the left parietal protuberance, and extending down towards the ear,—the bone is freely exposed, and

can be felt fractured and depressed, and, at the lower angle of the wound, to fully the thickness of the skull. The depressed portion is firmly fixed, and cannot be raised, nor indeed moved. There are no symptoms of compression, and none of the external senses are impaired. The wound has bled moderately, and he suffers very little pain. Pulse 74, and of good strength. Pupils quite natural.

Notwithstanding the extensive injury and depression of bone, yet in the absence of symptoms indicating impeded function of the brain, we considered it improper to have recourse to the trephine. Cold was applied to the head; the antiphlogistic regimen strictly enforced; and three hours after, re-action having come on, he was bled to thirty-two ounces, and a purgative given.

Next day his pulse had got up to 95, sharp and strong, but he suffered almost no pain, and looked well; he was again bled to twenty ounces.

From this time until 18th March he continued to improve steadily, being free from all pain; his tongue clean; pulse from 70 to 80; sleeping well, and his intellect unaffected; the wound suppurating freely, and sending up healthy granulations.

March 18.—He complains of a dull uneasy feeling in the head, but not amounting to pain. Tongue a little furred; pulse 70, rather weak.

March 19.—Looks much depressed; speaks with slowness and hesitation; his mind seems to be wandering, and in the morning he was observed to be slightly delirious; has had no shivering; skin cool and moist; face pale; vision seems perfect in both eyes; pulse 72, rather hard; tongue slightly furred; wound suppurating freely, and some of the fractured portions of bone can be felt denuded and quite dead. Complains of dull pain in the left side of head, and of want of feeling in the *right* arm. After consultation, it was resolved to trephine. One crown was first removed and then an angular portion with Hey's saw. Several large pieces of bone, and a number of smaller fragments, all in a dead state, and some of which projected internally in sharp points, were raised and removed. Dura mater feels tense and projecting, but without fluctuation. Operation borne well; a good deal of blood lost; says he feels more sensation in right arm. Next day the improvement was very evident. Tongue clean; pulse 72, natural; does not now in speaking show any wandering, but seems to hesitate a little from being unable to find words to express his meaning.

For the next four days he went on as well as possible, but on March 23d he was attacked with erysipelas of the head and neck, accompanied with low typhoid fever, which required the exhibition of claret for several days, and left him in a very weak state.

From this time he gradually recovered, but occasionally suffered from giddiness and temporary loss of memory, with hesitation in speaking. Blistering to the nucha was repeatedly had recourse to, and he was slightly mercurialised. About the middle of April he

was so well as to be able to work a little in the garden, and he regained considerable flesh and muscular power.

May 10.—His appetite having been very good for some time past, he had become plethoric, and showed evident symptoms of congestion in the head, with torpor and loss of memory; he was relieved by free venesection, purgatives, and counter-irritation, with restricted diet. From this time he continued almost free from complaint, but he never regained his strength, and occasionally had fits of nervous tremor, without marks of determination to head, but followed by giddiness, loss of memory, and tendency to stupor.

July 22.—Last night was attacked with severe giddiness, followed by stupor, and died suddenly this morning.

The body was examined forty-eight hours after death, and having been unable to be present, I am indebted for the following note of appearances to Drs Bruce and Wannan, who conducted the examination:—

Body muscular and in good condition; countenance pale but placid; pupils natural. Before dividing the scalp, it was found that the wound was completely and firmly cicatrised. On removing the calvarium, the opening in the skull was found to be covered by a membrane, which was firmly adherent to the dura mater, and also to the arachnoid and pia mater. To the latter was attached a tumour of the size of a small orange, imbedded in the left hemisphere of the brain. "At the point where it was attached to the internal surface of the coverings of the brain (a space about the size of a fourpenny piece), the several membranes were closely adherent to each other. The tumour was of firm consistence, but yielded to pressure, giving the sensation of fluctuation. An opening was made into it, and from three to four ounces of thick, healthy-looking pus were evacuated, the tumour at the same time completely collapsing. The walls of the sac of this abscess, as it proved to be, were of equal density and consistence throughout. They were about three lines in thickness, and consisted of a firm, tough texture, which, when examined microscopically, was found to be fibrous structure in an early stage of its formation. The brain was found to be very much softened immediately around the tumour, and to a greater or less degree throughout the whole of the left hemisphere. On cutting into its substance, it was found paler and freer from bloody points than the substance of the right hemisphere."[1]

The membranes generally were rather vascular. No other part of the brain was observed to be abnormal. There was great effusion of serum at the base of the brain, and in the upper part of the spinal canal.

---

[1] The tumour was sent to Edinburgh for examination. Dr Traill is consequently not responsible for the description contained between the inverted commas, which was furnished by Mr Bickersteth.—ED.

Part Second.

REVIEWS.

Principles of Physiology, General and Comparative. By WILLIAM B. CARPENTER, M.D., F.R.S., &c. Third Edition. London, 1851. Pp. 1098.

THIS is a truly admirable digest of general and comparative physiology, embracing a systematic account of the structure and functions of the individuals which compose the vegetable and animal worlds. After an introductory chapter on the nature and objects of the science of physiology (remarkably well written), we are successively presented with others "On the general characters of organised structures;" "On the nature and conditions of vital phenomena;" "On the component structure of organised fabrics;" and "On the distinctive characteristics of the vegetable and animal kingdoms." Chapters VI. and VII., embracing upwards of 300 pages, contain one of the best general accounts of the vegetable and animal kingdoms ever published. Here also the student will find a condensed and readable account of Professor Owen's vertebral theory. In the second part of the work we have a special description of the different functions in distinct chapters, each divided under the heads of,—1st, General considerations; 2d, As they are performed in vegetables; and, 3d, As performed in animals. The work concludes with an account of sensible motions in living beings; of the functions of the nervous system and of the production of sounds by animals.

The labour of compiling and writing this work must have been enormous; and we not only congratulate the author on its successful accomplishment, but the professional public and the student, on the possession of a book which will enable them to connect their anatomical and physiological knowledge with the whole range of the natural sciences. Considering the multitude of subjects referred to, and the numerous doctrines involved, it is perhaps superfluous for us to say that, if disposed, we could strongly object to many statements and opinions to which the author has given his sanction. But where there is so much general excellence, and where the tone of the whole is so harmonious, it would be indeed hypercritical to parade and discuss individual errors, however important in theory these may be. On one point only shall we make a few observations, because we consider a principle of no small importance to be involved.

An idea which the character of his work leads the author to impress on his reader, is embodied in a quotation from Herschel. "Science is the knowledge of many, orderly and methodically digested and arranged, so as to become attainable by one." It is by this standard we are told in the preface that the author desires his work to be tried, rather than by the amount of original matter it may contain. Judged then in the manner proposed, we are of opinion that it may be considered as having arrived at the highest degree of excellence of which such compositions are susceptible. But in justice to those whom we consider to be the real prosecutors of science, we cannot place such publications high on the list of scientific works. The more so, as the success of excellent compositions such as that before us has led to the publication of a class of works, and to the formation of opinions, which we conceive to be injurious to the interests of science and of education. The London press abounds with Manuals professing to give digests of the various branches of medical science, and the student is indirectly led to suppose that these furnish him with all that is necessary in the study of his profession. In consequence, the original authors in medicine are neglected. Those writers who in time past have done so much to advance knowledge, and whose graphic accounts and accurate observations are so well calculated to impress the youthful mind, are no longer read. It cannot be denied that the most able original memoirs and monographs, are at present drugs in the market. The most laborious researches to determine new facts, careful dissections, experiments and observations, even combined with the highest powers of generalisation, are considered by a large section of the professional public (of which the "British and Foreign Medical Review" may be considered the organ) to be of secondary importance. True, they are thought necessary, but then they must be properly digested, by another; they must be clothed in an agreeable dress, and presented in felicitous language; the *luxe* of fine paper, clear type, and numerous woodcuts, borrowed from all sources, must be added, and thus a successful, because a marketable, work is established. During this process, however, the merits of the real man of science are liable to be overlooked. He at all events is kept very much in the back ground; and the superficial reader is induced to shun the trouble of making out who are the individuals to whose labours he is truly indebted for the information the book affords him. This evil would be in some measure obviated if scrupulous care were taken to cite the name of every authority whose investigations or doctrines are deemed worthy of notice. Dr Carpenter states in his preface, that if he had followed this practice the bulk of the volume would have been doubled. Surely he cannot expect us to receive this statement literally, for we are persuaded that ample acknowledgment of the labours of scientific men in foot notes, would not have extended his book above half-a-dozen pages. Even the

illustrations are not directly referred to their authors; for instead of finding their names with the explanation below each woodcut, we are under the necessity of turning first to the "list of illustrations," where these explanations are unnecessarily repeated, and then to a corresponding number, in another list of the authors quoted. Very few, we apprehend, will take the trouble to study in this way 321 figures; and among the uninitiated, the merit of the real observer is apt to be passed over and transferred to the author of the book before him.

One or two good books like that of Dr Carpenter may be tolerated, but then they must be used and not abused. When recent, they will communicate to the student an excellent summary of what has been lately published; but they should never induce him to neglect the original memoirs and essays, in which facts and theories are first recorded. He should remember that the former class of works are in their nature ephemeral, whilst the latter, containing well-described facts and observations, may be always consulted with advantage. If John Hunter had lived in this age of digests, manuals, and retrospects, we doubt very much whether his "Treatise on Inflammation" would have been read. Happily, it might have been conjoined with some compilation of the day, and so made the subject of an article in the "British and Foreign Medical Review," and we should have been told that Hunter was a good observer, but that the other work was the more elaborate and scientific. We maintain that this modern practice of making compilation superior to original investigation, and of keeping the workman in the background, while his labours are appropriated, does not belong to the highest department of literature, and is not science.

We have written the above with a view of pointing out the error which has of late years been too much fostered by a certain class of writers, of placing manuals and other similar productions in a false position. There is too much of the book trade, and too little acknowledgment of those labours on which the production of good books, and the prosecution of science, alike depend. With every disposition to acknowledge the merits of Dr Carpenter's work, and cordially recommending it as we do to our readers, we cannot hold it out to the young physiologist as worthy of imitation. To the latter we say, at the commencement of a new session, that this beautiful and bulky volume, weighed in the balance of true science, is as a feather when poised with the shortest essay containing a new truth. Moreover, power of generalising *correctly* is more closely conjoined with habits of original investigation than some suppose; and we could illustrate this proposition by pointing to many published opinions of the author. At all events, every effort should be made to connect this power and those habits closely together, if we wish to keep steadily in view the aim of every scientific man. This we hold to be the cultivation of that earnest spirit which leads to the advancement, rather than to the diffusion, of knowledge.

La Pellagra investigata sopra quasi duecento cadaveri di Pellagrosi allo scopo d'interpretarne la condizione patologica, l'indole e la natura. Memoria con nove tavole dimostrative. Del D. Pietro Labus. Milano, 1847. 8vo. pp. 50.

Pellagra, as investigated by nearly two hundred dissections, with a view to determine its pathological condition, disposition, and nature. A Memoir, with nine descriptive plates. By Pietro Labus, M.D. Milan, 1847.

The work whose title is placed at the head of this article, is the last contribution which has appeared towards the elucidation of that singular affection which, during many years, has prevailed in certain parts of Northern Italy, and which is known by the name of Pellagra. Before making mention of the several interesting facts which the investigations of Dr Labus have led him to observe, we shall endeavour, in as concise a manner as possible, to present to our readers what may be considered as the sum of our existing knowledge on Pellagra, alike as regards its etiology and symptomatology. Frapolli[1] and Gherardini,[2] both of whom were physicians to the Grand Hospital of Milan, and Zanetti,[3] whose sphere of observation was at Cannobio, on the Lago Maggiore, were the earliest writers on the disease, their publications having appeared in 1771, 1778, and 1780 respectively. Dr Gaetano Strambio[4] was the next author, having at intervals, from 1784 to 1787, published his three essays, and in 1794 his two dissertations.[5] If to these we add the work of Luigi Soler,[6] published in 1791, and the more recent one of Giovanni Strambio,[7] son of the earlier observer of the same name, together with the papers of Dr Holland of London,[8] Rizzi Mosi of Milan,[9] and Briere de Boismont of Paris,[10] we shall probably have included all the most striking and suggestive publications on the subject of pellagra; at all events, we think that, by a reference to these, we shall be able to illustrate what was known of the disease up to the appearance of Dr Labus's essay; and having reached that point, we shall consult our author. Pellagra, then, at present prevails, and has done so for a consider-

---

[1] Animadversiones in Morbum vulgo Pellagram Mediolani, 1771.
[2] Nova Acta Physico-Medica, Tom VI., Norimbergae 1778. Observatio XXIV.
[3] Della Pellagra Descrizione. Milano, 1780.
[4] De Pellagra Observationes.
[5] Dissertazioni sulla Pellagra.
[6] Osservazioni Medico-pratiche sulla Pellagra. Venezia, 1791.
[7] Cagioni, natura e sede della Pellagra. Milano, 1824.
[8] Medico-Chirurgical Transactions, Vol. VIII.
[9] Annali universali di Medicina. Agosto, 1844.
[10] De la Pellagra. Paris, 1834.

able period, probably, according to the accurate inquiries of Gaetano Strambio, for upwards of a century, in the provinces of Lombardy to the south of the Alps, and between them and the Po. This district has long been, and continues to be, celebrated for its extreme salubrity, and for the richness of its soil; while the characters which chiefly strike the traveller passing through it, are the extent of the plains which support the cultivation of a rich pasturage, of rice, and maize, besides the vine; and the plentiful supply of water which, conducted by artificial channels resembling small canals, irrigates the country, thus becoming a chief cause of the luxuriant vegetation. To the north of the district of country so described, that is, in the direction of the Lago Maggiore and Lago di Como, the country becomes hilly and mountainous. Not less striking than the general character of the country, is the aspect of the poor agricultural population who inhabit it. A badness in quality, and deficiency in amount of their food, and inadequacyin their clothing, and in the accommodation of their homes, they have long been known to endure, while no district has suffered more from intestine commotions produced by political changes, or by the incursions of war. Dr Holland, in his most interesting paper, makes reference to the observed fact, of the increase of Pellagra during the fifty years preceding the publication of his observations, and believes it not improbable that the causes just specified may have had no little influence in its augmentation. It is sufficiently remarkable, in connection with this point, that, since the memorable year 1848, Pellagra has undergone a remarkable increase, while the fact of misery and wretchedness having, during the same period, and in the same beautiful country, been vastly augmented, is too well known to be insisted upon here. It is impossible to accept, as at all probable, the theory that the origin of the disease is to be attributed to the climate, for, as Dr Holland observes, that has remained unaltered for a period long preceding the history of Pellagra; and secondly, had a change in it occurred, and been instrumental in occasioning the disease, it would have affected alike the higher and the lower classes, many of the former being constantly resident in the very district where the Pellagra is most prevalent. These objections the same author applies to the argument by which mere locality is supposed to be influential in its production. Many other theories as to the probable origin of the disease have been advanced, and with no small ingenuity maintained; it may not be uninteresting to mention a few of these. Frapolli believed that the sun's rays were alone influential in its production. While Odoardi contended that Pellagra arose from want of salt, Albera and Guerreschi conceived that the great secret of its origin lay in the abuse of that article. Fanzago and Marzari attributed its invasion to the use of spoiled corn. Triberti to that of imperfectly baked bread. Alloni and Belloti believed Pellagra could alone arise from a miasm, while Calari and Fontana acknowledged contagion as its

only source. Some have attributed its occurrence to the long-continued use of maize as the only or almost only article of diet, while others, and among these Balardini, have asserted that Pellagra was nothing more than a poison, induced by the action on the body of verdegris formed during the culinary operations on the maize. Many other theories have been brought forward, several of which, like some of those now mentioned, admit of an easy refutation, but others, if not exactly correct, contain such an amount of truth as to merit a diligent investigation, though we cannot here afford to enter upon their discussion. We glean, however, from a review of all, that the causes already specified, namely, an insufficient and badly constituted diet, with imperfect clothing and house accommodation, are mainly instrumental in occasioning the disease, by inducing that cachectic state of body from which all the Pellagrosi suffer, and which the poor inhabitants of the district of Lombardy alluded to, in a greater or less degree, all present. We now turn for a short space to the symptomatology of the affection. The name "Pellagra" is derived from one of the earliest and most prominent symptoms of the disease (pellis agria), the dark eruption which makes its appearance on those parts of the body most exposed to the sun's influence; the back of the wrists and hands, the upper part of chest over the sternum, and the feet, being chiefly affected, less frequently the forehead, and other parts of the face. When this eruption first makes its appearance, which it does in the early spring, it is of an obscure dusky red colour, somewhat resembling an erysipelatous blush; after a few days vesicles form (sometimes small tubercles appear), these continue for a few days, during which time the hue of the skin is changing from a red to a deep brown, and the skin itself is becoming dry and scaly; after this the contents of the vesicles coagulate, they themselves disappear, and are succeeded by a partial desquamation, seldom leaving, after the first occurrence of the eruption, any marked appearance, save perhaps a glistening, or not altogether healthy cuticle. Individuals who have been attacked for the first time in spring by this eruption, preceded, as it invariably is, by a failure of health, as evidenced in loss of appetite, languor, debility, &c., and sometimes by an already cachectic appearance, generally recover their health as summer advances, and before the commencement of autumn present slight, if any, traces of the disease. This remission of the general and local symptoms lasts during the autumn and winter, and it is only when spring again approaches, and the heat of the mid-day sun becomes excessive, that the affection of the skin returns, preceded by the same train of general symptoms, only more severe, and consequently showing an increased derangement of the constitution. It is at this stage that the nervous system exhibits a decided and threatening alteration, as manifested by vertigo, confusion of ideas, cramps, &c. A few at this period become maniacal. The bowels are generally loose, the dejections un-

healthy, the appetite depraved. Before a second winter, improvement has begun, and by its commencement the remission is established; not, however, so distinctly as after the first, the deep furrows and rhagades attesting the altered and now callous condition of the skin; and the debility and cachectic appearance of the sufferer, showing how severely the attack of the second spring has tried his constitution. At this stage many pellagrous patients die, the more common causes of death being the deposition of tubercles in the lungs or abdomen, and the occurrence of dysentery. More generally, they enter upon a third winter, sometimes with, but more frequently without, an approach to the remission of the previous year. The debility attending this advanced stage is excessive, the breath is extremely fœtid, all the functions are vitiated, the senses impaired, dropsy often comes on, and on the advance of spring, the unfortunate sufferers, if not carried off by some intercurrent affection, most likely diarrhœa or dysentery, suffer an attack of mania more or less acute, recovery from which leaves them only incurable idiots. It is indeed a most melancholy spectacle to witness the victims of the advanced Pellagra. In the wards allotted to maniacal patients in the Grand Hospital of Milan, which we lately visited, out of upwards of a hundred patients, more than sixty were pellagrosi. Most of these had entered on a state of helpless dementia; others were plunged in abject melancholia, after an attack of acute mania; while several were suffering from the latter, in its most active form. Dr Holland has feelingly remarked, that the Pellagrosi offered a melancholy representation of physical and moral suffering, such as he rarely had occasion to witness.

Little opportunity has occurred for medical treatment in the earlier stages of the disease, and indeed experience has shewn that it is then not much required—an improvement invariably following a judicious change in diet, along with better clothing,—and non-exposure to those agencies which, although not to be regarded in the light of exciting causes, still manifestly increase the sufferings of the victims of the disease. It is only in the later stages of Pellagra that patients enter the hospital, and opportunities for observation and treatment are afforded. The measures already alluded to, have in numerous instances proved beneficial; but the return of the patients to the locality where the disease prevails has in too many defeated this success, and been followed by relapses. The whole subject of the disease, and more especially the question as to the alleviation of the sufferings of its numerous victims, and the means which might be adopted for its arrestment, appears to us as deserving of the most attentive consideration. True it is that measures have already, from time to time, been adopted for this end, but the continued advance of Pellagra only proves their insufficiency.

We proceed now to the very agreeable duty of making our readers acquainted with the work and labours of Dr Labus.

The author has divided the volume before us into three parts. In the first he details the morbid appearances which have been found in the bodies of the Pellagrosi, by antecedent as well as contemporaneous authors. This chapter does not claim much attention; for, as Dr Labus remarks, insufficiency of pathological knowledge, and erroneous deductions, distinguish previous inquiries into the morbid anatomy of the disease. The present learned physician of Milan, Dr Giovanni Strambio, seems to have been the earliest writer who carefully performed dissections, and recorded the morbid appearances; and his example appears to have had no little influence in stimulating our author to the performance of the same duty. In the second part, Dr Labus treats of the pathological changes he has discovered in his own investigations. And in the third, he attempts, by a reference to these investigations, to answer certain interesting questions in regard to the nature of Pellagra. In an appendix he has recorded four very interesting cases, and these in so careful a manner, and obviously in so truthful a spirit, as to assure us of his fitness for the task he has assigned to himself. Referring now to the second chapter, we find that Dr Labus divides Pellagra into two divisions. The one, *Pellagra vera*, true pellagra—not a very happy expression, we think—the peculiarity of which is, that *post-mortem* examination reveals no disease of any organ, save the sufficiently remarkable one, an atrophy or attenuation of the ileum (*assottigliamento dell'ileo*), which he has found in all such cases, and to which we shall presently direct attention. In the other division of Pellagra, either some marked lesion of internal organs is found, sufficient to have caused, or at least expedited, the fatal event, or death was known to have taken place from some other disease, the patient being at the same time affected with Pellagra. Out of 100 cases recorded by Dr Labus, 57 belonged to the latter class, and 43 to the former. The affections which existed in the 57 being:—Hepatization of the lungs in 17; anæmia in 10; tubercles in lung in 9; extravasations of blood in 5; the results of pleurisy in 2; hypertrophy of the heart in 2; cirrhosis of liver in 3; enteritis in 2; typhus with petechiæ in 2; fungoid tumour in 1; pericarditis and its consequences in 1; gangrene of lung in 1; encephaloid disease in 1; capillary apoplexy in 1; tetanus in 1; total 57. The atrophy of the ileum previously alluded to is, Dr Labus believes, and in this he is borne out by numerous dissections, the distinguishing pathological character of uncomplicated Pellagra. The only other portion of the intestinal canal which the author has found affected by this atrophy is the jejunum, and it in its two lower thirds alone. In the ileum he has found it in every part of its length. When occupying its lower portion, it always ceased three or four fingers' breadth above the cœcum. This peculiar appearance the author found to affect long tracts of the ileum. Sometimes a portion of healthy intestine intervened between two parts so altered. Atrophy of the ileum alone was found in sixty-

seven, twenty-eight of which belonged to the forty-three previously mentioned, as having died of uncomplicated Pellagra. The morbid appearance was limited to the jejunum in four, two of which were of the number of the forty-three. Both ileum and jejunum were affected in thirteen, seven of which were cases of *Pellagra vera.* Sometimes, but not often, the morbid portion of intestine was quite circumscribed. Certain other peculiarities were met with, chiefly as respects the form of the morbid portion of intestine, but such cases were rare. Other pathological characters of sufficient importance marked these altered portions. Such were always pale, of the normal resistance, but with complete absence (*mancanza*) of the *valvulae conniventes;* the mucous membrane was smooth, and when placed between the eye and the light, was seen to be studded with numerous little points of uniform elevation, and all presenting a rough appearance. Some among the observers of the morbid appearances of the Pellagrosi, have stated that the mucous membrane is eroded; such, however, Dr Labus believes not to be the case. He has never seen it deficient. Having shown how this affection of the intestines differs from the atrophy which, under other circumstances, is occasionally met with, the author proceeds to inquire, in what this morbid appearance consists. If the mucous coat be raised by the aid of very delicate instruments, and with great care, it will be found to be a little thicker than usual, but the fibrous layer immediately below it will be seen to present an extremely attenuated and flaccid appearance. The muscular coat, also, is similarly affected, but the peritoneal is unchanged. Dr Labus practised fifteen times injections of the vessels of the intestines; in the sound portions he experienced no difficulty, but in the atrophied portions he found it impossible from the constant escape of the injected fluid. Not content with careful examination of the diseased portions of intestine by the naked eye, Dr Labus has farther employed the microscope to aid him in his researches, and he has found that when the mucus from the affected portion of the intestine is examined by a high power, numerous spheroidal and opaque corpuscles are invariably to be detected. This portion of Dr Labus' inquiry is less complete and exact than we could wish, but he is working in the right direction, and ere long, we doubt not, his microscopic observations will be found worthy of confidence.

It thus appears that, as the result of numerous and carefully-conducted dissections, Dr Labus has arrived at the highly important pathological fact, that in this disease the jejunum and ileum are affected by atrophy—a morbid change of unquestionably great importance. We have already exceeded the bounds originally contemplated, and cannot now enter with Dr Labus on the consideration of any of the interesting questions which occur in his third chapter. We subscribe to his notion of the peculiar appearance of the skin

in Pellagra being no essential part of the disease, but merely a species of erysipelas *(una risipola propriamente detta solare)*, induced in a cachectic frame of body by exposure to the influence of a powerful sun.

We have the less regret in taking leave of Dr Labus at this time, as we know he still continues to avail himself of all his opportunities of investigating the pathology of pellagra, and that shortly we may expect more important contributions from his pen. We would urge upon him the necessity and advantage of making his labours chemico-pathological.

The Prevention and Cure of many Chronic Diseases by Movements. An Exposition of the Principles and Practice by these Movements for the Correction of the Tendencies to Disease in Infancy, Childhood, and Youth, and for the Cure of many Morbid Affections of Adults. By M. ROTH, M.D. London. 1851.

THIS is an octavo of 300 pages, designed to enlighten Great Britain upon the subject of the treatment of disease by movements, *alias* Kinesipathy, which system of practice originated, as our readers have already been informed (Monthly Journal, 1851, p. 258), with a Swedish fencing-master, of the name of Ling, and which, under the fostering care of his successors, has now, it seems, become a science. The said Ling, as we gather from a biography in the present volume, seems to have been a universal genius, having been successively a graduate in theology, a volunteer in the Danish navy, a fencing-master (in spite of gout in his arm), a lecturer on old Norse poetry, history, and mythology, a professor of fencing and gymnastics, a student of anatomy, physiology, and other natural sciences, and a writer of poetry. Let it be added, that he was "a man of high moral tone, pious, sincere, honest in all his dealings with his fellow-men. His intellectual powers were of a very high order," and he died with the honours of knighthood upon him. The fact seems to be, that Ling was a clever fellow, who, having devoted himself to teaching gymnastics, and being of opinion that what was worth doing at all was worth doing well, set himself to his work after a more methodical fashion than his predecessors, threw into his practice a spice of what he fancied to be physiological science, which made it take a little better, and succeeded in leading his countrymen to look upon him as a prodigy of genius. His whole merit seems to be, that, from the smattering of anatomy which he had picked up, he saw that, in order to exercise the muscles of the body properly, it was necessary to vary the motions more than had been the practice of gymnasts. This is evident from a chapter on the movements for the healthy body,

given by Dr Roth (p 122, *et seq.*), which is extracted from Ling's "Reglement," and which is exceedingly well illustrated by woodcuts. Bating a little absurdity in some of these movements, we should say that this chapter, or, still better probably, Ling's original book itself, might, when detached from Dr Roth's nonsense in other matters, be of some use to teachers of gymnastics and calisthenics. If Ling really did methodise and improve the practice of gymnastics, as there seems reason to believe he did, the world is indebted to him; and had Dr Roth written a book to instruct fencing-masters and drill-sergeants in the way of giving to these most useful athletic exercises an increased value as means of promoting vigour and health, the world would have been indebted to him too. But, unfortunately, the aim of the successors of Ling has been, not to develop what was really useful in his method, but to found upon it a system of quackery, to which there is no evidence that he ever intended it to lead; and, in pursuance of this, they now come forward with a series of statements, unblushingly propounded, as if they were ascertained facts, but for which there does not exist a shadow of foundation, except in their own audacity and confidence in the gullibility of mankind. We shall content ourselves with a few extracts from the present sample of this so-called science, in support of this assertion of ours, and these will, we believe, be quite sufficient to show our readers what sort of information is to be got from Dr Roth's octavo.

It is necessary, however, for the right appreciation of the following passages, that it should be known what is meant by the word movements, as used by Dr Roth:—

"By the word *movement*, in a medical and hygienic sense, is to be understood every change of position and difference of form, determined by time and amount, in the whole body, or in any part of it, and which may be produced by the organism itself, or by any animate or inanimate mechanical agent."—P. 38.

This very comprehensive definition will easily prepare our readers for learning that there is an infinite variety of movements, and we find, from Dr Roth's subsequent pages, that their name is Legion, and that many of these—such, for instance, as drumming with a couple of fingers on any part of the body—which, in our ignorance, we have supposed to be not worth noticing,—are in reality physiological agents of great power. Here is the way in which Dr Roth explains how this comes to be:—

"Whatever exists in our body, either as a part of it or as a foreign substance, must at a certain moment have a definite shape; therefore every change of the space in one part necessarily produces a corresponding one in the surrounding tissues—a change that is thence propagated to the most remote parts of the body, and which depends, with respect to its form, upon the amount of the alteration produced by the first movement."—P. 31.

And this sort of arrant nonsense is gravely laid down by an M.D., as the basis on which to build a system of medical practice.

As a sample of how individual movements are described by Dr Roth, we give the following strikingly novel and highly curious remarks :—

"THE RISING-UP MOVEMENT.—Rising up (the act of getting up) is a heaving movement effected by the muscles of the trunk and thighs, by which the body is raised from the lying, sitting, kneeling, or in general from an angular position, to a more erect or less angular one. The body itself, with or without any other burden, is the load which is to be raised. If any other burden is to be lifted from the floor, then heaving is always combined with rising up.

"*Effect.*—Rising up is a movement acting on the respective muscles, but by the change of the angles of the trunk, there occurs also a diametral change in the direction from before backwards, and consequently an effect upon the intestinal organs.

"THE LETTING-DOWN MOVEMENT.—Letting down (the act of lowering) is the entirely opposite movement to the act of getting up."—P. 79.

This is only one of a host of similar puerilities, which make up the greater bulk of this volume.

The following extract shows that the "movements" aspire to being of use in acute as well as chronic diseases, although the title of this book would not lead us to expect so much :—

"GONORRHŒA.—Dr Liljewalk, physician of the Garrison Hospital at Stockholm, employed the movements in this disease, having seen their good effects under Professor Branting's prescription, and says: 'The difficulty of employing all the originally prescribed movements induced me to use only the percussion on the sacrum, which I considered the most essential. I employed this movement in twenty-three cases, and must particularly observe that I chose those patients in whom the inflammation was of the greatest intensity, and I only forbade them violent exercise and too much walking. The result was—

"'1. It did not fail in any of the twenty-three cases to diminish in a few days the violence of the inflammation, and with more success than I had ever before seen.

"'2. In fifteen cases the disease, without any medicine, was perfectly cured in the average of nineteen days, while the medical treatment lasts here, in similar cases, about twenty-three days.

"'3. In eight cases the inflammation disappeared, but the secretion from the urethra continued. I suppose that this incomplete result was owing to the over-simplification of the treatment; and probably the vibration of the perineum (which for special reasons could not be employed) would have taken the place of the remedies which I then made use of.'

"The prescription in gonorrhœa is generally (besides some movements depending upon particular circumstances) the following :—

"1. Percussion on the sacrum, in the stride-standing position.

"2. Transversal chopping on the neck, in the sitting position.

"3. Pressure above the os pubis, in the lying position, with elevated back, while the separated and bent legs are drawn towards the abdomen.

"Vibration of the perineum, in the same position.

"The treatment begins with percussing the sacrum in the stride-standing position, which in the first day or two not only allays and relieves the more violent inflammation and copious secretion, but also changes the whole state of the disease in such a manner, that the following treatment by movements (different according to the state of the patient) produces an increased flow of arterial blood in the upper extremities, and the cure is very much accelerated. In the first stage, during which only a moderate stitching pain, tension, and little secretion appear, the percussion on the sacrum is alone sufficient, if re-

peated three or four times daily. If the symptoms become more violent, and accompanied by chordee and pains during urinating, &c., then other movements are necessary; they make use of the transversal chopping of the neck, which acts strongly against the chordee, and of the pressure above the os pubis in the above-mentioned lying position, which increases the venous absorption of the bladder and sexual organs, by its effect on the excited nerves of these parts.

"In the second period, if the urinating is very difficult, the perineum swollen and painful, the discharge mixed with blood, and fever is present, then a more general treatment is necessary. To increase the more local absorption in the urethra, vibrations along the whole tract of the perineum, from before backwards, are employed."—Pp. 230–232.

It betokens no small share of cool assurance in an author, that he attempts to foist such stuff as this on the public as practical medicine. His gifts in this respect will, we think, be allowed to be consummate, when our readers peruse the following therapeutical exposition of "transversal choppings," and "vibration," which are set down with as much confidence as if they were among the best ascertained facts of science:—

"Shaking Movement.—Shaking is a repeated movement, not executed with great force, but acting in the manner of slight jolts and jerks. Should the whole limb be subjected to it, then this is held by its free end with both hands of the assistant, who produces a considerable vibrating movement, which propagates itself to the fixed articulation, and to its circumference, by a very short and exactly executed shaking. If the shaking is to be employed on the head, trunk, or on tender parts, then we distinguish *a flat* (*superficial*) and *a point shaking*. The flat shake is executed by the assistant extending his hand towards the part of the patient's body he wishes to act upon, and making, under a continual pressure, a more or less strong vibrating movement with his arm, and by that means with his hand, which vibration is communicated to a certain part of the patient. The point-shake is done in the same manner, not with the hand, but solely with the end of the third finger, on the spot destined to be subjected to it; this last operation is executed very gently, so that it consists in a very fine tremulous motion, called *vibration*.

"*Effect.*—If this movement is employed in such a manner that it is transmitted to a certain articulation, the effect is directed upon the ligaments of this articulation. Employed on a greater or smaller surface, the venous absorption is increased according to the intensity of the movement. During the vibration of the front and neck, the patient feels a passing sensation of giddiness, which arises from the venous congestion produced during the movement, the secondary effect of which is an increased absorption. Point-shaking with the ends of the fingers on the pit of the stomach has a very good effect in hysterical and hypochondriacal pains. Shaking with the palm of the hand on the abdomen acts very well on the abdominal vessels and the functions of digestion.

"The merely mechanical effect of the shaking is principally the dislocation of the organic molecules; if employed daily for a long time and continuously, it produces a change of the texture in the more firm parts. Continued during a shorter time, in a slighter degree, it produces but a transient dislocation of the molecules, which being in many cases very curative, is therefore desirable. The more organic effect is directed to the blood and nervous system.

"*Vibration* acts similarly to shaking, but it is milder, and acts only superficially if done without pressure. In weakly constitutions, vibration must always precede shaking."—Pp. 90, 91.

"Chopping Movement.—Chopping consists in alternative short blows, produced by the external sides of both the operator's hands. Choppings are principally used on the posterior surface of the trunk, chest, and also on the limbs.

If it is desirable that the succussion produced by this movement shall be less and softer, then the chopping is done with the external edges of the two little fingers, while the other fingers are spread apart, but not kept spasmodically fast, so that they act also, by striking upon the little finger.

"Chopping may be confined to one part only, or may be exercised on a larger surface, by constantly moving the position of the hands. The chopping is called a *longitudinal* one, if the hands are moved in the longitudinal direction of the trunk or of the limb; and a *transversal* one, if the blows are executed across the limbs.

"*Effect.*—Choppings produce generally a venous absorption in the capillary texture, not only of the external skin and the tendinous expansions, but also, if more strongly used, in the muscles and bones; in imperfectly paralysed muscles they excite the innervation both of the motory and sensitive fibres. If directed on the lower extremities, on the soles, they act very well in hæmorrhoidal complaints, headach, &c. On the chest or along the spine, they are efficacious specific movements in certain complaints of the chest, partly by their direct influence on the muscles of the chest, partly by the tremulous, passive vibration communicated to the lungs. If chopping be employed on the chest, the fingers must always be spread and separated, that the percussion may be more elastic."—Pp. 92, 93.

We rejoice in the appearance of this book,—a few more such treatises upon such systems, all equally professing to work wonders — all of them impostures, and things will come right at last. Even those unhappy *gobemouches* who are always on the gape to swallow any bait which quackery holds out to them, will come at length to see through the homœo-hydro-iso-mesmero-kinesi-pathies, to which they are daily solicited to give their faith and fees, and will learn *stare super antiquas vias*, which have been trod, with as much comfort and security as any human path can be, for centuries.

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

LARYNGITIS—TREATMENT BY TOPICAL APPLICATIONS.

Case I.[1]—Alexander Flint, æt. 27, a salesman, was admitted into the Clinical Ward, February 17, 1851, suffering from extensive lupus of the face, severe diarrhœa, Bright's disease, and scrofulous caries of the left knee-joint. Under appropriate treatment the diarrhœa ceased, the lupus was cured, and the disease of the kidney much alleviated. On the 24th of May, about three months after admission, he first complained of dry cough and slight pain in the throat, with difficulty of deglutition. These symptoms were increased on the follow-

[1] Reported by Mr W. Calder, clinical clerk.

ing day; and on examination, the mouth and fauces were unusually red, with minute florid elevations scattered over the mucous surface. Notwithstanding the application of leeches, and sponging the fauces with a solution of the nitrate of silver, the laryngitis progressed, so that on the 14th of June the pain and difficulty of deglutition had increased, and his voice had become indistinct and hoarse. The cough also continued, but was now attended with a difficult expectoration of muco-purulent matter. On the 30th of June, notwithstanding the assiduous use of astringent gargles, occasional sponging of the fauces with solution of nitrate of silver, and the application of leeches, he was evidently worse, and he could only speak in a whisper. *July 6th.*—To-day Dr Horace Green, of New York, who went round the wards with Dr Bennett, stated that this was a remarkably good example of what he had named follicular disease, affecting the larynx. He passed the sponge, saturated with a solution of nitrate of silver (℈ij. to ℥i. of water), through the larynx into the trachea. The patient could not take a breath for some seconds afterwards, and described the sensation as like that produced by a piece of food "passing down the wrong way, and causing choaking." The immediate effect of the operation was decided improvement of the voice, and more ease in deglutition. From this time his symptoms gradually left him. On the 10th, the sponge was again passed into the larynx by Dr Bennett, and produced the same sense of temporary suffocation; but immediately afterwards he spoke with perfect clearness of voice. The application was made every second day until the 16th, when all the laryngeal symptoms had disappeared, the voice was normal, and there was no cough, expectoration, pain, or difficulty of deglutition. He now left the house; the disease in the joint had made considerable progress, but the renal disorder was much alleviated.

Case II.[1]—Helen Guthrie, æt. 25, married, a fisherwoman, admitted into the Clinical Ward, July 4th, 1851. Four months ago was seized with a cough, attended with hoarseness of the voice, dryness of the throat, painful deglutition, and pain in the larynx, which symptoms have continued with greater or less intensity up to the period of admission. Latterly, there has been considerable expectoration of purulent matter, often tinged with blood. On admission she complains of cough coming on in paroxysms, dryness in the throat, and pain in the larynx, voice cracked and occasionally absent. There is no difficulty in swallowing, but copious expectoration of frothy mucus. Can inspire without difficulty. Percussion over chest elicits nothing abnormal. On auscultation, the inspiratory murmur is harsh over superior third of chest on both sides. Over larynx and trachea there is heard a dry snoring sound. On examining the fauces, red patches were observable here and there, with slight erosion on the left side. The fauces and epiglottis were sponged with a solution of nitrate of silver (℈j. to ℥j. of water). This was repeated on the following day, and the voice was evidently improved. On the 6th, the sponge, saturated with the solution, was passed into the larynx by Dr Horace Green, of New York, and produced no feeling of suffocation whatever. It was passed afterwards every day by Dr Bennett till the 14th, when she left the house, all the laryngeal symptoms having disappeared, and the voice nearly restored to its proper tone.

Commentary.—The two cases above recorded point out to you in a very marked manner the great advantage to be derived from the method of local application to the larynx, introduced by Dr Horace Green, of New York. This practice consists in the direct application of a solution of nitrate of silver to the interior of the larynx and trachea, by means of a bent whalebone probe, with a piece of sponge fastened to its extremity. Numerous attempts had been made, with more or less success, by Sir C. Bell, Mr Vance, Mr Cusack, and MM. Trousseau and Belloc, to carry this practice into effect, and the re-

[1] Reported by Mr D. O. Hoile, clinical clerk.

sults obtained, even by their imperfect efforts, exhibited the great advantages which were to be derived from it in the treatment of laryngeal diseases. Now, thanks to Dr Green, we can with safety and certainty apply various solutions directly to the parts affected, and the two cases you have observed must convince you of the benefit which patients so treated may obtain. In case I. you have observed the progress of a tolerably acute case of laryngitis from its commencement to its termination,—the distressing symptoms produced, and the loss of voice occasioned. You have remarked, I trust, the gradual increase of the disorder, from its commencement on the 24th of May until the 6th of July, when you saw Dr Green himself pass the sponge into the larynx, and the immediate effect it occasioned. Lastly, from that moment you saw the case get better, and its termination in perfect cure eight days afterwards. No stronger evidence could be offered you in any single case of the benefit to be derived from a local application, especially when it is considered that the usual treatment was actively employed, consisting of leeches externally, gargles, and the application of a strong solution of nitrate of silver to the fauces, pharynx, and epiglottis. It was only when the application was made directly to the part affected that good was obtained. The second case, though more chronic, and though she went out before a perfect cure was obtained, is also calculated to impress upon you the value of this treatment.

The instruments to be employed are, first, a tongue depressor, with a bent handle, such as I now show you, by means of which the tongue can be firmly pressed down, so as to expose the whole of the fauces, and the upper edge of the epiglottis. In doing this, some patients experience no inconvenience, whilst in others there is such excessive irritability, that spasmodic cough or even vomiting is occasioned, which prevents the possibility of seeing the epiglottis. Secondly, a whalebone probang, about ten inches long, having at its extremity a round piece of the finest sponge, about the size of a gun or pistol bullet. The probang, towards the extremity, must be bent in a curve, which, according to Dr Green, ought to form the arc of one quarter of a circle whose diameter is four inches. Sometimes the curve must be altered to suit particular cases; and, when it is thought necessary to pass it into the trachea, the curve must be considerably less. It is important that the sponge be fine, and capable of imbibing a considerable quantity of fluid; that it be *sewn* firmly to the extremity of the whalebone, and that this last should not be cut in the form of a bulb, but tapered as much as is consistent with firmness.

The solutions of the nitrate of silver which will be found most useful are of two strengths. One is formed of ℈ij. and the other of ʒj. of the crystallised salt to an ounce of distilled water. On some occasions a solution of the sulphate of copper has been found beneficial, and it is very possible that, as our experience of this kind of treatment extends, the application of other substances in solution may be found capable of meeting particular indications. Dr Hastings, of London, speaks highly of the bicyanurate of mercury employed in this way.

The method of introducing the sponge which I have found most successful is as follows:—The patient being seated in a chair and exposed to a good light, you should stand on his right side, and depress the tongue with the depressor held in the left hand. Holding the probang in the right hand, the sponge having been saturated in the solution, you pass it carefully over the upper surface of the instrument, *exactly in the medium plane*, until it is above or immediately behind the epiglottis. You now tell the patient to inspire, and as he does so, you drag the tongue slightly forwards with the depressor, and thrust the probang downwards and forwards by a movement which causes you to elevate the right arm, and brings your hand almost in contact with the patient's face. This operation requires more dexterity than may at first be supposed. The rima glottidis is narrow, and unless the sponge come fairly down upon it, it readily slips into the œsophagus. Its passage into the proper channel may be determined by the sensation of overcoming a constriction, which you yourself

experience when the sponge is momentarily embraced by the rima, as well as by the momentary spasm it occasions in the patient, or the harsh expiration which follows,—symptoms which are more marked according to the sensibility of the parts.

If the probang be properly prepared, and the operation well performed, the actions which take place are as follows:—1st, The sponge, saturated with the solution, is rapidly thrust through the rima into the larynx, and frequently into the trachea; for if the distance of the probang be measured from that portion of it which comes in contact with the lips, the extent it has been thrust downwards can be pretty accurately determined. I am persuaded that on many occasions I have passed it pretty deep into the trachea, not only from the length of the probang which has disappeared, but also from the sensations of the patient, although this may be thought by some a fallacious method of determining the point. In this first part of the operation, the rima glottidis is as it were taken by surprise, and the sponge enters, if the right direction be given to it, without difficulty. But, 2d, the rima glottidis immediately contracts by reflex action, so that on withdrawing the instrument you feel the constriction. This also squeezes out the solution, which is diffused over the laryngeal and tracheal mucous membrane. Now, if the sponge be a fine one, it will be found capable of holding about ʒss. of fluid, the effect of which upon the secretions and mucous surface almost always produces temporary relief to the symptoms, and strengthens the tone of the voice,—results at once apparent after the momentary spasm has abated. 3d, The action of the nitrate of silver solution is not that of a stimulant, but rather that of a calmative or sedative. It acts chemically on the mucus, pus, or other albuminous fluids it comes in contact with, throws down a copious white precipitate, in the form of a molecular membrane, which defends for a time the tender mucous surface or irritable ulcer, and leaves the passage free for the acts of respiration. Hence the feeling of relief almost always occasioned; that diminution of irritability in the parts which is so favourable to cure, and why it is that strong solutions of the salt are much more efficacious than weak ones. It may be easily conceived that such good effects must be more or less advantageous in almost all the diseases that affect parts so sensitive, from whatever cause they may arise; and that this treatment is not adapted to one or more diseases of the larynx, but, like all important remedies, meets a general indication which the rational practitioner will know how to avail himself of.

The mucous membrane of the larynx consists of ciliated epithelium externally, a basement layer below this, and areolar tissue internally, richly supplied with blood-vessels. Scattered over its surface are numerous follicles, which secrete mucus. It is liable to the same structural alterations as all other similar membranes, which may be divided into—1st, Exudation into the areolar tissue, between the basement membrane and epithelium, or upon the external surface; 2d, Abrasions or desquamations of the epithelial layer; 3d, Ulcerations extending more or less deep into the areolar tissue; and 4th, Obstruction, swelling and subsequent ulceration of the mucous follicles, a lesion particularly described by Dr Horace Green, and denominated by him "follicular disease of the air-passages." These different lesions may be more or less complicated with each other, and will vary in intensity according to the rapidity of their progress, and the extent to which the mucous membrane is implicated. Sometimes the exudation is thrown out quickly and infiltrates the textures, as in œdema glottidis, or in malignant angina. At other times it is poured out on the surface as in croup. More frequently it is partial, occasioning subsequent abrasion or ulceration, and is allowed to become chronic. Perhaps the most common form it assumes is when it is chronic from the commencement, sometimes dependent on atmospheric changes, at others following repeated attacks of "cold"; in a third class dependent on too much straining of the voice, as occurs in public speakers, clergymen, singers, &c., and occasionally

connected with some general constitutional disorder, as syphilis, tuberculosis, or some form of cancer. All these forms of laryngeal disease may be further associated with similar lesions of the fauces, tonsils, uvula, and pharynx.

The symptoms will of course vary according to these different circumstances. The acute forms are accompanied with general fever, considerable local pain, more or less obstruction to deglutition and respiration, and loss or alteration in the character of the voice. As a general rule, it may be said that lesions of the fauces, tonsils, and neighbouring parts, are indicated by greater or less difficulty or uneasiness in swallowing, whilst the laryngeal disorder is evinced by changes in the character or power of sustaining the voice. Thus, as a general result of the local irritation, spasmodic action is evinced, and we have cough, at first dry, but afterwards attended with mucous or purulent expectoration, and not unfrequently with discharge of blood. Elongation of the uvula may produce these effects. It has been lately supposed that hooping-cough is only an obscure form of laryngeal disease. In the more acute and extensive cases of exudative laryngitis, the spasms are more violent and prolonged, and the greatest caution is necessary in watching persons so affected, lest, from sudden and continued closure of the glottis, fatal asphyxia be induced. Some of you may remember the case of a woman, who not long ago died suddenly in this way, in the middle of the night. Indeed, the case is so instructive in this point of view that I shall read it to you.

Case III.[1]—Frances Nichol, æt. 25, a shoebinder, married, was admitted February 27, 1851, complaining of sore throat, but breathing easily, and otherwise presenting no urgent symptoms. On the following day at the visit I found her breathing to be laborious and noisy; cough frequent; expectoration difficult, with frothy sputum tinged with blood; countenance anxious; lips livid; pulse 130, small and soft; cannot speak, nor can any one give any account of her. On examining the mouth and fauces, the mucous membrane was seen to be covered with tenacious muco-purulent matter. The soft palate is perforated by ulcerations the size of a pea in three places; there is another ulcer the size of a fourpenny piece on the roof of the mouth. The tonsils and mucous membrane surrounding the glottis were somewhat swollen, but not unusually red. On percussing the chest, no dulness could anywhere be detected. Respiratory murmurs over the large air-tubes loud and harsh, with occasional mucous râle, but their character masked by the loud snoring noise in the larynx. To have ℥ss. of wine every half-hour; an antispasmodic mixture of sulphuric æther, ammonia, and opium; the ulcers and mucous membrane of the fauces to be sponged with a weak solution of nitrate of silver, and the steam inhaler to be used assiduously. These remedies alleviated all her symptoms, so that in the evening she gave a history of her case. From this it appeared that she had suffered from cough upwards of four years, had laboured under secondary syphilis, and had ulcerations in the throat for twelve months. Seeing that she was so much better at the evening visit of the house-clerk, the intensity of the disease was supposed to have abated, but in the morning she was found dead in bed.

Sectio Cadaveris, March 3d.—The opening of the fauces was considerably contracted; and the mucous membrane of tonsils, soft palate, and from this to the root of the tongue, presented numerous ulcerations, extending to the submucous tissue, and undermining to some extent the mucous membrane. The ulcers were mostly rounded in form, of exceedingly various size, up to a diameter of three-eighths of an inch; the edges not at all elevated, and for the most part smooth, as though scooped out by a punch. The floors of the ulcers consisted of the submucous tissue perfectly clean and pale, without the least trace of granulations or pus. The neighbouring mucous membrane was scarcely at any point more vascular than natural. The arytæno-epiglottidean folds were

[1] Reported by Mr Henry Thom, clinical clerk.

hypertrophied,—that of the right side being thickened and infiltrated, that of the left being flaccid and relaxed. They could be made to lie in apposition, so as almost to close the opening of the glottis. The mucous membrane of the entire larynx was somewhat rose coloured ; and the submucous tissue of the epiglottis, the chordæ vocales, and the ventricles, considerably infiltrated with fluid. Throughout the trachea, the membrane was of a rose colour, becoming deeper towards the bronchi, and was everywhere covered with a thick mucus, which lay in small semi-transparent drops, the size of a very small pin's head, on the opening of the follicles. The tissue of the lungs was for the most part healthy, but here and there a few small portions of its substance were collapsed. The mucous membrane of the larger bronchi was congested, and the smaller ones on the right side yielded drops of purulent mucus, on compressing the cut surface of the lung. There were several small cancerous nodules in the liver, but all the other organs were healthy.

In this case I think there can be little doubt that during the night some obstruction occurred to the breathing, dependent on the local disease, which caused asphyxia and death. Neither can we have any hesitation in thinking, that had tracheotomy been performed in time, life would have been saved, inasmuch as the tissue of the lungs was healthy, and the only lesion found in those organs was a trifling bronchitis. No doubt the amelioration of the symptoms which was observed at the evening visit removed the idea of urgency, but this is just the reason I have cited the case, as a lesson to all of us, with regard to the watchfulness which is necessary in the treatment of such disorders. In another case, occurring in a man who entered the clinical ward, labouring under symptoms so similar that I need not detail them, I ordered tracheotomy to be performed at once, and the result was the preservation of life and restoration to health, although the ulceration destroyed the vocal cords, and the aphonia was complete.

The diagnosis of laryngitis is most important, and must be derived,—1st, From the general symptoms; 2d, From the results obtained by careful examination of the air-tubes and lungs by auscultation and percussion ; and 3d, From an inspection of the parts. With regard to the general symptoms, I have already alluded to the relative value to be attached to difficulties of deglutition and of speech. With regard to the interruptions of respiration, the nature of the expectoration, and the cough, we cannot with certainty refer them to the larynx, without a careful study of the condition of the pulmonary organs. Indeed, the attention which has been lately directed to the fauces and larynx, in consequence of the writings of Dr Horace Green, has demonstrated the important fact, that many of those disorders which have been sometimes called " chronic bronchitis," and others which have not unfrequently been supposed to indicate in young persons incipient phthisis, are really a chronic form of laryngitis, altogether local, and readily removed by topical applications. The distinction between them, however, often demands the greatest care in examination, but when *a good auscultator* fails to detect the signs characteristic of bronchitis or phthisis pulmonalis, whilst, on the other hand, there is unusual hoarseness or shrillness of the laryngeal murmur, dryness of the throat, and hacking cough, sometimes accompanied by muco-purulent expectoration, or even occasional spitting of blood, then his suspicions may be directed to laryngeal rather than to pulmonary disorder. It is the more important to notice this, because a good authority has lately stated,—" Expectoration of blood in persons labouring under chronic bronchitis, with or without emphysema, but without notable disease of the heart, justifies in itself a suspicion of the existence of latent tubercles." (Walshe.) In making this diagnosis, however, I must recommend to you the exercise of the greatest caution, and especially not to confound the natural hoarseness heard in the larynx of some individuals with the coarse sounds heard in others only when the organ is diseased.

The examination of the throat and upper edge of the epiglottis will do much to remove any difficulty you may experience, because in many cases alterations

in the mucous membrane of the larynx follow and accompany similar changes in the mucous membrane of the fauces and pharynx. Indeed it may be accepted as a general law, which admits of but few exceptions, that morbid changes in the mucous membranes of the pharynx and larynx proceed from above downwards, as is well observed in scarlatina. Lesions often attack the fauces or tonsils and spare the larynx; but if long continued, the latter is affected consecutively. Again, when the larynx is first attacked, as occurs among clergymen, and in the ordinary croup of children, the follicular disease in the one, and the coagulated exudation in the other, tend to pass down the trachea, and not upwards into the fauces. It follows, that when hoarseness of the voice, cough, and other laryngeal symptoms are accompanied by abrasions or ulcerations in the mucous membrane of the soft palate or uvula, by thickening or irregularity in the epiglottis, and especially by the follicular disease formerly alluded to, presenting elevated pimples more or less numerous scattered over the parts, there is every reason to believe that the larynx is similarly affected. The tongue-depressor formerly alluded to will enable you to examine these parts with the greatest ease, and in most cases the upper edge of the epiglottis will with its aid be brought into view. Still it is only by inference that we can form an opinion of the condition of the larynx. Indeed in many cases, even the summit of the epiglottis cannot be seen, the larynx being so deep-seated that it cannot be brought into view with the tongue-depressor. Hence, as regards the actual examination of the mouth and throat, we only receive exact information as to the state of the fauces, uvula, tonsils, and back of the pharynx, and valuable as such information is, we cannot determine by it with exactitude the condition of the glottis. Occasionally, under such circumstances, the finger will give us some notion, however vague, and we may feel swelling, induration, or irregularity in the epiglottis. But to derive information in this manner tact and habit are necessary. On the whole, although the local examination with the tongue-depressor should never be omitted, I am inclined to believe that it does not furnish us with the exact diagnosis that some would have us believe. In no case ought you to depend upon it alone: it should be conjoined with the knowledge derived from the inspection of parts, that derived from a careful study of the symptoms, and of the physical signs furnished by the air-tubes and lungs. It is on the general symptoms, the alteration of the voice, and auscultation of the larynx itself, combined with negative signs in the lungs, that I principally depend for a knowledge of the lesion which has taken place.

With regard to the treatment, you will gather from what has been previously said, that I regard the new mode of applying topical remedies introduced by Dr Green as a valuable addition to our other means of cure. No doubt its good effects have been somewhat exaggerated by its supporters, but sufficient evidence has been offered to you in the two cases before us that its advantages are real. It follows, from what has been previously said, that the earlier it is applied the greater the chance of success, especially in acute cases of scarlatina and croup. It has also been employed in hooping-cough, laryngismus stridulus, hay fever, and other diseases hitherto considered spasmodic, and with such success, as to lead to the conclusion that these disorders are really dependent on local irritations or an obscure form of catarrh. In various kinds of laryngeal disease occurring in the adult, whether primary or secondary, I have employed it pretty extensively, in many instances with permanent good results, and in a large number with temporary alleviation. Indeed, nothing is more remarkable than the immediate effect it has in clearing the throat and improving the tone of the voice, and hence, in many cases which do not admit of cure, it may be employed as a palliative. As such, I have successfully used it in old cases of chronic laryngitis and bronchitis, clergyman's sore throat, spasmodic asthma with accumulation of mucus in the trachea, and so on. No doubt, as our experience in its use extends, we shall determine with greater certainty the indications which demand or oppose its employment. I have already, however, met with several cases where it has been very injudiciously employed, and others

where the sponge had been passed by unskilful hands repeatedly down the œsophagus without any good effect, the patient having been persuaded for a considerable period that it had been applied to the larynx. Circumstances of this kind may bring the practice into disrepute with some, but I trust you will discriminate, and not lightly abandon it from a few failures, or be led into the opposite error, and suppose, from one or two favourable cases, that it is capable of being invariably successful.

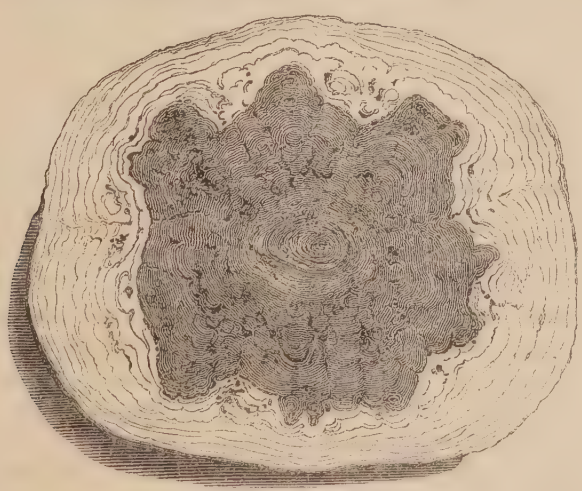
CLINICAL SURGERY.

REPORT OF SOME OF THE MORE INTERESTING CASES OCCURRING IN THE CLINICAL SURGICAL WARDS OF THE ROYAL INFIRMARY. BY E. R. BICKERSTETH, ESQ., *Resident Clerk.*

During the last six months, five cases of calculus vesicæ have occurred in these wards. All have done well, and are now entirely relieved of their former distressing symptoms.

Some of the cases are not possessed of more than ordinary interest; and of these I shall therefore merely mention the prominent facts. The others require more particular notice. I report them in the order in which they occurred.

Case I.—Peter Trotter, æt. 14, was admitted on the 10th of April, suffering from the ordinary symptoms of stone. For as long as he can remember, he has had more or less uneasiness in the region of the bladder; but it is only within the last few months that his sufferings have been such as to incapacitate him for exertion. On introducing a sound, a calculus of large dimensions was immediately detected. It could be felt also by the finger introduced into the rectum. On the 16th, the patient being brought under the influence of chloroform, Mr Syme removed the stone by the ordinary lateral operation. The neck of the bladder was cut through to the requisite extent, and the parts were then fully dilated with the finger. Notwithstanding this, some difficulty was experienced in extracting the stone, owing to its very large size, and the narrowness of the boy's pelvis. One superficial artery required to be tied. A gum elastic tube was then introduced into the bladder by the wound, and retained by tapes. When he awakened, he expressed himself comfortable and free from pain. The stone was about the size of a hen's egg, of a nearly spherical form, though somewhat flattened from side to side. It measured in



its long diameter two inches; in its short, an inch and three quarters; and weighed two ounces and three quarters (Troy). The external surface was of a yellow-ochre colour. It was smooth, but covered with a number of flattened mammillated eminences. On making a section, it was found to consist of a

central nucleus of lithic acid, around which was a layer of oxalate of lime, two-thirds of an inch in thickness, of a dark brown colour, and very nodulated externally; and outside this again were the regular concentric laminæ of lithic acid, which formed its external boundaries.

April 18.—He has not had a bad symptom, and now declares himself quite comfortable. The tube removed to-day from the bladder.

May 5.—The urine passes chiefly by the natural passage.

May 16.—For some days past all his water has passed by the urethra, and the external wound has now all but cicatrized. Feels well in every respect. Dismissed *cured.*

Case II.—John Henderson, æt. 61, admitted the 11th of June, on account of stone in the bladder, under the symptoms of which he had been labouring for five years and a half. Two years ago he was in the Infirmary, suffering from the same complaint. During his residence, which was of three months' duration, the operation of lithotrity was performed repeatedly, and apparently with success; for when he left, he was perfectly relieved from every symptom of calculus, and repeated careful examinations with the sound failed in discovering any foreign body. After ten months' enjoyment of perfect health, he began to suffer in the same way as before; and finding his symptoms increasing, he at length again applied for relief. A stone was detected without difficulty, and on the 16th Mr Syme performed the operation of lithotomy, removing a calculus the size of a large walnut. Its weight was 316 grains, and on section it was found to consist of phosphates, with a central nucleus of lithic acid, the latter being evidently a fragment of the stone previously crushed. On the 25th the

urine was in great part flowing by the urethra, and on the 14th of July he left the Infirmary perfectly well in every respect, not having had a bad symptom since the operation.

Case III.—Archibald Welsh, æt. 6, a sallow and unhealthy-looking boy, was admitted on the 14th of August. Two years ago he first suffered from urinary symptoms, complaining of pain at the extremity of the glans penis, and of difficulty in making water. Attacks of this kind came on periodically, lasting generally for one or two days, and then entirely disappearing for two or three weeks. In this state he continued until three months ago, his general health not suffering materially; but since this period he has never passed water without extreme pain; generally it has come only by drops, and that after violent straining and ineffectual attempts in various positions. His sleep has been disturbed every half-hour during the night. He has lost flesh rapidly, and his health has suffered much. On examination, a stone was detected lying at the neck of the bladder, and it was then determined, on account of the severity of the symptoms, that the operation should be performed on the following day. In the evening he had complete retention, lasting for five hours. The child was in extreme agony. He could not be kept in bed, but ran about the ward screaming loudly. Chloroform was administered, and eight ounces of turbid urine were drawn off by the catheter.

August 15.—During the night he was more comfortable, and slept better than

for many weeks past. His bowels are freely open, fluid fæces passing involuntarily from him each time he strains to make water. At twelve noon Mr Syme removed the stone by the ordinary operation. It was of the size of a hazel nut, and externally was coated with a soft white layer of phosphates, which, when rubbed off with the finger, disclosed the characteristic surface of the mulberry calculus.

On the 21st the urine was passing chiefly by the natural passage, and the child's health and condition were greatly improved.

August 28.—The wound has completely healed, and the boy is running about in good health and spirits. Discharged cured.

Case IV.—Adam Sinclair, æt. 33, admitted August 25. For the last two years he has suffered from urinary symptoms, manifesting themselves by frequent calls to make water, and by the stream being frequently arrested during its passage. This was the only inconvenience he experienced, until nine months ago, when pain during and after micturition commenced suddenly, and was so severe as entirely to disable him. Frequently the water would stop altogether for a while, and occasionally it was only after several attempts in various positions and by pressure with his finger on the perineum, that he was able to succeed in emptying his bladder. Latterly the urine has been very turbid, and often a quantity of transparent glairy matter has escaped, mixed with it. Upon passing a sound, a calculus was immediately felt near the neck of the bladder.

On the 29th, the patient being under the influence of chloroform, Mr Syme removed the stone. A vessel bled freely, and was tied. During the extraction a portion of the stone crumbled in the forceps, but nevertheless the whole was brought out together. It was of an oval shape, about two inches and a half in length, and three-quarters of an inch in diameter. Its exterior was rough and porous,

consisting of phosphates. During the examination of it, one half crumbled in the hand, leaving a piece of slate-pencil, three-quarters of an inch long, projecting from the centre of the remaining portion. On questioning the man concerning this, he admitted that two years ago he had introduced the pencil into the urethra, and that it had accidentally slipped beyond his command.

Sept. 24.—For the first time since the operation, he has to-day passed urine by the urethra. The wound is slowly contracting, and the man feels well in every respect.

Oct. 13.—Some of the urine still flows by a narrow fistulous opening which as yet exists in the perineum. During the last fortnight his appetite and general health have somewhat failed, and in consequence it has been thought prudent to remove him to a more airy ward.

Oct. 20.—The wound has now completely cicatrized, and for the last five days all of the urine has passed by the urethra. His health also is improving rapidly.

Case V.—William Forrester, æt. 48, a strong, healthy-looking man, was admitted on the 2d of September. Until nine months before he had always enjoyed perfect health, and had never suffered from the slightest urinary symptom. At that period he met with the following accident :—While raising one end of a heavy stone, his hands slipped, and he fell backwards, striking the lower part of his back with violence against the ground. At the moment he felt severe pain in the perineum and hypogastric region, and never since has

he been able to pass water without more or less pain, both during the act and following it. His symptoms—the ordinary ones of calculus vesicæ—have never been severe, and he states that he is at present, and has been all along, in precisely the same state as immediately after the accident. A calculus, of apparently large size, was detected by means of the sound near the neck of the bladder.

Sept. 6.—Mr Syme to-day performed the ordinary operation of lithotomy, and removed the stone. Slight difficulty was found in extracting it, owing to its size and peculiar form. It is of a triangular shape, and remarkably flattened from side to side. The three edges are each about one inch and a half in length, while its greatest thickness is not more than two-thirds of an inch. Its surface is of a yellowish brown colour, smooth above and below, with the exception of a few small flattened mammillated projections. Its margins are thickly studded with larger prominences of a similar character. Near one of the angles of the calculus is an orifice capable of admitting a small bean ; the edges of this orifice are rounded, and covered with minute elevations like those just described, except at one part, where a thin smooth lamina of calcareous matter projects abruptly, and forms a portion of the boundaries of the opening. The free edge of this lamina is clear, sharp, and well-defined, and conveys the impression that a portion has been broken off it.

On making a section of the stone, the orifice is found to lead into a small cavity, which is filled with a firm, dark-brown membranous substance, imbedded in which are minute granules of phosphatic deposit. This substance forms the nucleus upon which the several layers of calcareous matter have been successively deposited. The most internal lamina, about two lines in thickness, consists of the white phosphates. It forms a circle, complete in rather more than three-fifths of its extent, but wanting in the remaining part, being here interrupted by the orifice communicating with the exterior. Upon this phosphatic layer are concentrically arranged, in the manner shown in the accompanying sketch, the various laminæ of lithic acid of which the remaining portion of the calculus consists.

On microscopical examination the substance filling the cavity was found to consist of blood corpuscles and ill-defined filaments of fibrous matter.

On the 16th of September, all the urine was passing by the natural channel, and on the 4th of October he was discharged cured, never having had a disagreeable symptom since the operation.

From carefully examining the characters of this most peculiar calculus, and associating the facts thus ascertained with the man's own account of the origin and history of his complaint, the following inferences may, I think, with reason be deduced :—

1. That the stone has existed in the bladder for a far longer period than nine months.

2. That before this period it was attached to some part of the walls of the bladder, and that the separation was the immediate consequence of his fall.

3. That its mode of attachment with the bladder was either by means of a clot of blood adherent to its internal surface, or through the calcareous matter being deposited upon some small vascular prominence.

Thus much there is every reason to believe, but still more may, with a tolerable degree of probability, be supposed to have been the case. The calculus was of a triangular shape ; it was also of a certain size, and it was peculiarly flattened on both its upper and lower surfaces. In all these respects, it corresponds remarkably with that part of the bladder called the "Trigone vesical." Moreover, the bladder, when collapsed, is of a triangular form, and can never contract within the fixed boundaries of this space. Thus it seems probable that this was the part in which the stone had been attached prior to the accident.

Part Fourth.

PERISCOPE.

PHYSIOLOGY.

ON THE DERMOID LAYER OF THE MEMBRANA TYMPANI. BY JOSEPH TOYNBEE, F.R.S.

Between the epidermoid and radiating fibrous layers of the membrana tympani, there is a distinct and complete lamina of membrane, which is continuous with the dermoid layer of the meatus. The presence of this membrane, to which, on account of its structure and functions, I have given the name of dermoid layer, is best demonstrated by carefully dissecting under water the membranous meatus from the upper surface of the osseous tube as far as the attachment of the membrana tympani. At this point the periosteum of the meatus is seen to become continuous with the radiate fibrous lamina; this being cut through, the dermoid layer is seen passing down over the outer surface of the radiate fibres, and separating them from the epidermis. If the upper portion of this layer be drawn gently downwards by one hand, by means of a fine needle in the other, the delicate cellular tissue connecting it to the fibrous lamina can be broken up, and it may be removed entire. The presence of the dermoid lamina may also be shown by the introduction of a fine bristle between it and the radiating fibrous layer, at the superior part of the membrana tympani, and then by passing it down, the cellular tissue is lacerated.

In a healthy state, when uninjected by blood or by artificial means, the dermoid lamina is thin and transparent; examined by the microscope, its structure is found to resemble areolar tissue. When injected, this membrane is observed to have numerous blood-vessels ramifying through it, and they form an elaborate plexus; when these vessels are enlarged, they give the red appearance to the surface of the membrana tympani, which is so frequently met with during life. It is upon the supply of nerves to this membrane that the exquisite sensibility of the membrana tympani depends.

A knowledge of the existence of the membrane here described is of interest to the anatomist, who recognises in it the secreting organ of the epidermoid layer of the membrana tympani; and to the surgeon, who, by its presence, is able to understand phenomena occurring in certain diseases of the ear, which have been hitherto incomprehensible to him.—*Philosophical Transactions*, 1851.

ON HÆMATOIDINE AND BILIFULVINE. BY VIRCHOW.

The author considers hæmatoidine to be a compound of hæmatin and some protein substance, the latter probably forming the crystalising, and the former the colouring portion of the compound; and this is the more likely since Reichert has found an albuminous substance in the form of tetrahedral crystals, in extravasated blood. Wherever hematoidine is being formed, these two substances are present, and when its formation is completed neither is to be found,—hence the author's conclusion that the new body is a compound of both. With regard to Henle's supposition, that hæmatoidine is of a fatty nature, Virchow remarks that no doubt fat has a peculiar attraction for this substance, and seems somehow to favour its formation by promoting the metamorphosis of hæmatin; but that its nature is not fatty, and that the occurrence of coloured drops of fat after treating masses of hæmatoidine with mineral acids, is to be accounted for by its having formed the superficial layer of the fat; farther, its crystalography differs entirely from that of cholesterine, beside which it is often found simultaneously with this substance, as in cysts of the thyroid body. Its crystalline form is also entirely different from that of stearine; and besides pure crystals of the latter have not yet been found in the human body.

During his investigations of hæmatoidine, he was led to examine the bile, with the view of discovering cholopyrrhine crystals, which resemble the former; instead of these, however, he found in the bile and liver of a man, dead of cancer of the liver, and repeatedly afterwards, under similar conditions, crystals, probably corresponding to bilifulvine. These conditions are retention of the bile consequent on chronic catarrh of the gall-bladder, by which the mucus is transformed to crumbling, greasy, transparent lumps, on which the biliary colouring matter is deposited, and fat drops and crystals are found. The bile is, under such circumstances, like thick, dark-brown gruel, in which are seen numerous small dark points, the formation-places of the crystals in question. These are long, hair-like, yellowish-red needles, of apparently irregular form, about 0·005 of a line in length, and about 0·0005 of a line in diameter. Most appear, at first sight, to have a thick lumpy end and a fine point; sometimes they are bent and twisted. Closer examination shows this appearance to depend on the larger needles being compounded of numerous smaller ones, sometimes placed regularly, and at other times covering each other, always, however, more or less loose, and never forming large masses, but connected with drops of fat and tables of cholesterine. The drops of fat are placed on the points of the needles, while they (the needles) are deposited on the narrow side of the cholesterine tables. The needles are yellowish-red, like hæmatoidine, and difficult to recognise, especially in thick fluids, where they frequently resemble the colourless needles of margarine. These bilifulvine needles dissolve easily in potass, and are not re-deposited; acetic acid has no action on them; concentrated sulphuric acid sometimes slowly and gradually dissolved them; at other times more rapidly, a reddish-yellow areola first forming round the needles. Pure nitric acid has no action on them; the bile surrounding them undergoes the well-known changes of colour. Bilifulvine appears to belong to the granular pigments, ranking between hæmatoidine and melanin.—*Schmidt's Jahrbücher*, August 1851.—B.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

PECULIAR FORM OF THROMBUS. BY DR MONTGOMERY.

After speaking of the commonly described forms of Thrombus, Dr M. continues:—

So far, all the cases alluded to have been of effusion into the external labia pudendi, but I think I have learned from sufficient observation that these are not the only situations in which thrombus occurs during, or in consequence of labour, but that quite a similar accident, though differing much in degree,

happens not unfrequently in the tissue of the cervix uteri, and especially in the substance of the anterior lip of the os, giving rise to a condition greatly resembling in some of its characters that which is generally spoken of as œdema of the part, from which, however, it is altogether a different affection. The first time that this matter attracted my attention in practice was in the following case :—

Mr S. requested my attendance on his wife, on 11th March 1830, at ten P.M., she being then in labour. The night before she had diarrhœa, and some discharge of blood from the vagina; she had six children by a former marriage, and her last was now five years old. I found her with labour established, and with some hemorrhage, but not much. One part of the lower portion of the cervix, towards the right side, felt very prominent, thickened, and spongy, so as most strikingly to resemble a portion of the placenta; and it appeared to me that it was from this part the hemorrhage was proceeding. The head presented naturally; the labour progressed favourably, and as the descending head came to press more forcibly into the os uteri, and on the spongy tumour, its contents were discharged; the hemorrhage ceased, and did not again return, and at half-past twelve o'clock a male child was born, healthy and vigorous. The placenta came away in about ten minutes. There was no further hemorrhage or other unfavourable symptom, and the lady recovered well. Mr S. was a medical man, and, seeing the hemorrhage, he became anxious, and made an examination, when he was greatly alarmed at feeling, as he thought, the placenta at the os uteri, and immediately sent for me.

After hearing his report, I examined the patient, and at the first moment thought his account was only too correct; but a little further examination easily satisfied me of the true state of the case, because it was clearly evident that the tumour resembling the placenta was not merely applied to the inner surface of the uterus, but was identified with the substance of the organ, so that it was impossible to insinuate the point of the finger between them, while it could be readily passed behind or partially around. To those who may not have met with this state of the parts, I would beg leave to say, that the resemblance of this tumour, when in the form in which it occurred in this case, to the placental structure, is sometimes so close as very readily to deceive an incautious examiner. The circumstances under which we meet with this condition in practice are generally as follows :—About the time when the os uteri is more than half dilated, especially if the waters have already been evacuated, we find that the anterior lip gets gradually, but pretty quickly, fuller, thicker, and more prominent, and, instead of yielding readily before the pressure of the head, and slipping away upwards, it continues to descend as the head is forced downwards; and if we try with the point of the finger to push it past the head, and out of the way, we do not succeed; an unexpected delay now takes place, and a labour which we have every reason to hope would go on quickly, and be completed within a short time, is protracted for perhaps two or three hours, or even more, and the patient's pain increased both in character and duration. At length the swollen labium diminishes, suddenly recedes, and disappears, and very often, at the same moment, some blood is discharged, generally of a deep rich colour, and now the retarded head descends more quickly.

I do not wish to be understood to say that the swollen lip in such cases always bursts and discharges its contents, for such is not the fact, but that this is frequently the result I feel quite assured. I had recently (10th October 1850) an opportunity of *seeing* this condition of the anterior lip of the os uteri during labour. The lady had four children, and her labour was in every respect satisfactory. Shortly after the discharge of the liquor amnii the pains became urgent, and the anterior lip of the os began to swell, and immediately, as in all other instances of thrombus that I have met with, the propulsive power of the pains was at once impaired, although they continued strong, and returned at short intervals; but still the swollen lip resisted the passage

of the head, or at least did not yield, and continued to be pushed down with it, until with each pain it protruded under the arch of the pubis, *of a deep purple colour, like that of black currant jelly;* suddenly it disappeared, and one or two ounces of deep, rich coloured blood flowed away; the pains became in a few minutes much more efficient, and the labour was soon happily completed. The lady recovered well. Is this accident an occurrence of little moment beyond the temporary delay and increased suffering which it causes? or is it one that may have dangerous results? are questions of no inconsiderable importance.

Were I to judge from my own experience alone, I should be bound to say, that I had found both forms of thrombus, the external and the internal, free from *fatal*, though liable to produce *serious* consequences; but the experience and observations of others have shown that the bursting of bloody tumours, wherever situated, has been often a source not alone of great danger, but sometimes even of death. I have already alluded to the occasionally pernicious nature of the hemorrhage accompanying the thrombus of the external labium, and I recently heard the particulars of a case, and examined the uterus after death, in which, I think, death resulted from a thrombus in the substance of the cervix uteri.

The case to which I allude was brought under the notice of the Obstetrical Society by Dr George Johnston. The woman had an easy and apparently safe labour, with a breech presentation, and for three or four days afterwards went on perfectly well; but on the fifth day, greatly to the surprise of those who saw the patient, and found the uterus well and firmly contracted, a profuse hemorrhage took place suddenly, and the patient sank within an hour and a half. After death, it was ascertained that there was in the substance of the cervix uteri, close to the os, at the left side, a distinct cavity, capable of containing a small orange, into which opened the mouths of several blood-vessels.

After a careful inspection of the parts, I felt no doubt that this was an instance of the accident I have been describing; that, in fact, a thrombus had formed, the cavity of which was filled by a coagulum; that the thin stratum of uterine structure, or perhaps only mucous membrane and areolar tissue, interposed between the cavity of the thrombus and that of the uterus, was gradually attenuated, and at length burst, or sloughed away, allowing, of course, the coagulum to escape, and the open blood-vessels suddenly to pour out a torrent of blood, under the overwhelming influence of which life was extinguished within the awfully brief space of an hour and a half. It is to be observed, that in the case of thrombus of the external labium, related at the commencement of these observations, the slough separated on the fourth and fifth days; and here, in the case now before us, the fatal gush of blood took place just at the same interval after labour. It is but right to say of such a case, that it exhibits one of those unforeseen but perilous accidents against which no human care or caution could guard, or human skill be always available to save the patient's life.

In the month of July of last year, I saw a case which I have great reason to believe was of the kind now under consideration.

A lady affected with varicose veins, which extended all up the lower extremity, and could be traced into the vagina, was delivered, after a natural and favourable labour, at midnight; but shortly afterwards a fearful rush of blood took place very unexpectedly, for *the uterus was well and firmly contracted.* So great was the hemorrhage, that complete prostration was immediately produced, and when I saw her, she was cold and pulseless, nor had she any return of pulsation in the radial artery for six hours and a half from the time of the sudden hemorrhage, and during a part of that time the action of the heart could neither be felt nor heard. All this time the uterus remained perfectly contracted, but in the situation of the anterior lip its substance felt as if broken up into a soft pulp, the consequence, as I believe, of the formation and rupture of a bloody tumour. To our great joy, she ultimately rallied under the treatment adopted, and completely recovered.

I believe the formation of a thrombus at or near the os uteri, its rupture, and consequently open state of some vessel or vessels in the cavity thus formed, is very often the real cause of those hemorrhages after the birth of the child and expulsion of the placenta, where the uterus is found to be well and firmly contracted; and it is to be recollected, that just where the thrombus forms is precisely the situation in which the contractile power of the organ is most feeble; and should it happen towards either side, it is then close upon the very part where the blood-vessels send in the largest supply to the uterus. And I would say, that while we should neglect no general or subsidiary means to check the hemorrhage, or secure the patient from its dangerous effects, our *greatest security*, under such circumstances, will be obtained by plugging the vagina, while, at the same time, we take the necessary precautions against allowing the uterus to relax and become distended with blood.

If the occurrence of this affection has been described or noticed by any former writer, I am not aware of it; but in the writings of two of our most eminent authorities in practical midwifery, I find in each a passage distinctly bearing upon the view I have here put forth, and, as far as they go, tending to confirm its truth. At p. 271 of his Introduction to Midwifery (5th edit.), the admirable Denman says:—"The uncoloured mucous discharge from the vagina, which pretty generally occurs before labour, on its accession is usually tinged with blood, or a small quantity of pure blood is discharged. This sanguineous discharge, which varies in quantity and appearance in different women, is popularly called a *show;* and it happens more particularly at two periods of a labour,—when the os uteri *begins* to dilate, and when it is finally dilated. In the first instance, it is probably occasioned by the separation of a few of those vessels by which the membrane which connects the ovum to the uterus was originally bound; and in the second case, by the effusion of some blood before *extravasated in the substance of the os uteri; for this part, in some cases, acquires an uncommon thickness from that cause, independent of any œdematous or inflammatory tumefaction.*" And Dr Burns observes, at p. 460 of his Principles of Midwifery (9th edit.):—"In tedious labour the os uteri, and even the cervix, sometimes becomes swelled, as if blood were effused into the substance." Where such a condition is recognised during labour, we shall show our wisdom by interfering as little as possible with it; by abstaining from attempts to get the gorged lip of the os over or past the head; and when the head does descend, and is expelled, we should be more than usually slow to withdraw the body of the child, more than usually careful to secure complete uterine contraction, and more than usually watchful of the state of our patient after all is over; although we must, at the same time, acknowledge the painful truth, that, with all the care and caution that the most anxious circumspection can prompt to the adoption of, an accident of this kind may take place under circumstances of which we have no intimation, until alarmed by the urgent danger of our patient, or struck aghast by her unexpected and untimely death.—*Dublin Medical Press*, August 27, 1851.

SUCCESSFUL CASE OF TRANSFUSION OF BLOOD IN POST-PARTUM HEMORRHAGE. BY DR MORMONIER.

Dr Mormonier was called to a woman, aged 30 years, of a lymphatic temperament, slightly weakened by frequent pregnancies and tedious labours. On his arrival, the pains had almost ceased. The patient was feeble and exhausted by long and fruitless attempts to expel the child, the labour in his opinion being delayed by ante-version of the uterus. Turning was performed, unusual hemorrhage followed, the placenta was rapidly extracted, and the uterus excited to contraction; the hemorrhage was soon arrested. In an hour and a quarter hemorrhage occurred in abundance, and was terminated by syncope. The hemorrhage soon recurred, and this time left the patient in a long fainting fit, and in a state of great weakness. Dr Mormonier was called back to her. He

found her extremities cold, the pulse almost imperceptible, and sometimes absent. The sight was obscured, and there was frequent fainting.

He employed astringent and refrigerent applications, the concentrated infusion of ergot, dry frictions on the surface, and hot cloths to the limbs for three quarters of an hour, without the least amendment. The death of the patient seemed inevitable, when he determined, although unprovided with the necessary apparatus, to perform transfusion of blood. The apparatus which he was able to obtain consisted of a child's syringe, capable of holding seventy grammes (between two and three ounces), some hot water, basins, and linen. A young female, a neighbour of the patient, consented to supply the blood.

Dr Mormonier laid bare the right basilic vein for about an inch and a quarter, he then isolated it for about four-fifths of an inch, so as to pass a thread under it, which might tie it down to the canula of the syringe. He then made an incision into the vein for one-fifth of an inch; only two or three drops of blood escaped. The vessel was compressed above and below. He then caused the blood to flow from the young woman into a cup placed in a vessel of hot water. Having first warmed the syringe, he filled it with blood, taking care to introduce no air, and introduced it into the opening in the vein. After carefully pushing down the piston about one-third of its length, the blood was found to be retarded. This inconvenience was obviated by wrapping the syringe in cloths dipped in hot water, when it was again introduced. All the blood contained in the syringe now entered the vein. The total quantity introduced was about ninety grammes (about four ounces). The operation was unattended by pain or any other inconvenience. Almost immediately after the transfusion, the respiration became more regular and the pulse stronger; the disposition to syncope ceased suddenly; the obscurity of vision was rapidly dissipated. Dr Mormonier ordered the frictions and hot cloths to be continued, and administered rhatany and ergot. The circulation and the animal heat were gradually re-established. Two hours after the operation the patient slept for some minutes, and this was followed by an unhoped-for amelioration. From this time convalescence was rapid; the milk was secreted regularly. In ten days the patient could sit up for an hour each day. On the twentieth day she was quite well, and on the thirtieth day she had resumed her ordinary occupations.—*Gazette Médicale*, July 5th, and *London Journal of Medicine*, August 1851.

[We doubt greatly if so small a quantity of injected blood could have possibly been the cause of the re-action described; more probably the restoration was a result not owing to, but occurring in despite of, the transfusion.]

CASE OF OVARIAN PREGNANCY OF TWELVE YEARS' DURATION, WITH A PERFECTLY MATURE FŒTUS. BY DR KIWISCH, OF ROTTERAU.

The author of this contribution to pathology stated that the preparation which he then exhibited to the Würzburg Society had been removed from the body of a woman, 34 years of age, who had died in convulsions with Bright's disease of the kidneys. She had born twins at an early age; and upwards of eleven years since, the signs of a new pregnancy were so distinct that an accoucheur had been engaged. About the seventh month she suffered from severe uterine hemorrhage; but, as neither then nor afterwards did labour occur, it was supposed, notwithstanding that fœtal movements were felt, that the diagnosis of pregnancy had been erroneous, and the patient was henceforth treated as the subject of an abdominal tumour. It is very remarkable that neither during the development nor after the death of the ovum were any symptoms of disordered health known to have occurred. At the periods of the greatest development of the fœtus, the circumference of the abdomen was always less than is usual; and in the last year of her life it very much decreased.

On examining the body, the tumour was found partly covered by the intestines, but not adherent to them, lying somewhat to the right side; and being connected to the uterus by the broad ligament, it had the appearance of an en-

larged ovary lying freely on the brim of the pelvis. The tumour, when removed entire from the pelvis, presented the following characters :—

Its size was about that of the head of a child of two years of age. It was perfectly round, and was covered with a white shining membrane, which might have been mistaken for peritoneum, and was on the one side continuous with the broad ligament. On its upper surface was the hypertrophied and flattened fallopian tube. The fimbriated extremity of the tube was so intimately united to the outer covering of the tumour, that it could not be traced, and therefore its abdominal aperture could not be discovered ; neither could any communication be traced between the tube and the cavity of the tumour. The upper part of the coats of the tumour was very thin, and at this part presented a small bunch of what proved to be finger bones protruding through the membrane. The tumour was covered by a smooth, firm, continuous tissue, of the consistence and hardness of a fibrous membrane. In like manner, diverticula of the peritoneum were found, which contained the feet. On the under surface of the tumour was a substance about two inches in length, and about three lines in thickness, which was evidently the altered remains of the ovarium. This body was examined by Drs Kiwisch and Kölliker ; it presented, however, no trace of structure by which the true nature could be determined.

On opening the tumour the inner surface of the membrane was found firmly adherent and incorporated with the parts, with which it was in contact, of a compressed fœtus,—*e.g.*, with the right parietal and temporal bones, and with the bones of the foot and hand, which lay most externally. On the surface at other parts the membrane was raised, as if in blisters, by a fatty unctuous substance, which intervened between the fœtus and the tunics of the cyst. Those portions of the fœtal bones which were further removed from the external coats of the tumour, were covered by their own integuments and soft parts ; this was seen to be the case with the other portions of the scalp, neck, and upper and lower extremities ; but it was impossible to ascertain the condition of its surface generally, without entirely destroying the preparation, so completely was it folded and compressed upon itself in every direction ; and so disfigured was it by this compression, that it was difficult to recognise anything like features. The integuments of the scalp presented fully-formed hair, and the size of the several bones of the skull and of the extremities left no doubt that they belonged to a mature fœtus. On the inner surface of the sac was also to be perceived the remains of a placenta, about the size of a small plate, having on its internal aspect some traces of amnion and umbilical cord. The uterus was distinctly hypertrophied, particularly in the direction of its length, which was augmented at least to the amount of one-third. The other tube was united by adhesions to the ovary and uterus.

Dr Kiwisch observes, with regard to this case, that if this cannot, on anatomical grounds, be shown to have been an ovarian pregnancy, still more are physiological opinions adverse to the supposition of development occurring within an ovarian cyst ; inasmuch as late experiments appear to have shown that the rupture of a cell is necessary to the impregnation of the ovum by the immediate contact of the spermatozoa.

The rupture of the graafian follicle was attributed to pressure exerted upon the ovary by adjacent organs, and not to any independent pressure existing in these organs. This pressure may be unequally exerted, and the minute ovum in that case retained after the fluid has been discharged. This explanation is applied by the author to the case now related, which shows the graafian follicle to be sufficiently vascular to supply nourishment to an ovum in the course of development.—*Verhandlungen der Physicalisch-Medicinischen Gesellschaft*, in Würzburg, 1851.—*London Medical Gazette*, September 19, 1851.

MATERIA MEDICA AND TOXICOLOGY.

ON THE GOOD EFFECTS OF MUGWORT (*Artemisia vulgaris*) IN COMBINATION WITH ASSAFŒTIDA IN CHOREA AND SLEEP-WALKING IN CHILDREN. BY DR NEUMEISTER, OF ARNEBURG.

The author was led to the employment of this method of treatment by the following circumstance:—In the case of a choreic girl of 11 years of age, he had failed by all the most approved methods of treatment, in producing any permanent satisfactory results. The patient passed from under his hands, but in the course of a few weeks was cured; and he now learned from the parents of the child, that this had been effected by widow of a clergyman in the neighbourhood, who herself prepared and vended a secret remedy for the cure of such spasmodic affections. At first he was disposed to attribute the above result either to his previous treatment, or to a spontaneous cessation of the disease from the child advancing in growth. Another case, however, soon occurred, which led him to doubt the soundness of these conclusions. This we give at length in his own words, as the narrative is curious in other respects.

"I received a letter from Judge N., a rich proprietor, whose physician I had been for many years, requesting me to undertake the cure of a peasant's daughter, who was suffering under the most extraordinary spasms, and, according to the country people, was possessed by the devil. Compassion for the child, along with dislike at the unpleasant noise which this case was far and wide exciting, and which had almost converted the peasant's house into a place of pilgrimage, had induced the judge to make this request, which he urgently pressed.

"I found in fact, on arriving at the peasant's, his house surrounded by from twenty to thirty persons, for the most part old women, who were walking up and down with countenances expressive of the highest degree of expectation and devotional terror. I learnt that the hour was near in which the evil spirit would recommence his work; and on entering the room, I saw on a somewhat raised bedstead a pale fair-haired girl of about 11 or 12 years of age, lying quietly as if asleep. It was ten o'clock in the morning. Not far from the bed of this child, near the window, stood a table covered with a white table-cloth. Upon this lay a large black Bible open, and near the Bible stood two lighted tallow candles, and a large willow branch. Before the Bible sat an old man with smooth, lank hair, and spectacles on his nose. I recognised in him a superannuated tailor, who earned a miserable subsistence in the village by repairing old garments. The tailor, without allowing my entrance in the least to disturb him, had, as was afterwards told to me, opened up the psalms before him, and muttered away from the same continually. I had entered quietly, and determined to await the issue of the scene. The child gradually began to grow restless; it moved first an arm, then a leg, then both legs, then its whole body, made distorted faces, and uttered a shrill cry. The tailor immediately raised his voice to the same pitch, and exclaimed slowly and syllabically,—'Beelzebub! Beelzebub!' The child now screamed much louder, and moved more convulsively; but the louder the child screamed, so much the more energetically did the tailor roar out 'Beelzebub! Beelzebub!' All the old women now rushed in, the room filled with people, and soon became insupportably hot. The father and mother of the child cried and lamented. The child itself took the most violent choreic convulsions, sprang high up in the bed, flung itself out of bed on to the ground, where, by the advice of a somewhat sensible aunt, bedding had been laid down, then lifted itself up, and succeeding in reaching the middle of the room, where it turned itself round like a top, with a rapidity which neither Elsler nor Taglioni could ever attain to. The little one then fell down exhausted, and lay for a few minutes quite still. During this time the tailor had begun with nasal tone to chaunt a psalm, I do not know which.

The people present, the most of whom had brought Bibles with them, sang along with him; but as a well-known church melody, which the tailor was determined to adapt to the psalm, did not at all suit the text, there came forth one of the most extraordinary songs possible. The tailor now sprang up, laid hold on the willow rod, and dipped it in the water; and now, seeing that he was about to act, I began to pay still greater attention to what was going forward, determined to set myself at once against everything unseemly, or that might be prejudicial to the child. The child still lay quite quiet; the tailor stood beside her, the Bible in one hand, the rod in the other, his glance fixed on her with the utmost expectancy; a breathless, almost painful, silence reigned in the apartment. All at once the child began to move; first, as I distinctly marked, convulsively with the right arm, then with the right leg, and then with the whole body. Suddenly she sat upright, screamed out, and began an inward muttering, much in the manner of one who is forming inarticulate sounds within his breast; the tailor listened; the girl uttered all kinds of tones, moving convulsively the whole time. The tailor could restrain himself no longer; he gave the child two or three strokes on her back with the rod, and called out in a loud shrill voice,—'Beelzebub Tobias! Beelzebub Tobias!' I may as well remark here, that when I afterwards asked the tailor why he gave the surname of Tobias to the devil, he replied that this was no surname, but signified, in the holy language of the Patriarchs, 'Go away;' but I adopt the hypothesis, that the tailor, who in his youth had travelled in the south of France, had retained an indistinct recollection of an old Roman Catholic form of exorcism, and that Tobias might mean *abeas, abeas;* the rest of the operation he may perhaps have added from his own fancy. I will not, however, depict further the conduct of these honest but silly people. I will only remark, that the girl had daily one of the severest attacks of St Vitus' dance, which lasted from three to four hours, and sometimes recurred again in the afternoons, accompanied by delirium and ecstatic states, in which she said and sung the strangest things. As it was evident that the above-mentioned conduct of the old tailor, and those belonging to the child, as well as the concourse of people, did but heighten the attacks, and make the malady far worse, it was time to put an end to it. Through the endeavours of the village pastor and schoolmaster, the parents were persuaded to send their child to the town to be cured by me. She was, at the judge's expense, boarded in a sensible family, and all visitors were interdicted. Only one aunt, a well-informed, sensible woman, was allowed to keep her company. In the space of a few days, although I had as yet done nothing, the attacks were milder and shorter, the child had quieter nights, and, compared with the low, close, hot room of her parents, found herself very comfortable in the high airy apartment which she now occupied. I followed in my cure the same course as in the first-mentioned case,—aperients, carbonate of iron, quinine, baths, douches, cold immersions, and strychnia were employed by me. I succeeded in bettering her condition, but after short intervals the St Vitus' dance always returned, not certainly with the exaggerated character it at first manifested, under the tailor's manipulations, but still with considerable severity. The child became worn out, turned somewhat weak in her memory, and I feared that, from the consequences of the continued convulsions, she might settle down into idiocy. The afore-mentioned judge then besought me to invite the pastor's widow to try here the same means which had cured her own child. After some hesitation,—for it was certainly asking much of a doctor to seek counsel of an old woman,—I determined to do so. Meanwhile, I was myself curious to prove the effect of this remedy, of whose harmlessness I at least believed myself to be persuaded, and then I had the hope of learning from the good woman the nature and qualities of her nostrum. I delivered up to her the cure of the child; she gave her a somewhat bad smelling drink, but which did not taste badly, and the child was obliged to take a full glass of this every day; besides this, she rubbed on her stomach a greasy liniment of an

equally unpleasant smell. After each time of taking the medicine, the girl had to walk for an hour up and down, but the rest of the time she remained in bed. Already, on the third day, the attacks became strikingly weaker, and after perhaps fourteen days she was completely cured. Not the slightest convulsive movement remained behind. The girl was able to walk a long way, had a good appetite, and slept well, became lively and vigorous again, and has been healthy ever since. I begged the pastor's wife to give me, for the good of mankind, more particular information regarding her secret remedy. She was a pious woman, and did not hesitate to do so. She told me that many years ago she had become acquainted with the remedy through another pastor's wife, whose mother was a Pole, and who had herself in her youth been cured by it of epilepsy. The remedy consisted of mugwort, *brick* oil, bone oil, assafœtida, and castor, the latter two ingredients she got at the apothecaries', the mugwort she gathered herself, and the brick oil and bone oil she was obliged to prepare each time. For the bone oil she mixed hartshorn, horse's hoof, and dog's bones —all well cleansed and dried—of each an equal quantity, put them into a deep pan, laid another of the same size on the top of it, cemented them both closely and firmly together with clay, and then exposed the whole for many hours to a strong heat. The united cooking apparatus was allowed to cool, and when it was quite cold the pans were separated. She then took from it a brown oil, which had a singed smell—this was the animal oil. The brick oil she prepared by letting pieces of brickbat lie for several days in old olive oil, and then frying it. As for the mugwort, the herb is to be taken when it can be had fresh and good, but when this is not the case, the root must be used. It must, however, be gathered only in autumn; but of this root only the lateral yellow radicles are to be used, and they must be carefully dried in the shade. If the herb is used, take a pound of it while fresh, pour six pounds of river water over it, and let it stand covered up in a cool place, then strain off the liquid, at the same-time expressing the herb; then put into a separate pot the yolks of six eggs, four ounces of assafœtida, one-half ounce animal oil, and one-half ounce brick oil, rub altogether until it becomes homogeneous. This mixture is to be added to the mugwort water, with honey and sugar according to taste. If the root, not the herb, of the mugwort is used, it must be powdered or contused, and remain ten days lying in water before it can be used. The patient must drink from a wine glass to a cupful of it three or four times a day; to each cup or glassful must be added from fifteen to twenty drops of tincture of castor. The remedy, therefore, consists of a very powerful infusion of *Artemisia vulgaris*, with assafœtida, oleum animale Dippelii, empyreumatised olive oil, and castoreum. The woman assured me that in cases of young persons of both sexes, from the ages of eleven to seventeen, who were affected with spasms, she had seen the most excellent effects resulting from the use of it, whether the spasms consisted of epilepsy, St Vitus' dance, or somnambulism."

Subsequently, the author had an opportunity of testing and proving the success of the remedy in his own niece, a well-grown girl of thirteen, who was affected with chorea and somnambulism. The formula which he used, which retains all the essential ingredients of the widow's nostrum, was the following:— A pound of the tops of *Artemisia vulgaris* was digested for three days with eight pounds of water, and strained. To the liquid was added three ounces of assafœtida, with enough of gum-arabic to make a perfect mixture [this we believe might be omitted], and of syrup to sweeten it. This was given in teacupfuls, and to each dose was added eight or ten drops of a mixture of two parts of Dippel's oil, with one of tincture of castor. Subsequently, the Dippel's oil drops were omitted, as they were thought to cause headach and foul tongue, and the infusion of mugwort with assafœtida was given alone. The disease speedily gave way under this treatment, the chief manifest physiological effect of which was copious diaphoresis. Menstruation occurred after about a week of this treatment, and the girl speedily acquired perfect health—*Journ. für Kinderkrankheiten*, Jan. and Feb. 1851.

[In the first of these cases it should be observed that the disease diminished after a change of locality, and when the excitement to which the patient had been exposed was removed. In the second case, menstruation appeared a week after the commencement of the remedy, which doubtless had the chief share in producing the cure.]

SIMPLE METHOD OF PRESERVING EXTRACTS OF VEGETABLE JUICES. BY M. STANISLAS MARTIN.

The author had previously recommended to naturalists, who wish to preserve the juices of plants, to concentrate the watery part rapidly by mixing them with sand or powdered glass, and thus exposing a large surface to the air. This proceeding suggested another, which is more simple, more expeditious, and more portable for a traveller.

It consists in bruising the plant, expressing the juice, soaking in it fabrics of linen hemp or cotton, and then drying them by exposure to the air.

As soon as the cloth dries it is to be moistened again with the juice, and again hung up to dry, and this is repeated till the cloth becomes somewhat hard. He has found that a square metre (1 yard 3 inches) can imbibe a kilogramme (2 lbs. 8 oz. Troy) of active matter, which is more than sufficient for a chemical analysis.

Cloths, covered with vegetable extract, are generally hygrometric,—it is therefore proper to preserve them in bottles or boxes, which are air-tight.—*Bulletin de Thérapeutique*, 15th September 1851.

POISONING WITH ARSENIC. BY M. BARSE.

The important question of the distinction between arsenic furnished by arseniferous soil, and that used in the arts, and criminally for attempts at poisoning, has been recently carried before the Court of Assizes of Lot and Garonne.

A man, Goubinel, and his wife, were brought before the court for a double crime of poisoning, committed by both conjointly, on the person of the former husband of the one, and on that of the former wife of the other. The remains of the former husband, Venaud, interred for three years and one month in the cemetery of the Commune of La Maurelle, were exhumed, as well as those of Jeanne Touron, the wife of Goubinel, interred for eighteen months in the cemetery of Cazeneuve. The coffin of Jeanne Touron was found perfectly intact; the body had been enclosed in it, clad in numerous petticoats and body clothes. The coffin of Venaud, interred for a longer time, was not in the same state of preservation. The decayed boards were broken up in several places; a portion of earth was found in contact, and to some extent mixed, with the detritus of the body which they contained. The experts subjected to analysis the remains of Jeanne Touron, and the presence of arsenic was manifested. Those of Venaud, submitted to the same experts, gave a like result. It has been already said, that the remains of Venaud were mingled more or less considerably with the adjacent earth, from which it was impossible to disengage them completely. The experts analysed a portion of that earth pure, and to their astonishment it also gave arsenic! The experiments were repeated, and the analysis arrived at the same conclusion,—that the earths of the cemetery of La Maurelle were arseniferous.

This result involved the necessity of analysing also the earth of the cemetery of Cazeneuve, where Jeanne Touron had been buried; they were no less arseniferous than those of the cemetery of La Maurelle. The experts were thus disconcerted as to their researches. However, after having been assured that the coffin of Jeanne Touron was in perfect preservation, that the body had been preserved from contact and admixture with foreign matters, that all the articles interposed between the earth and the body did not contain arsenic, especially the planks of the coffin, the winding-sheet, and upper part of the

clothing, with which they had been buried, they came to the conclusion that it was a case of poisoning; but so far as concerned Venaud they ventured only to express a doubt. It was under these circumstances that the procurator of Villeneuve thought it necessary to appeal to the scientific knowledge of M. Barse. According to M. Barse, these toxicological difficulties have no real existence. The presence of arsenic in the soil cannot form an obstacle to the certain verification of poisoning by arsenic, science having the sure means of distinguishing between free or pharmaceutic arsenic and that of the soil. Thus arsenic of the arseniferous earths is not in a soluble state; on the contrary, it is combined with other minerals, such as iron, cobalt, antimony, &c. Its separation is very difficult, and does not take place, whatever may have been said upon this subject, either under the action of potash or of sulphuric acid. M. Barse has treated the earth of the cemeteries of the Maurelle and Cazeneuve with these agents; the arsenic remained insoluble; it only yielded to the combined action of sulphuric acid and heat pushed to 300°. Now such combination of agencies does not occur in nature. Therefore, if bodies contain arsenic, the arsenic is proper to them, and not communicated *ab extra*.

Discussing successively these facts, M. Barse remarked, relative to the body of Jeanne Touron, that the coffin in which it was enclosed had been found perfectly close and preserved; that the winding-sheets and coarse clothing with which, contrary to usual custom, this woman had been buried, were perfectly intact. But since the arsenic of the soil is insoluble, it could not reach by infiltration, through a close coffin, to the body. If, then, the body furnished any, it was because it was proper to it. But even admitting the solubility of the arsenic of the soil, and the possibility of its communication to the body by infiltration, it must, before reaching the body, have permeated the wood of the coffin, the cloth of the winding-sheet, and the thick *stuff* of the vestments. But analysis proves its absence from these various objects. Then that which was found in the body was proper to it, and could not have been communicated by the soil, since the intermediate objects did not contain it.

Therefore M. Barse confirms and adopts the conclusions of the first experts upon this point: Jeanne Touron, the wife of Goubinel, had taken poison during her life, and in sufficient quantity to cause death. With regard to the body of Venaud, there was a farther difficulty. The interment dates back upwards of three years, and the coffin, which was broken up, had in some places allowed a little earth to penetrate. M. Barse undertook fresh experiments upon parts of the remains which appeared exempt from all admixture, and upon parts of the bones carefully freed from all foreign matter. The presence of arsenic was manifested. But what was the origin of that poison? Might it not be believed, justly, that in spite of the precautions taken, some foreign matter did remain mixed with the organic detritus? Might not the presence of some foreign element falsify the results of the experiments? Upon the principle of the insolubility of arsenic of the soil, the question of origin can always be determined. The arsenic procured from the remains of Venaud is easily soluble. Therefore it is pharmaceutic arsenic.

However, to arrive at an exact and rigorous decision, new experiments were made. Bodies of the same date as that of Venaud, and as closely contiguous as possible, were exhumed. Taken in the same conditions and submitted to analysis, they did not furnish arsenic. That of Venaud alone in the cemetery of La Maurelle yielded any. This was the first confirmation, but it was not all. M. Barse felt the necessity of submitting it to new investigations. In Venaud's house, and in the chamber where the unfortunate man died, he found around his bed the probable place where his vomitings had fallen; for Venaud, before he died, had had violent attacks of vomiting. A portion of the wall, nearest to the bed, bore on its lower part numerous stains similar to those of splashes of dirty water. When these had been observed the planks nearest to the bed were diligently scraped, as well as the portion of the neighbouring wall which appeared splashed. As a means of comparison, various other parts

of the planks and wall were equally scraped in different places. On analysis the scrapings of the planks taken near the bed gave arsenic; the scrapings of the same planks taken in other places gave none. Submitted to the same experiments, the scrapings of the wall taken near the bed, and which appeared splashed, gave arsenic; those taken in other parts gave none. But it might be alleged that certain soils of that country are arseniferous, and it is not impossible that the presence of arsenic on the planks was the result, for example, of the contact of the deposit of mud. The question of origin is again at once cleared up by the principle of the insolubility of arsenic of the soil. But that which was taken from the scrapings is soluble,—therefore it is pharmaceutic arsenic. With these new elements M. Barse did not hesitate to decide the difficulty left undecided by the former experts. Venaud had absorbed arsenic during life in sufficient quantity to cause death. Goubinel and his wife were found guilty by the jury, with extenuating circumstances, and were condemned to hard labour for life.—*L'Union Médicale*, 27th September 1851.

Part Fifth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING VII.—*July* 19, 1851.—Professor BENNETT, P., in the Chair.

COMMUNICATIONS.

CANCROID GROWTHS.

1. *Dr Bennett* made some remarks on the existence of a class of tumours to which the name of *cancroid* was applicable, inasmuch as they very closely resembled true cancer in their more apparent characters, and often also in their tendency to a wide-spread distribution, while structurally they were very different from the ordinary types of cancerous growths. He thought it a matter of the utmost importance, that structures which differed so much from each other should be studied with regard to their symptoms, progress, liability to return after excision, and susceptibility of undergoing spontaneous disintegration, instead of being, as they are at present, all confounded together under the term malignant. It had been stated by Müller that enchondroma was not liable to return, but he had recorded a case in his work "On Cancerous and Cancroid Growths" (Obs. 48), where a cartilaginous tumour of the humerus had been amputated, and yet where a disease apparently similar had returned in the axilla and neighbouring parts, and destroyed the patient. Unfortunately this secondary growth had not been examined microscopically, so that, strictly speaking, it had not yet been clearly demonstrated that enchondroma was malignant, although the case in question rendered this probable in some instances. He considered there could be no doubt that epithelial growths were occasionally susceptible of being propagated to the neighbouring glands, of ulcerating and producing death exactly in the same manner as tumours really cancerous. He believed that, as exact observations extended, other forms of structure differing from cancerous, cartilaginous, or epithelial growths, may be found also malignant in the usual acceptation of that term. In proof of this, he related two very remarkable cases, in both of which

death had been occasioned by apparently an encephaloma, in one instance attacking the lung and in the other the ovary.

The first case was recorded in Obs. 43 of his work, "On Cancerous and Cancroid Growths." It was that of a young woman, who had a fungous tumour of the leg, which was amputated by Mr Norman, of Bath, the stump healing without accident. Dr Davies of that city had sent Dr Bennett a portion of the tumour, the structure of which is described and figured in the work referred to. It consisted "of fusiform corpuscles in different stages of development, mingled with naked nuclei, a multitude of molecules and granules, and a few blood globules. Some corpuscles were of oval form, others elongated and caudate, others truncated at one end, or spindle-shaped. None exceeded the 1-200th of a millimetre in breadth; and in length they varied from the 1-100th to the 1-25th of a millimetre. Several contained a minute granule, about the 1-500th of a millimetre in diameter. On the addition of acetic acid, most of the loose molecules and granules were dissolved, and a faintly fibrous structure produced, in which oval bodies were scattered, varying, in their longest diameter, from the 1-100th to the 1-50th of a millimetre."

The structure in consequence in no way resembled cancer; yet two years afterwards disease of the lung commenced, and she died with all the symptoms of cancer of that organ. On examination, an encephaloid tumour of the lung was found, a portion of which was kindly sent to Dr Bennett by Dr Davies, who with Mr Norman attended the case, and on microscopic examination it was found to possess exactly the same structure that was previously described as forming the tumour of the leg.

The second case was that of a woman who died of encysted tumour of the left ovary, a portion of which was sent to Dr B. by Mr Russel of Birmingham. It exactly resembled encephaloma, being pulpy in consistence, white in colour, and yielding a copious milky juice on pressure. The other ovary presented an incipient condition of the same morbid alteration. On microscopic examination, it was found to consist of masses of columnar epithelium, which had undergone more or less of the fatty degeneration, and were loaded with oil granules. These were arranged side by side, and frequently presented in the juice, squeezed from the surface of the growth, a concentric arrangement, which would be best understood by examining the drawings exhibited. Associated with this epithelium was a multitude of diaphanous bodies, varying greatly in size,—in some of which oil granules had also accumulated, so that many of them presented all the characters of compound granular corpuscles. No trace of cancer cells could anywhere be discovered. Of this singular form of growth Dr Bennett had now seen two other examples, both of which had occurred in the ovary.

In conclusion, Dr Bennett stated that it was only by multiplying such observations, and determining, in the first instance, the structure and progress of the different forms of malignant growths, that we could ever hope ultimately to arrive at the mysterious laws which governed their origin and propagation.

In the conversation which followed, *Dr Robert* of Marburg (visitor), and *Dr W. T. Gairdner*, made remarks; and it was agreed to take up the important subject of cancer, and malignant growths generally, at a special meeting, on some future occasion.

EXPERIMENTAL INQUIRY INTO THE EFFECTS OF VARIOUS ANIMAL MATTERS INJECTED INTO THE BLOOD.

2. *Mr Millington* laid before the Society the following experiments and remarks:—By the invitation of the President, I am induced to bring before the Society the results of some experiments that I have made, to determine the effects of certain animal fluids on the blood. The Society is aware that the formation of numerous small abscesses in the viscera, especially in the lungs and liver, and the dangerous constitutional symptoms that accompany them, so frequently observed after operations, parturition, and diseases attended with suppuration, have of late been pretty generally referred to the entrance of purulent matter into the blood.

But the manner in which pus acts in these cases has been much disputed. The numerous disseminated abscesses of the viscera have been usually explained by the mechanical arrest of the pus globules in the capillaries of those organs. This opinion is based upon the well-known physiological fact, that foreign substances, entering the blood, and failing to be discharged by the natural emunctories of the body, collect, as on a filter, in the capillaries of the lungs or liver, and give rise to inflammation there, for their elimination. This view was thought to be established by the experiments of Cruveilhier, who introduced metallic mercury into the veins of living animals with the effect of producing in the lungs or liver numerous small abscesses, each containing a small globule of mercury, according as the metal was introduced into the general or portal circulation. Mr Lee, of London, was the first to call in question this mechanical explanation of the *modus operandi* of pus; and he has the merit of first demonstrating, by actual experiment, that when pus is added to living blood it causes its speedy coagulation. Mr Lee expresses the opinion, that this effect of pus on blood is most marked when the pus is in a fœtid state; and that, when introduced into the system, it acts by imparting its own condition to the blood.

From a sense of the practical importance of the subject, I have availed myself of the extensive opportunities the Royal Infirmary affords for prosecuting an inquiry into the pathology of the disease; and with the view of guarding against many fallacies incident to all investigations of this kind, I have made numerous experiments, not only with pus, but with various animal and vegetable fluids, &c. It would of course be here quite out of place to enter into the details of all my experiments; and I shall therefore content myself with some general remarks, and bring before the Society an experiment performed at 7 A.M. this morning, which shows the effects of fresh pus, putrid animal fluid, and bile, on the blood. The particulars of the experiment are briefly as follows:—The integuments of the neck of a living animal having been quickly reflected, one of the jugular veins was readily opened, and six ounces of blood were allowed to flow into a vessel containing half an ounce of unequivocally fresh pus; the same quantity of blood was received into a second vessel, containing half an ounce of moderately putrid animal fluid; a similar amount of blood was received into a third vessel, containing half an ounce of bile. The contents of the vessels were now gently stirred with glass rods, and the changes observed in the blood were carefully noted. That to which the pus had been added began to coagulate almost immediately, in a manner which is quite peculiar, and which I have had repeated opportunity of witnessing. Its colour also underwent a change, becoming slightly darker. The blood to which the putrid fluid had been added became immediately very dark, but did not begin to coagulate until after the lapse of some minutes. The blood which had been mixed with the bile was quickly changed to a brownish-black colour, but it evinced little tendency to coagulate, though it remained for hours undisturbed.

The Society will see, in the blood placed on the table, a corroboration of these statements. In this vessel, containing the blood to which pus has been added, it will be seen that there is a firm crassamentum, which has a peculiar striated appearance, owing, as the demonstration now under the microscope shows, to the aggregation of the pus globules. The crassamentum often contains in its interior small rounded masses, harder than the remaining part of the clot, and which, when examined by the microscope, are also found to consist almost entirely of pus globules. These effects are characteristically produced when the pus is quite fresh; and, judging from my own experiments, there appears no doubt that putrid matters of all kinds retard coagulation; and that the crassamentum formed by blood to which putrid matters have been added, possesses certain distinguishing properties to be now noticed. In this vessel, which contains the blood to which the putrid animal fluid has been added, the crassamentum is throughout of a very dark colour, of uniform consistence, but soft, and readily broken down by the finger. I have not examined this specimen of blood with the microscope, but in others I have frequently been unable to detect any change. In some instances

I have, however, noticed the diminution, and in one case (where the matter added to the blood was extremely putrid) the almost entire disappearance, of the coloured globules, the colouring matter being generally diffused. The colourless corpuscles appeared always unchanged. In the third vessel, which contains the blood to which the bile has been added, it will be seen that though only half an ounce of bile was added, it has been sufficient to prevent any other than an almost diffluent crassamentum from being formed,—an effect probably due to the soda of the bile.

Such being the effects of pus and putrid fluids on the blood, the question arises,—What hypothesis will give the most natural explanation of the symptoms and morbid phenomena of the disease now commonly known by the name of Pyohæmia? M. D'Arcet has supposed that it is owing to the liquor puris becoming purulent after it has reached the lungs; but M. Sédillot has shown, that the serum produces no ill effects, when free from putrefaction; and that when putrid, though local diseases are induced, they are *not* of the nature of visceral abscesses, but of gangrene. M. Andral has referred this disease to a decomposition of the serum and the action of ammonia on the blood, rendering it dark and dissolved; but when the pus is *fresh*, it appears to me very difficult to understand how the animal matter can be converted into ammonia. And, further, the action of pus on blood is *not* to render it dark and dissolved, but, on the contrary, the crassamentum is firmer than natural. The changes, supposed by Andral to be owing to the entrance of pus into the circulation, are the result, as I have shown by repeated experiment, of the entrance of putrid matter into the blood. A careful perusal of the experiments of M. Gaspard will satisfy any one, that putrid matters exert their action principally on the mucous membrane of the intestines, giving rise to frequent, dark, and extremely offensive evacuations, usually attended with decided relief, and in some instances with entire disappearance of all the symptoms. The opinion of Rokitansky, that pyohæmia is to be referred to a spontaneous change in the blood, is quite untenable, as we have no evidence that the blood ever becomes diseased spontaneously. The opinion of Mr Lee, that pus imparts its own condition to the blood, is probably nearer to the truth; but, from a careful observation of the symptoms of the disease, from numerous experiments, and from the number of *post-mortem* examinations that I have attended, I have been irresistibly impressed with the belief, that the effects which follow the introduction of pus into the system are such as can be best understood by considering that it acts as a poison; and I think its resemblance to other animal poisons in its mode of action can scarcely fail to arrest attention. In many of them we have first a local disease (in the case of pyohæmia, this may be a stump after amputation, a diseased bone, that part of the uterus to which the placenta was attached, &c.), upon which secondary affections supervene in the form of violent inflammation in one or more parts of the body. In addition, there is a severe constitutional affection, which is characterised by a peculiar fever.

The opinion, that referred the secondary abscesses in the viscera to the mechanical arrest of the pus globules in the capillaries of those organs, was based upon the supposition, that the globules of pus were larger than the colourless corpuscles of the blood; but the best proof of the erroneousness of this opinion is to be found in the impossibility of distinguishing the pus globules from the colourless corpuscles of the blood. M. Sédillot has indeed stated that they might be distinguished; but the concurring testimony of all subsequent observers shows that he was mistaken. The experiments of Mr Gulliver appear to me to throw much light on the formation of these visceral abscesses. He found that the introduction of pus into the cellular tissue caused suppuration there more surely than foreign bodies of the most irritating kind; and he expresses the opinion, that it determines the nature of the subsequent exudation. I have witnessed nearly the whole texture of the liver destroyed by abscesses, the result of absorption of purulent matter from the interior of the colon; whilst the other organs of the body, after a careful scrutiny, were found free from all appearance of the same disease. *This, and*

numerous facts of a similar kind, show that the whole of the pus absorbed from the ulcerated intestine never passed beyond the capillaries of the liver, but was arrested there, not by any mechanical laws, but owing to alterations in the vital properties of the blood itself, which determined the nature of the subsequent inflammation; and it is worthy of especial observation, that whilst idiopathic inflammation of the same organs ending in suppuration is attended with *hectic* fever, those resulting from animal poisons are attended by a fever which is essentially *typhoid;* and we have good reason to think that this peculiarity of the effect of inflammation depends on a deviation from the natural state of the vital properties of the blood.

In conclusion, I would remark that while the history, symptoms, morbid appearances, and analogies of the disease with other blood diseases, arising without doubt from animal poisons, tend to establish the fact, that purulent matter acts as a poison when received into the blood; yet we cannot conclude, as Mr Gulliver has done, that a small quantity of pus, whether in the blood or in a serous cavity, acts like a leaven, and speedily leads to the production of a great deal more; because it has been proved by repeated experiment that small quantities of pus may be injected into the blood without occasioning any ill effects. We must therefore conclude, that the poisonous action of pus on the blood is different from that of any other known poison; and that it requires, for the development of its poisonous action, that it should be introduced into the circulation in quantities sufficient to alter the natural state of the vital properties of the blood.

ON THE SYMPHYSIS PUBIS AND ITS CONTAINED CAVITY.

3. *Mr Zaglas* laid the following observations before the Society:—

The connection of the innominate bones at the symphysis pubis having uses which are intimately connected with the mechanism of support of the spine, I beg to call the attention of the Society to that part of the body,—for the few remarks which I am going to enter upon are in some measure supplementary of the general sketch already given of the relations between the pelvic bones, although they may not be entirely devoid of interest, when considered also independently of this circumstance. It is of the central cavity of the symphysis pubis that I have chiefly to speak; and I have to state the result of upwards of thirty observations, made for the purpose of ascertaining whether this cavity is, as commonly stated, an occasional or exceptional state depending in women on parturition and its consequences, or a state uniformly present in both sexes, and at all ages.

In the approximate number of subjects mentioned above, I have met with no exception to the presence of this cavity; upon which ground I may be justified in advancing, that it is a general and normal appearance for every age and for both sexes. It is true that, owing to the shortness of time since accident first induced me to pay attention to the subject, I have been able to make but a comparatively small number of observations, and I must add that, concerning the ages, the youngest subject that presented itself was a girl about ten years old. But then we must consider that, notwithstanding the want of instances in the earliest periods of life, and the smallness of the total number when subdivided into the separate amounts for every variety of sex and age, the constancy of the appearance throughout even these varieties, almost establishes the validity of the assertion.

On the present occasion I shall abstain from saying anything on the structure of the symphysis pubis, reserving this for another opportunity, when I may speak of it in connection with other subjects. And I shall at present say only a few words on the position and the extent of the pubic cavity. It is hardly deserving the name of a cavity, for it is but a slit between two opposed epiphysal cartilages of the pubic bones. It has, therefore, no transverse dimension, and extends through the symphysis pubis only lengthways and antero-posteriorly, measuring an inch or somewhat more in the former direction, and about four lines to half

an inch in the latter. When the symphysis pubis is sawn across transversely, the slit is perceived on the posterior half of the surface of section; for from the pelvic cavity it is separated only by a thin layer of fibrous tissue, while anteriorly the layer of fibrous and fibro-cartilaginous tissue is very considerable. Often the slit extends still further towards the back, owing to a protrusion of the posterior margins of the above-mentioned cartilages into the pelvic cavity, where they give rise to a more or less elevated ridge. This ridge, which sometimes is straight, sometimes serpentine, is then externally bridged over by the fibrous layer above-mentioned, and internally divided in two halves by means of the slit.

In the interior of the pubic symphysis, pus or other morbid deposits are found occasionally, contained in a cavity, chiefly after a fatal issue of labour. Such cavities are not to be confounded with the described one, although hardly a doubt can be entertained that the morbid deposits frequently occupy the same position. The parietes of the described cavity present no aspect of ulceration, nor anything like a morbid appearance, though occasionally a little detritus may be found contained in it, and consisting of patches (epithelium? layers of cartilage?) detached from the walls, which is easily accounted for by the constant attrition which they have to sustain, and is similar in character to the appearances met with in other articulations.

Remembering in what way the spine rests upon the innominate bones, the usefulness of such a construction in the symphysis pubis becomes apparent, although it would not seem to be one absolutely necessary, for a slight mobility of the two pubic bones, such as a connection by means of dense fibro-cartilaginous tissue would allow, appears sufficient for the purpose. When, for instance, the weight of the body is thrown forwards, and the base of the sacrum wedging itself betwixt the innominate bones, pushes them outwards, these latter bones are in the condition of two levers undergoing a great strain; the fulcra of these levers being the acetabula, the forces being applied opposite each side of the sacrum above, and at the points of contact of the two pubic bones below, where these bones are pressed against each other.

The symphysis pubis may, therefore, be considered as the common point of application of forces acting on two lateral levers (the ossa innominata); and it is clear that in so far as these levers admit of motion, that motion must be communicated to, and represented at, the symphysis, in the form of a change in the angle of the pubic bones towards each other. (The cartilaginous ridge of the symphysis, considered in the first cause of its protrusion, would imply that such a tendency towards motion, or even actual motion, takes place in a plane parallel to the superior outlet of the pelvis; but that this is neither the only nor the most important direction of the motion, I am satisfied, both from careful consideration of the complicated mechanism of the whole, and from numerous experiments and measurements, which I can but mention here, because entering into the necessary details would lead far off from my present purpose.) Here the usefulness, we may say the necessity, of a symphysis pubis impresses itself upon our consideration, and to render it more apparent, I may, in addition, allude to the consequences of a connection of the pubic bones, by means of a continuous osseous substance. There would be an osseous lamina, comparatively thin and long, stretched between the two acetabula, and receiving the whole weight of the body transmitted from them to its central point, in a direction very favourable for fracture. It should be considered, that the weight of the body may be here applied at a very great mechanical advantage, and so as to exceed very much its original amount of force, by means of the leverages which result from the different postures of the body. It is impossible to conceive how, under these circumstances, such a lamina could escape fracture, even in young persons. Moreover, I believe there is no instance where nature has trusted to the osseous substance for a resistance of the kind.

But there is no such imperious necessity for the existence of a pubic cavity or slit. The usefulness, however, cannot be denied, especially if the slight pubic motion be considered as assuming the direction which I have more specially alluded

to. That it subserves an important purpose, however, is attested by the generality of its appearance, of which fact the following is a good illustration.

I here show to the Society a section of the symphysis pubis, from a young female, about eighteen years of age, and unmarried. On removing the symphysis pubis with a small portion of the pubic bones (which I did while the body was quite recent, through the kindness of Dr Gairdner, and in his presence), our attention was directed to the fact, that the articulation appeared hardly moveable. Yet, even in this instance, you will observe that the slit or cavity above-described is perfectly developed.

4. *Dr Bennett* called the attention of the Society to the recent observations of Donders on the movements of the brain, produced by circulation and respiration in the fœtus, or in the opened skull of an adult animal. Professor Donders had clearly shown, by a most ingenious experiment on the living animal, the absence of all movements in the perfectly closed skull; a result which might, indeed, have been anticipated, and which was in perfect harmony with the observations made in Edinburgh, long ago, by Monro and Kellie, who considered the nervous substance and its fluids as completely filling up the cranium with incompressible substances, to which, therefore, no real general movements of the kind alluded to could possibly be communicated when the skull was entire. Dr Donders was also of opinion, that because during partial asphyxia he was enabled to see increased redness of the cerebral membranes, that therefore the quantity of blood in the brain was increased. But this opinion was in no way proved to be correct by the experiment, which only showed that *certain* vessels were congested, which congestion was probably counterbalanced by diminished capacity of other vessels, deeper seated. (See "Monthly Journal" for August 1851.)

MEETING VIII.—*July* 26, 1851.—Professor BENNETT, P., in the Chair.

COMMUNICATIONS.

1. *Dr Bennett* submitted to the observation of the Society the blood, and a portion of the spleen, of the man Tinlay, whose case had been previously narrated by Dr Bennett in the "Monthly Journal" as a well-marked example of leucocythemia; and who had lately died at Hull, under the care of Dr Sandwith, by whom the diseased parts had been transmitted to Edinburgh. The spleen was very greatly hypertrophied, and, like the blood, contained an immense proportion of white corpuscles, of the type usually found in such cases. The clots of blood in the heart presented a peculiar opaque granular appearance, as if mixed with pus; the microscopic structure of the fibrine also strongly resembled that observed in some inflammatory exudations, from the large number of corpuscles mixed with it. (This communication is minutely detailed in Dr Bennett's paper, in the "Monthly Journal" for October 1851.)

2. *Mr Drummond* communicated the details of another case of leucocythemia, which had occurred in the hospital. The case was one of complicated disease,—the liver being much atrophied and tuberculated, the kidneys affected with Bright's disease, and the spleen much enlarged, soft, and containing a mass of yellow friable deposit; the patient, a male, aged about 60, was very intemperate, and died of these diseases with ascites and anasarca. The excess of white corpuscles in the blood was well-marked, and the spleen contained a large number of similar corpuscles, as well as some more composite bodies, of large size (up to $\frac{1}{30}$th min.), and containing numerous internal nuclei. (This case is also detailed in Dr Bennett's paper, as above cited.)

3. *Dr W. T. Gairdner* took the opportunity of adverting to the existence of a peculiar appearance in the blood of leucocythemia, and in very many other pathological structures (such as the pus of abscesses, &c.) In such cases there

were often found very pale transparent vesicles, perfectly simple and homogeneous in structure, which sometimes occurred floating freely in the fluids, and sometimes were found to form an external envelope to pus corpuscles (in which latter connection they had been figured by Dr Bennett several years ago). Dr Gairdner exhibited many drawings, which he had at different times made of these bodies in all their relations, and entered at length into various speculations suggested by them as to the formation of the cell-wall in pathological and physiological structures. His views on the subject had been stated to the British Association of Science in 1850, but not published in detail; and he now observed, that much controversy had arisen in Germany in regard to these bodies, which had been independently observed by Bruch and others in a great variety of situations.

A lengthened debate followed in which Dr Bennett, Mr Goodsir, Dr Hannover of Copenhagen, and Dr Gairdner took part.

4. *Dr Hannover* of Copenhagen laid before the Society various preparations illustrative of his published views of the structure of the vitreous humour of the eye in the different orders of the mammalia. The preparations were made by hardening the vitreous humour in chromic acid, which enabled its laminated structure to be seen distinctly on section.

Mr Cobbold said, he had examined the vitreous body after the manner pointed out by Dr Hannover. The great difficulty he had in regarding the striated appearance in the preparations on the table to be of a membranous character, arose from the fact, that when a single puncture was made through the hyaloid into the vitreous mass in its recent condition, after a few minutes, the fluid contents escaped, and all that we had left was its external tunic and a few septa or membranous prolongations of the hyaloid, constituting but a fiftieth part of the bulk of the vitreous body. It therefore appeared to him impossible that the stratified appearance could be of membranous origin, and he was strengthened in this conclusion by observing that in all instances where this character had been demonstrated, chromic acid or some other coagulating re-agent had been employed.

Publications.—The President, Vice-Presidents, and Secretaries were authorised to superintend the publication of the reports of the past summer Session.

In adjourning the Society till Saturday, November 15th, the President congratulated the members on the success which had attended their meetings. He trusted that during the vacation new facts and observations would be accumulated, which would render their future *réunions* as instructive and interesting as those which had just concluded.

EDINBURGH OBSTETRICAL SOCIETY.

Session X.

Meeting VI.—*June* 25, 1851.—Dr Simpson, President, in the Chair.

Dr Simpson communicated a case, and showed a preparation, of gangrene of the placenta. The child was born alive.

He gave the details of an instance in which he had induced premature labour upon Kiwisch's plan, by injecting tepid water into the vagina. Labour supervened in about twenty-four hours. The fœtus was found presenting by the head at the commencement of the injection; but, when labour at last came on, it had turned, so that the feet and breech came down.

He showed two polypi, which he had removed a few days before, by a new kind of instrument,—viz., a hook, like Dr Ramsbotham's decapitation instrument. One of these polypi was very large, and when the stalk was divided by

the instrument in question, the polypus required to be removed from the vagina by a pair of forceps.

SPURIOUS MENSTRUATION IN NEWLY-BORN INFANTS. BY DR MATTHEWS DUNCAN.

The cases which *Dr Duncan* was about to relate to the Society were of very rare occurrence. All forms of hemorrhage from mucous surfaces in newly-born infants were very seldom met with. Cases of epistaxis, of hæmatemesis, and of bloody evacuations from the bowels, did occasionally occur within the first few days after birth; and not a few such cases had been recorded by various authors, who, however, were very rarely found to allude to the form of hemorrhage under consideration.

Authors had attempted to account for all these forms of hemorrhage at so early a period of life by ascribing them to the great and rapid change produced in the child at birth by passing from the condition of an intra-uterine fœtus, with a placental circulation, to that of an infant with a pulmonary circulation, and respiring air. If this were ever the cause, it certainly was not so always. For in many cases there was satisfactory evidence that the bleeding had commenced while the child was still in utero, and consequently before the change above mentioned had taken place.

The only case Dr D. had seen occurred in a fine, strong, healthy child, the first-born of its mother. It was a case of head-presentation, and the labour was easily terminated. The mother was a young healthy woman, of sanguine temperament. In her, menstruation recommenced four weeks after delivery, and recurred regularly during the whole period of nursing. The vaginal discharge in the infant appeared the day after its birth, and had all the appearance of menstrual blood. It continued for six days, every napkin on removal bearing a large stain of the blood. About the fourth day the mammæ began to swell and inflame. After the vaginal discharge ceased, the swelling of the mammæ disappeared.

The following case occurred recently in the practice of Dr Cunningham of Currie, who kindly communicated it to Dr D. The child was born naturally and easily, the head presenting. It was two days old when the discharge from the vagina commenced. This appeared to be venous blood, from the colour, and was in considerable quantity (perhaps a drachm every time the cloth was removed). It continued for four days. The mammæ of the child were enlarged and indurated at the time; but when the discharge stopped, the swelling disappeared.

The third case of bloody vaginal discharge (see *Gaz. Méd.*, 1835, p. 248) occurred in a strong child, born of a healthy young woman. After it was three days old, a slight loss from the genital organs was observed, the blood flowing drop by drop. In this way it continued for three days, and then became more and more watery. On the fourth day, some drops of pure blood came away, and then the discharge ceased. It had the appearance of menstrual blood. Five days afterwards the mammæ became inflamed. Subsequently the infant had good health.

These cases seemed to arise, not from any extraordinary circumstance in the labour, or disease in the infant, but were apparently a consequence of sanguineous engorgement of the sexual system of the newly-born infant.

In all the cases the labour was easy. The children were born with the head presenting. In none of the cases did the breech of the child suffer any violence. No disease existed in mother or child; nothing morbid was discovered in the genital organs of the children. The discharge flowed for from four to six days, and then ceased spontaneously. After the vaginal discharge, there supervened engorgement and inflammation of the mammæ; and although engorgement of the mammæ of both male and female children is of frequent occurrence, still its coincidence with the vaginal discharge in these cases, and its apparent connection with it, could scarcely be looked upon as merely accidental.

VARIETIES.

RESOLUTIONS OF THE FACULTY OF PHYSICIANS AND SURGEONS OF GLASGOW.

At a meeting held 1st September 1851, the following minute was passed:—"The Faculty, having had their attention called to certain resolutions by the Royal Colleges of Edinburgh, in reference to homœopathy, and heard the opinion of their Council, to whom the matter was referred at last meeting, do now, in conformity with that opinion, express their concurrence in, and approbation of, the views taken by the Royal Colleges, as specially detailed in the resolutions of the Royal College of Physicians; but, as none of the Fellows or Licentiates of Faculty, so far as known to them, have hitherto professed or countenanced this system, they consider any further notice of the subject, on their part, in the meantime, unnecessary."

MEDICAL SOCIETY OF LONDON.

At a special general meeting of this society, held on Friday, October 10, the following resolutions, recommended by the council, were unanimously adopted:—

"I. That in the opinion of this Society the practice of homœopathy, or the prescribing medicines in what are called 'infinitessimal doses,' under a pretence that they are useful in the cure of disease, is founded in palpable error, is a delusion on the part of the practitioner, a deception on the public, and manifestly dangerous to its welfare.

"II. That the Fellows of the Medical Society of London cannot honourably hold any professional communion with homœopathists.

"III. That consequently any Fellow of this Society who shall hereafter practise homœopathy, or who shall knowingly meet in consultation any professed homœopathist, will thereby render himself unworthy of the Fellowship of this Society.

RAVAGES OF SCURVY IN RUSSIA.

In the new Russian provinces of Bessarabia, &c., an epidemic of scorbutus broke out in December 1848, continued to spread and become more serious till May 1849, and in June began to subside gradually. The cause was by universal consent referred to the scarcity of means of subsistence, consequent upon a short grain crop, a failure among the garden vegetables, a severe winter, and deficient supply of fuel. The German colonists, whose circumstances were more comfortable than those of the rest of the population, escaped. Districts in which horse-flesh or fish were used as food suffered comparatively little, yet the epidemic seemed to be guided in its spread to certain localities, where famine was not felt, by miasmatic influence. The symptoms were those usually described—in the severer cases, general weakness, indurations of the muscles especially those of the inferior extremities, hemorrhages, &c.; as sequelæ, dropsies, dysentery, passive inflammations of internal organs, gangrene, and typhus of a putrid type were observed. The means of cure found most efficacious were, acetate of iron, with bitters and aromatics, milk-diet, yeast, turpentine, both internally and externally, tar-water, and creazote. The total number of scorbutic cases amounted to 260,444, of which 67,958, or more than a fourth part, proved fatal.—*Med. Ztg. Russl., in Schmidt's Jarbücher.*

A NEW QUACKERY.

In Naumberg a man named Mahner is preaching the necessity of a new regeneration, not in the spiritual, but physical sense. He warns a sickly race that it must return to the lost state of "primitive health," or *Urgesundheit*, as the means of more fully enjoying life, and attaining a patriarchal old age. It is to be secured by a diet of bread and water, going barefoot, and letting the hair and beard grow; in short, making a nearer approach to man's original state in costume than the decencies or prejudices of modern society will altogether permit. On this topic he has been lecturing to a chosen few, but his doctrines do not seem to take, bread and water not being tempting, even with fourscore years promised as the prize of self-denial. It is also said that the

apostle does not fully act up to his own precepts, preferring a well-spread table, and every variety of wine or beer to the pure element ; but this may be a calumny of the hotel keepers. The German journals are perpetually turning up some eccentricity of this sort ; to prepare or preserve health, the oddest "cures" are resorted to. A section of the public seem determined to escape the hands of the faculty, and die by some irregular process, rather than with the aid of medicine. In a single advertising sheet could recently be counted up a water cure, a grape cure, a milk cure, and a hunger cure ; to these must be added another just getting into vogue—some of the former having had their day—it is the cure by muscular exercise, by which cripples from rheumatism are put through a course of gymnastics, and dancing is prescribed for the gout. These, however, are all merely cures or remedies ; the "primitive health" theory is founded on a system of living which would literally make "man's life as cheap as beasts," without much prolonging it. As far as hair and beard go, many of the Berliners are meeting the Naumberg preacher more than half way.—*Lancet.*

FALSE HOMŒOPATHIC AND MESMERIC REPORTS.

Several paragraphs have been industriously circulated in some of the English provincial newspapers, to the effect that about a dozen of the last year's graduates of this University, were homœopathists. It has been also stated that several of the Edinburgh professors are mesmerists, and believe in clairvoyance, &c., &c. We notice these reports merely to state that they, and others of a like nature, are utterly false, and have no foundation whatever.

THE AUSTRALIAN OPHIR.

The history of the discovery of the Australian gold is remarkable and instructive. The true discoverer is a man of science, who, sitting in his study at the distance of some fifteen thousand miles from the spot where the gold has been actually found, foretold its existence and abundance. We allude to the distinguished traveller and geologist, Sir Roderick Murchison, whose prophecy must, we think, be looked upon as the greatest practical triumph of the science of geology. The prediction was first made seven years ago, and insisted on repeatedly at various times down to 1850. It was, however, most clearly and distinctly enunciated in 1846, in a paper contained in the "Transactions of the Royal Geological Society of Cornwall." We give the remarkable prophecy in his own words, as they were written five years before the actual discovery of the Ophir of the antipodes :—

"But if gold exist not (in any appreciable quantity at least) in your otherwise richly endowed mineral county, there are, I am happy to say, good grounds for hope, that in their most distant colony Englishmen may find it abundantly. In an address to the Royal Geographical Society, delivered in May 1844, when commenting upon the valuable labours of Count Strzelecki in deciphering the structure of the great N. and S. chain which ranges along the eastern shores of Australia, I specially insisted upon its striking resemblance to the Ural mountains, whether in direction, in structure, or in alluvia: remarking, by the way, that *as yet* no gold has been found in this alluvium. I now learn, however, that fine specimens of gold have been found on the western flank of the Australian cordillera, particularly at the settlement of Bathurst, where it occurs in fragments composed of the same matrix (viz., quartz rock) as in the Ural. My friend and associate in the Imperial Academy of Petersburgh, Colonel Helmersen, has indeed recently suggested that a careful search for gold ore in the Australian detritus will, it is highly probable, lead to its detection in abundance ; since the Russians had long colonised the Ural mountains, and had for many years worked mines of magnetic iron and copper in solid rocks, before the neglected shingle, gravel, and sand, on the slopes of their hills and in their valleys, were found to be auriferous. If, then, in the course of your statistical inquiries, you may know of any good Cornish miner about to seek his fortune in Australia, be pleased to tell him to apply his knowledge of the mode of extracting tin ore from his own gravel to the drift and debris on the flanks

of the great N. and S. chain of Australia, or any smaller parallel ridges of that great country; for great would be my pleasure to learn, that through the application of Cornish skill, such a region should be converted into a British 'El Dorado.'"

Sir Roderick Murchison did more than this. He publicly addressed the Colonial Office on the subject of the Australian gold, and warned it as long ago as 1848, entreating that precautions might be taken against such disorders as might arise from his anticipated discovery, so that the Colonial Office has been duly forewarned three years before the working of the mines, and we can only hope the hint has not been thrown away on it.—*Examiner.*

PROVINCIAL MEDICAL DIRECTORY FOR SCOTLAND.

We have been requested to intimate to our readers, that the arrangements for the completion of this useful work would be greatly facilitated if such members of the profession as have *not received* blank schedules, would make immediate application for them to the editors, at No. 4, Adam Street, Adelphi, London; and if gentlemen who have delayed to send in the necessary return, would now transmit their schedules to the same address, without further loss of time.

The proposed work well deserves the patronage of members of the medical profession, and it is mainly to their co-operation with the editors that it must be indebted for its accuracy and completeness.

MR JOHN JOSEPH GRIFFIN.

In a pamphlet lately addressed to Dr Greville, by Mr John Joseph Griffin, he hazards the supposition that one of the Conductors of the "Monthly Journal" is the author of the reviews and other notices of his "Manual of Botany," which have been published in the "North British Agriculturist," as well as in our own pages, and proceeds to charge him with no small amount of critical and moral delinquency.

Our readers are, however, entitled to make another supposition—which, moreover, we beg to assure them is correct,—viz., that the writers of the reviews in question are in no way connected with each other, and that Mr Griffin has been wrong in aiming these thrusts at us in the dark.

PUBLICATIONS RECEIVED.

Translation of the Pharmacopœia of the Royal College of Surgeons of London, 1851; with Notes and Illustrations. By Richard Phillips, F.R.S., &c. London: Highley & Son. 1851.

Bradshaw's Companion to the Continent; with Observations on the Influence of Climate and Travelling. By Edwin Lee, Esq. London: W. J. Adams. 1851.

Zeitschrift für Rationelle Medicin. Herausgegeben von Henle und Pfeufer. Neue Folge. 1. Band. 1. Heft. Heidelberg. 1851. We have not received Heft III. for 1850.

God in Disease: or the Manifestations of Design in Morbid Phenomena. By James F. Duncan, M.D. Nisbet & Co., London. 1851.

An Essay on Infantile Remittent Fever, with especial Reference to the Diagnosis from Hydrocephalus. By Charles Taylor, M.R.C.S. Reprinted from London Medical Gazette. London: 1851.

TO CORRESPONDENTS.

Communications have been received from Dr Hall, Montreal; Dr Allan, Forres; and Dr Miller, Bombay. Others have been privately acknowledged.

We have also received a communication, signed *Medicus*, and although we entertain no doubt either of the good faith of the author, or of the accuracy of the facts which he details, we have two reasons for declining to make use of his narrative. In the first place, nothing shall induce us to violate a rule which we long ago adopted for our guidance,—viz., to exclude from our columns all anonymous contributions, for the accuracy of which we cannot admit our own responsibility. It would, we believe, conduce materially to the respectability of Journalism, and to the peace of the profession, if this course were more generally followed by our contemporaries.

In the second place, we differ from *Medicus* on the policy of attacking the quacks in the way proposed. All tampering with their practices, all professional intercourse with their persons, should be shunned by honest and enlightened men. The cause of quackery would suffer more from this negative procedure, than from the most elaborate illustrations of the ignorance and malpractice of its professors.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Case of Inversio Uteri successfully reduced on the Ninety-Third Day.* By Hugh Miller, M.D., Bombay.

On the 9th of November last, I was called to see P——, the wife of a native merchant here, aged twenty-five years, three years married, and the mother of two children. The husband stated, "that she was delivered of her second child on the 12th of August, under the care of a native 'dye,' and about an hour after the child was born the after-birth was expelled, and attached to it a tumour, adhering also to the body. The 'dye' removed the former, and returned the latter into its place, and much blood was then lost. A medical gentleman saw her two or three days afterwards, but did not make any examination of the parts at that time. About a week after the birth of the child, however, during the act of defecation, this tumour was again expelled; and the family being then alarmed, the doctor was sent for, and allowed to examine for himself, and he also returned the tumour into the passage as the 'dye' had done.

"A consultation of medical gentlemen was then held, and the husband was informed that nothing could be done in the meantime for his wife, but to give strengthening medicine, and keep her quiet; and she was under their treatment for fifteen days without improvement. A Parsee 'quack doctor' was afterwards employed, who used lotions, ointments, and other remedies, for more than two months; but she daily lost quantities of blood, and became weaker." Finally I was asked to remove a polypus, which was supposed to be growing in the passage.

I found the patient lying on her back; face and lips extremely pale and bloodless, pulse 96 and weak, skin cool, and tongue white. She complained of pains in the hypogastrium, and over the whole of the abdomen, and felt sick and faint when these parts were pressed upon, however slightly. She could not retain her urine at any time above an hour, but more frequently it passed off in

driblets every few minutes. Her bowels were constipated; and she had a continuous discharge from the vagina of clots of blood and mucus. The vagina felt tight at the orifice, but cool and dilateable; and the uterus was completely inverted, without procidentia—*i. e.*, the organ inverted on itself, and lying in the vagina. The body was enlarged, and of a pear shape, the apex turned upwards; the cervix firmly contracted, and grasping tightly the parts which have descended within it. I could not ascertain that any portion of the rectum was engaged, but the bladder was dragged towards it. With the finger, very carefully inserted beneath the folds of the inverted cervix, it could be slightly moved; pressure on any part of the uterus, however, gave pain, which could not be borne. A considerable quantity of discharge, principally blood and mucus, came away with my finger. By the speculum, the exposed surface of the uterus appeared smooth and free from ulceration, but bleeding from an immense number of small orifices, congested, and so tender that she could not bear it to be turned over so as to expose the cervix or upper part of the vagina,—hence no ulceration could have been seen if it existed. The patient informed me that the midwife had made various ineffectual attempts to remove the placenta by pulling at the cord, and that it was while she was using great force in this way that the whole came down as above stated.

I directed warm water fomentations to be applied both internally and externally till I should see her again. Knowing the fickleness of the natives, I expressed some reluctance to interfere in such a serious case, after other medical gentlemen had failed; but was entreated to undertake the treatment, and do whatever I might consider necessary.

Nov. 11*th.*—Repeated the examination with great care, and resolved that, although the inversion had taken place *three months* ago, its reduction might be accomplished, and ought at least to be attempted. I stated this to the husband as my opinion, and informed him fully of the danger which would attend the attempt, and the possibility of failure:—1st, That if nothing be done the patient cannot possibly get well, and must remain an invalid for life, which, under present circumstances, it does not appear can be very prolonged. 2d, If I succeed in restoring the inversion, she may be restored to health and usefulness. But 3d, Inflammation may be induced, and prove fatal. This explanation all parties fully understood, and though they had not been previously made aware of the nature of the case, they at once acquiesced in what I proposed to do.

Nov. 12*th.*—In company with my friend Dr Leith, who made a careful examination of the case, and agreed with me in the opinion I had given, we decided at once to make the attempt to reduce the inversion. It was thought that, if the patient were under the influence of chloroform, the hand could, especially if cautiously used, be passed without injury to the parts.

The patient being placed on her back, on the edge of a low bed, the bowels having previously been freely acted on by medicine, Dr Leith applied a handkerchief with some chloroform to her face, but she was afraid, and could not be induced to inhale it. I therefore proceeded to pass my hand, well lubricated with ointment, and the dilatation was accomplished much more easily than I had anticipated, although it seemed to cause considerable pain. The lower part of the uterus was grasped between the first two fingers and the thumb, and pressed upwards, gently at first, but firmly and steadily for about *ten* minutes; the vagina being put on the stretch, the cervix uteri felt slightly more open, and by continuing the pressure with the thumb, I succeeded in inserting the points of my fingers within the stricture, forcing a portion of the uterus before them; a little increased pressure then carried the whole of the fundus into its place at once, just as if it had been an India-rubber bottle turned outside in. My fingers were then within the uterus, pressing the fundus upwards; and I left them so for a little till contraction of the organ pressed them downwards. The uterus could now be felt above the pubis, and I withdrew my hand; and Dr Leith satisfied himself that the reduction was complete.

With a soft sponge and very cold water I cleaned out all the clots of blood from the vagina, and directed hot fomentations to be incessantly made over the abdomen and vulva.

℞. Træ. Mur. Morphiæ, ♏ xxx.
Mist. Camphorat., ℥j. M.
Fiat haust. To be given immediately.
℞. Pul. Ipecac. Comp., gr. xxx.
Mist. Acaciæ, q.s. M.
Fiant pil. vi. One to be taken every four hours.

We left her with strict orders to the attendants to keep her perfectly quiet, and to give her only a spoonful of rice congee occasionally.

Vespere.—Pulse 96; skin moist; has slept a little from the draught and pills; pain of abdomen less than it was before the operation; examined with the finger, and found the uterus as I had left it; some tumefaction of the external parts, and a little tearing of the posterior edge externally. Ordered to continue the pills and fomentations.

13*th.*—Has passed a good night, having slept five hours; pulse 96; skin cool; fomentations gave relief; voided a pint of urine at once this morning; bowels not moved; vagina somewhat tender to touch, but a small-sized speculum passed easily. On removing a quantity of clotted blood from the vagina, the uterus appeared much more healthy than could have been expected. The lips of the os seemed denuded of the villous coat, and at one point a little ulcerated. The whole cervix tender to the touch of the speculum. The vagina has escaped with a trifling laceration on the posterior wall, but not deeper than the mucous membrane. Applied five

leeches to the cervix, and after they came off, sponged the parts well with hot water. Fomentation to the abdomen to be continued.

℞. Hydr. Chlorid., gra. vi.
Pulv. Ipecac. Comp., ʒss. M.

Fiant pil. vi. One pill to be given every three hours during the day.

℞. Zinci Sulphat, ʒss.
Aquæ Rosæ, Aquæ Fontis, āā, ʒxii. M. et S.

Fiat Lotio. The lotion to be used over the tender parts externally, and quietness enjoined.

Vespere.—Said she was better; skin cool; thirst less; pain less; has slept a great part of the day. To have no medicine during the night.

14*th.*—Has slept well; scarcely any abdominal pain now; pulse 100; bowels not yet moved; complains of headach.

℞. Ol. Ricini, ʒj., to be taken now. Continue fomentations.

15*th.*—Passed a good night; bowels moved once; some griping pain in the abdomen, but no tenderness on pressure to-day. Has a slight hemicrania.

℞. Infusi Rhei, ʒvii.
Quinæ Disulph. gra. xii.
Acid Sulph. Dil. ♏ xv. Fiat Mist.

One-third part of the mixture to be taken morning and evening.

19*th.*—Bowels have been freely moved by the mixture, and the headach is quite gone; improving rapidly in strength.

December 6*th.*—I was prevented by sickness from seeing my patient from last date till this. The laceration in the vagina is nearly healed. The uterus is very much decreased in size, and less tender to the touch. The mucous covering over the cervix, which formed the stricture, is completely gone, and the part red and bleeding. The ulceration also extends within the os as far as the eye can reach; and a sponge on the small forceps passes freely to the fundus. General health improving.

January 14*th*, 1851.—Under the usual remedies, the ulceration has been gradually improving, and to-day the catamenia has appeared copiously, but without pain.

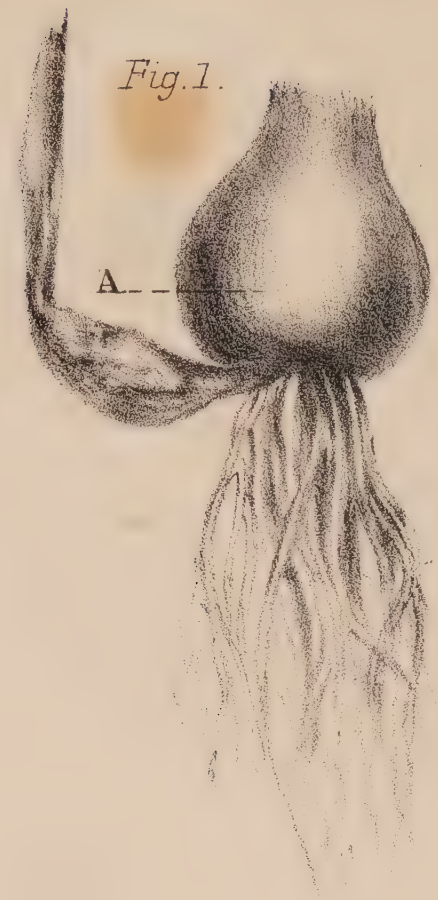
February 19*th.*—The catamenia appeared again on the 14th inst., freely and naturally. The uterus is still decreasing in size, and there is no tenderness on pressure anywhere, although the ulceration is not yet perfectly closed.

March 20*th.*—Has again menstruated at the proper date this month, and in every respect healthy.

April 22*d.*—The uterus is now of the natural size, the ulceration appears to be quite healed up, and there is neither any pain nor vaginal discharge. The catamenia again appeared this month at the proper period, being the fourth since the operation; and my patient's health is now completely established, and she is restored to her family and to society.

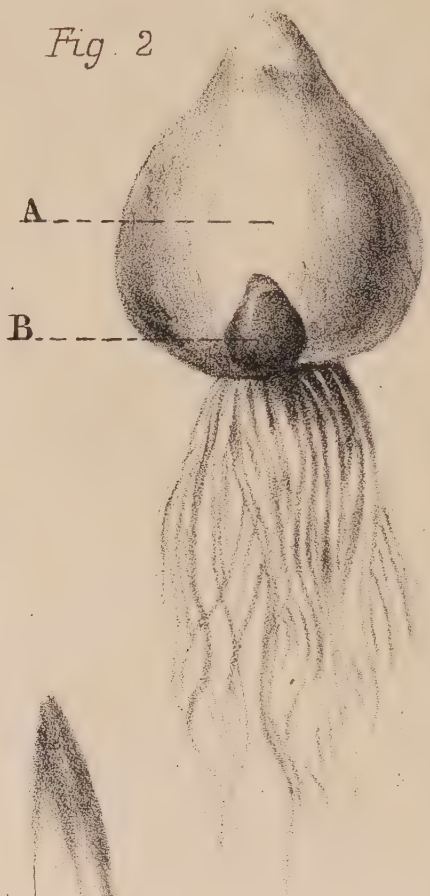
BOMBAY, 22d August 1851.

Fig. 1.



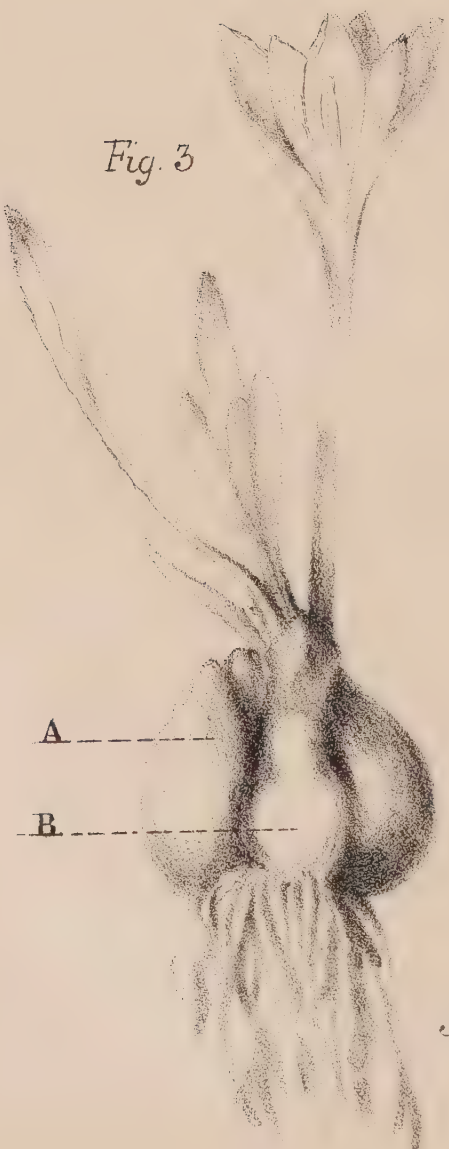
June

Fig. 2



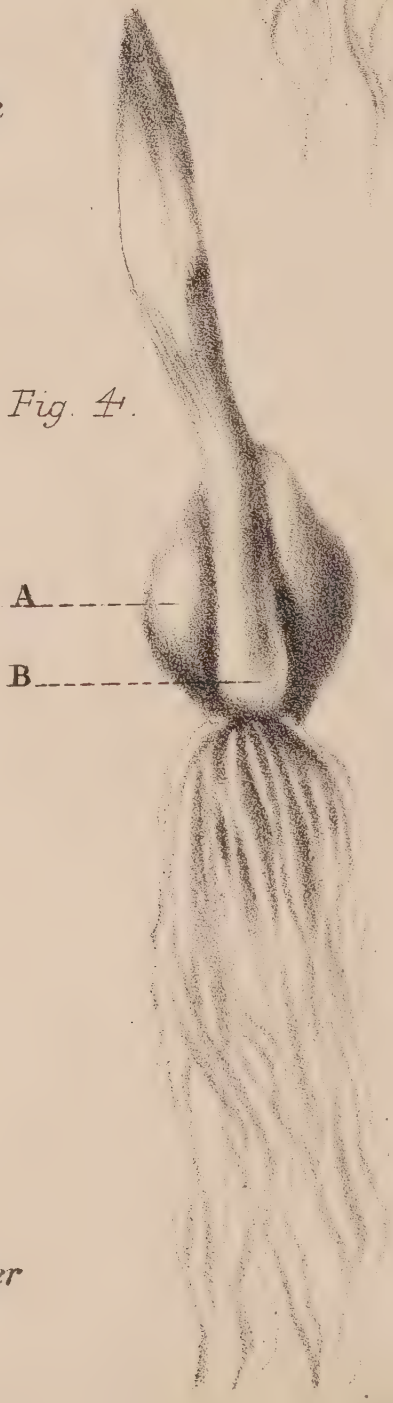
July

Fig. 3



September

Fig. 4.



February

Fig. 5.

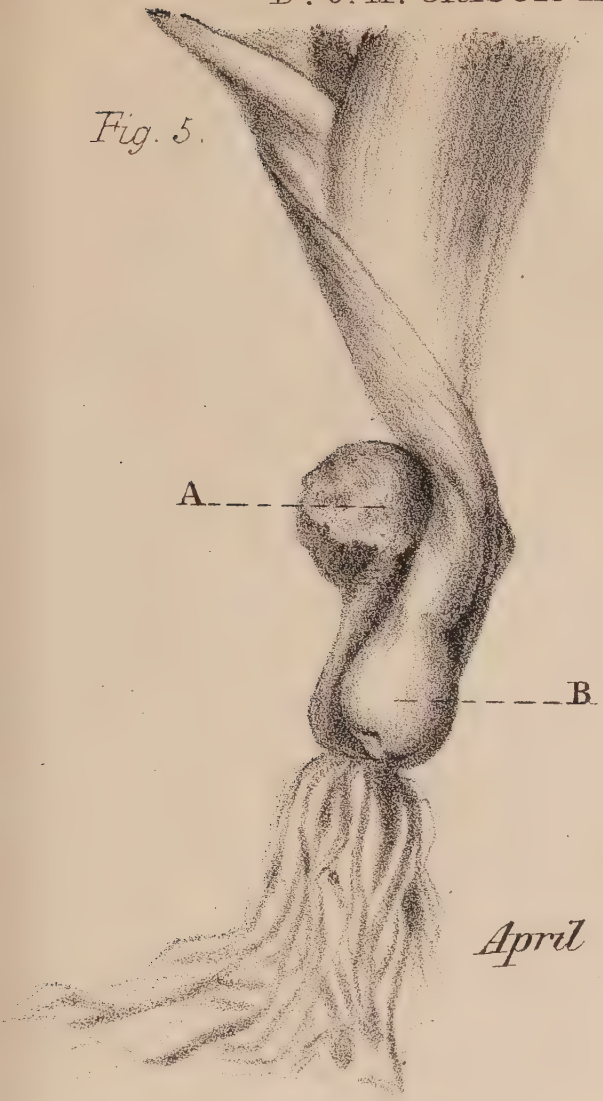


Fig. 6.

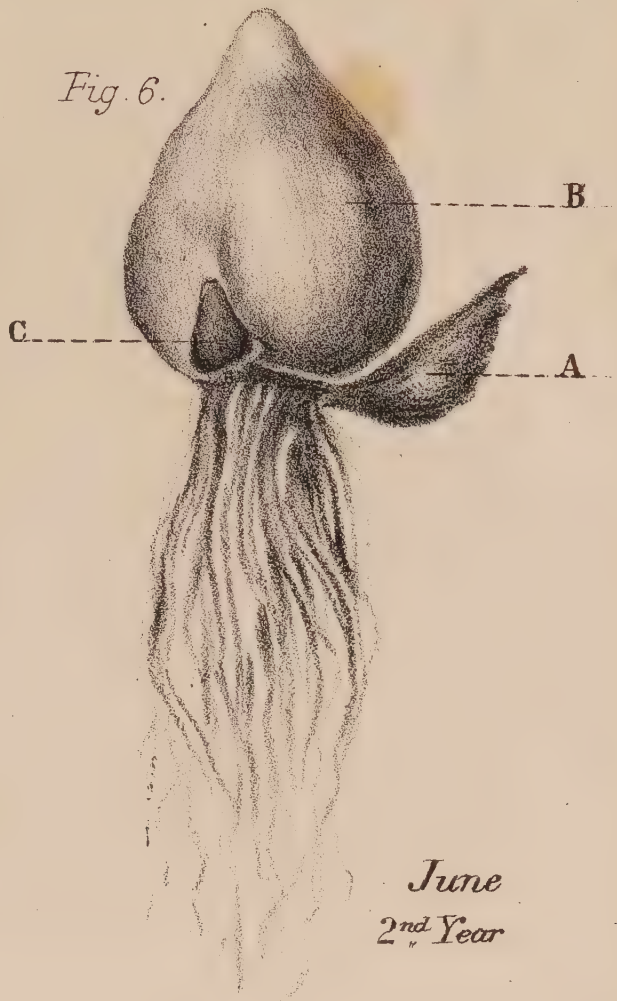
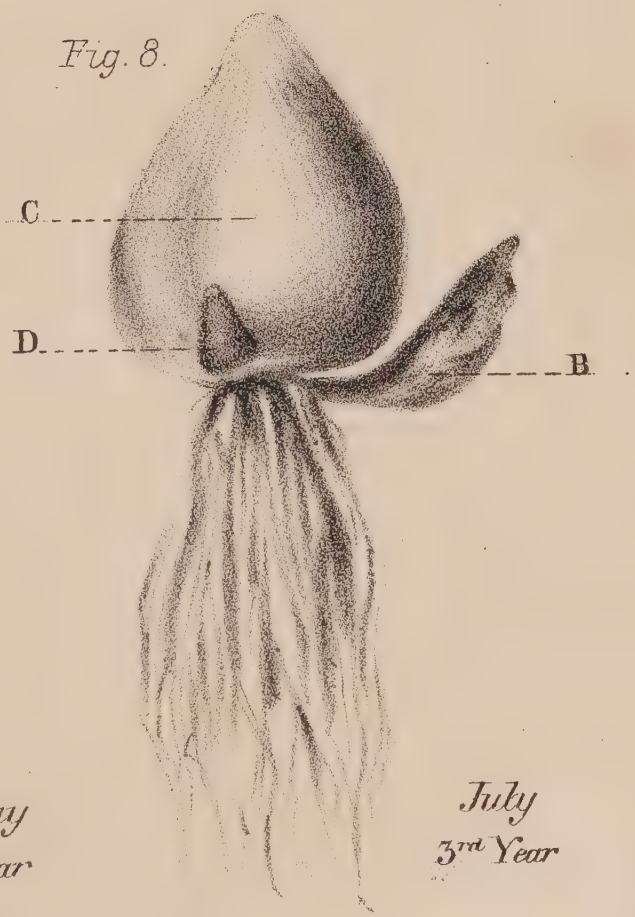


Fig. 7

Fig. 8.



ARTICLE II.—*On Colchicum Autumnale, chiefly with reference to the Growth of the Plant, and its Physiological and Therapeutic Actions.*[1] By J. M^cGRIGOR MACLAGAN, M.D., Edin., Extraordinary Member of the Royal Medical Society.—(*With a Plate.*)

IT appears probable that this drug is identical with that known to Dioscorides,[2] Paulus Ægineta,[3] Alexander Trallianus,[4] and other ancient authors, under the names of "Κολχικον" and "Ἑρμοδακτυλος;" but it is equally probable that its use in medicine did not become established till so late a period as 1763, when it was introduced, with many other remedies, by Baron Stoërk,[5] of Vienna. Subsequently it has been examined with much attention by Sir Everard Home,[6] Sir Charles Scudamore,[7] Dr Haden,[8] and Mr Want.[9]

In the following inquiry, I propose to direct attention,—first, to the botanical and chemical histories of the drug, chiefly with reference to the growth and preservation of the bulb, and to the nature of the active principle obtained from it; and secondly, to its physiological and therapeutic actions.

BOTANICAL HISTORY.

COLCHICUM belongs to the endogenous natural order *Melanthaceæ* of Lindley, and *Colchicaceæ* of De Candolle, of which order the following are the principal distinguishing characters:—

They are in general bulbous, or fibrous-rooted plants. In some the flowers are half-subterranean, like the *Crocus*; in others they form spikes, or simple branching herbaceous stems, as in the genus *Veratrum*. The Calyx and Corolla form together a perianth, generally consisting of six pieces, which are usually petaloid. The ovary is three-celled, with many seeds.

They belong to Linnæus' class and order *Hexandria Trigynia*. The *Melanthaceæ* approach in character most nearly to the *Liliaceæ*, but are distinguished from that order by their tripartite fruit, and anthers turned outwards.

These plants are common at the Cape of Good Hope, Asia, and North America.

---

[1] Being an abstract of an Inaugural Dissertation, delivered to the Medical Faculty of the University of Edinburgh, August 1851.

[2] Dioscorides—Materia Medica, lib. iv., cc. 84, 85.

[3] Paulus—Materia Medica, lib. vii., c. 3.

[4] Alexander—Medici Libri, lib. xi.

[5] Stoërk de Colchico, 1763.

[6] Sir E. Home—Philosoph. Transac., 1816-17, part 2.

[7] Sir C. Scudamore on Gout and Rheumatism, 1819.

[8] Haden on Colchicum, 1820.

[9] Want—Lond. Med. and Phys. Jour., vol. xxxii.

"This order seems to be confined within no geographical limits; it is, however, far more abundant in northern countries than elsewhere." [1]

In their properties, these plants are almost always poisonous in all their parts, and act generally as *narcotico-acrids*.

The principal genera of this order which furnish medicinal plants, and whose active properties have been most fully investigated, are COLCHICUM, VERATRUM, and ASAGRAEA, to the former of which, however, our attention is at present limited.

There are three well-known and distinctly-defined species of COLCHICUM,—namely, *Colchicum autumnale*, *C. montanum*, and *C. variegatum*.

Colchicum Illyricum has been supposed by some to be a distinct species, but more generally it is considered to be identical with *C. variegatum*. To this species the drug known as HERMODACTYL has been referred.

Kunth[2] believes that the *Colchicum montanum* of Allioni and the *C. autumnale* χ of Bertolini are identical. He thus describes this species:—The tube of the perigone is five or six times longer than the limb; the calycine segments lanceolate; stamens equally inserted; bulb unifloral; leaves succeeding the flowers, linear lanceolate, obtuse, and attenuated towards their base. *C. variegatum* he regards as a distinct species, and thus describes it:—Leaves oblongo-lanceolate, channelled, and undulated at their margin; the lanceolate segments of the corolla acute.

COLCHICUM AUTUMNALE is the only species officinal in this country. In vernacular language it is termed *Autumn Crocus*, or *Meadow Saffron*. It is indigenous to Britain, occurring frequently in Suffolk, Oxfordshire, and Wiltshire, in England; but in Scotland, although found in many situations, it must be regarded rather as an introduced than a native plant, the only Scotch station assigned to it being at Alloa.[3]

The leaves of COLCHICUM are broad, lanceolate, and erect.

The flowers arise from the bulb by a long tube, which is surrounded at the base by a membranous sheath.

The stamens are inserted in the oblongo-ovate segments of the pale purple perianth.

The ovary is at the base of the bulb, and its long filiform style runs up the whole length of the tube.

In its mode of inflorescence, COLCHICUM AUTUMNALE somewhat resembles *Tussilago Farfara*, or common *Colt's-foot*, inasmuch as the flowers and leaves occur at different seasons,—the difference being, that in the *Colt's-foot* the leaves immediately succeed the

---

[1] Lindley's Vegetable Kingdom.
[2] Kunth—Enumeratio Plantarum, tome iv., 1843.
[3] Lightfoot—Flora Scotica.

flowers, whilst in the COLCHICUM several months elapse between the flowering and leaf-bearing seasons.

The manner of the propagation of COLCHICUM AUTUMNALE is interesting, not merely in a botanical point of view, but also as regards the medicinal qualities of the corm or bulb at different periods of its growth.

It is well known to be capable of being propagated in two different ways,—namely, by seeds and bulbs. Dr Christison,[1] however, has mentioned a third,—viz., the throwing off of a number of small immature bulbs from the parent plant. The manner in which perfect plants grow from seeds and immature bulbs has not been determined. It has not, indeed, been accurately ascertained if these immature bulbs ever produced perfect plants at all; but it seems probable that their immature separation from the parent renders them incapable of coming to perfection, and they are perhaps to be regarded more as a kind of abortion, than as the fruits of a legitimate mode of propagation.

I have endeavoured to trace the growth of the plant from seeds, but in vain. The COLCHICUM in Scotland rarely produces fruit, and never ripens its seeds. I have never succeeded in attempts to grow any of the seeds procured from the druggists' shops.

The propagation of the plant, by its more ordinary method of forming a single new bulb, has been better ascertained; and, in order to enable the growth of a single bulb to be traced from its origin to its termination, I shall in my description refer to the accompanying diagram,[2] where the individual bulbs are lettered, and can be easily followed through all the stages of their progress.

We shall suppose the time at which the observation is commenced to be *June*, and that we have in view a nearly full-sized bulb, which we shall call A. At this time it is as large as an apricot, firm, amylaceous, and extremely bitter, and having attached to it the shrivelled remains of the old bulb, and the leaves, now yellow and decayed.

At the end of *June*, or commencement of *July*, a small bulb will be observed to have become developed upon the side of the corm A, at its lower part. This small bulb we shall call B. At this time it is a little larger than a grain of wheat, and lies in a small fissure on the side of the parent bulb, a little above the origin of the radicles. It increases slowly and gradually in size till the beginning of *August*, when it appears as a mere dilatation of the flower-stalk, which it then commences to put up.

In *September* the flower is in full perfection, the long tube of the perianth of which, has raised the six-partite limb to the height of from six to eight inches above the ground. The flower remains for two or three weeks, and then dies down; and nothing of the

---

[1] Dispensatory.

[2] The figures in the diagram are reduced one-third.

plant is seen above the surface till the beginning of *February*, when the leaf-stalk commences to rise.

If at this time the plant be taken up, A and B will still be found to be united, but B will be observed to have increased little in size since autumn, being but still hardly larger in diameter than the leaf-stalk.

The bulb B thus grows little during the autumn, but in winter it increases rapidly in size; in *April* it is like a large hazel-nut, and from that time it increases still more, till the end of *June* or beginning of *July*, when it is, as Dr Christison states, as large as an apricot.

In *April* the leaf-stalk is found perfected by a fine group of dark green leaves, generally three in number, and having within their sheath the capsules which ought to ripen their fruit in the course of summer.

In *May* the old bulb will be found dry and withered, and with very little starch; and in *July*, if the plant be taken up, three bulbs will be found,—A, now reduced to the form of a membrane, bearing no resemblance to a bulb at all; B, now arrived at full growth; and a new member of the series, C, the progeny of B.

This third bulb, C, it is unnecessary for us to trace further; it follows a course precisely similar to that through which B has passed, and which has just been described.

The history is now complete, so far as regards the rotation of flowers, leaves, and fruits; but the lifetime of B has not yet expired; for if we take up the plant in *May* of the third year, we still find the shrivelled remains of B, and C large, firm, and amylaceous, now bearing leaves, having flowered during the previous autumn. We may go on still further, and if in *July* the plant be examined, B will be found to have almost disappeared, C large, amylaceous, and extremely bitter, and at its base a new bulb D, of very minute size, which in the ensuing autumn will produce the flower.

Thus we have traced the growth of COLCHICUM from its infancy, through maturity, to decay.

It must be evident, from the foregoing observations, that the plant is essentially biennial, but it has been thought by some to be triennial. "It sees a part of three successive years, but only outlives two revolutions of each season." (Christison.)

Of peculiarities in the growth of COLCHICUM, I may mention one, which has struck me forcibly on account of its frequent occurrence.

In *February* and *August*, instead of one leaf-stalk and one flower-stalk making their appearance at their respective periods, I have often remarked that two have occurred, one on either side of the parent bulb. I believe this to be one of the effects of cultivation, as I have nowhere seen it remarked in descriptions of the plant by botanical authors.

Having observed, in cases where the leaf-stalk was accidentally removed from the parent bulb, that a new bulb was thrown out from the top part of the old one, I made the following experiment, in order to ascertain, if possible, the cause of this peculiarity.

Two bulbs were taken up on 1st *November;* from these the leaf-stalks and their bulbs were detached, and the parent bulbs replanted. Three weeks afterwards a small leaf-stalk was observed to have been given off from the top part of each bulb; and on another occasion, on which the same experiment was tried, two of these leaf-stalks made their appearance. In both of these instances they were found to proceed from very minute bulbs, not larger than barley-corns.

I believe that there is always one of these adventitious bulbs at the top of each parent bulb, but that they never germinate, where the plants are indigenous, unless the proper leaf-stalk and its bulb be removed. When they do so, however, they always remain attached to the parent bulb, and are perfected in much the same time as the normal bulb of the leaf-stalk.

Dr Christison has mentioned that the full size of a COLCHICUM bulb is that of a small apricot. This I believe to be perfectly correct, in places where the plants are indigenous; but I have frequently procured specimens from the Botanic Garden here, where they were cultivated for the purpose of examination, of the size of large apples; and in *October* 1849, I procured one which weighed nine and a half ounces.

I have lately received from the North-Western Provinces of India two specimens of COLCHICUM bulbs, which respectively bear the names of "SORINJAN TULK," or bitter *Sorinjan*, and "SORINJAN SHEERUN," or sweet *Sorinjan*. Both of these were brought from Bombay, and I believe are identical with those mentioned by Dr Royle,[1] and thus described by Dr Pereira:—"The SORINJAN SHEERUN resembles the cormus of colchicum autumnale. The cormi are flattened, cordate, hollowed or grooved on one side, convex on the other; they have been deprived of their coats, are externally dirty-yellow or brownish, internally white, easily broken, farinaceous, odourless, tasteless, or nearly so, and worm-eaten."[2] This description entirely coincides with the specimens in my possession. They are so easily broken, that when I received them nearly one-half was reduced to powder.

Geoffroy[3] has correctly pointed out how they may be distinguished from the cormi of COLCHICUM AUTUMNALE. "They are not rugose, are white internally, are moderately hard, easily broken,

---

[1] Royle's Materia Medica. [2] Pereira's Materia Medica.
[3] Geoffroy—Traité de Matière Médicale.

and form a whitish powder; whereas the dried cormi of COLCHICUM AUTUMNALE are rugose, softer, and have a reddish or grayish tint, both internally and externally."

The SORINJAN TULK appears to me to resemble the bulb of COLCHICUM *variegatum;* they are much smaller than the preceding, and possess considerable bitterness; they are not so easily broken. Pereira states that they are marked by longitudinal stripes, indicating a laminated structure. I have failed to discover this in the specimens in my possession; and I presume if the true HERMODACTYL possesses a laminated structure, it cannot be considered as a true cormus, and therefore cannot belong to the colchicum tribe. The Hakeims use both kinds in *rheumatism*, and in many nervous disorders, in doses of two to four grains of the powder three times daily.

I also received from the same source a few seeds, which were said to be those of COLCHICUM *Illyricum*. They were of the size of, and altogether not unlike, *Lupin* seeds. I attempted to grow them, but in vain. They are used in the same diseases as the SORINJAN, and are said to be diuretic.

PHARMACOLOGY.

Every part of the plant, excepting the leaves, has been used in medicine. The flowers have been used galenically, and have been supposed to be that part of the plant used in the preparation of the *Eau médicinale d'Husson*. In most modern pharmacopœias, as in those of the British Empire and of France, the seeds and corms alone are officinal.

As found in commerce, the seeds of COLCHICUM are rough, small, and almost spherical, imparting to the mouth a bitterness and acrimony which surpasses that of the bulb. They are about the size of millet seeds, and have a dark brown colour.

The slices of the bulb are grayish-white, somewhat kidney-shaped, and have a dark-brown covering externally. When in good preservation, they are dry and easily fractured. They should not be thicker than a half-crown. Their taste is extremely bitter, and somewhat acrid.

There has been much difference of opinion as to the time of year in which the cormus should be taken up for medicinal use. According to my own observations, the middle of JULY is the most fit time for this purpose. The bulb is then of its greatest size, it is firm, amylaceous, and extremely bitter. The *bitterness* is the character which affords the best criterion of its activity.

According to some, the amount of *Starch* and firmness in the bulb are the proper guides; but the amount of active principle does not necessarily bear any corresponding ratio to that of the starch, in proof of which I may observe, that the bulb, when more spongy and watery, and less amylaceous, as in *April*, possesses almost as much bitterness as when nearly mature,—a circumstance remarked

by Dr Christison, and which I have confirmed by repeated observation.

I feel confident that we can place no reliance in a test of goodness proposed by Dr A. T. Thomson.

In 1820 Dr Thomson[1] published a paper showing the tincture of *Guaiacum* to be a test for *Gluten*, and pointing out its applicability as a test for the goodness of COLCHICUM. His manner of procedure was as follows :—Ten grains of the bulb were rubbed in a mortar, with sixteen minims of distilled vinegar, and immediately afterwards sixteen minims of the *tincture of guaiacum* were added. A beautiful cœrulean-blue colour was produced with those specimens which, according to Dr Thomson, were good.

I have several times made, with specimens which I knew to be good from their bitterness, similar experiments, and obtained no cœrulean-blue colour, but only a dirty brown.

In the same year (1820) Mr Battley[2] published similar experiments which he had made, but with entirely different success from Dr Thomson.

In order to ascertain what the true nature of this re-action might be, I expressed the juice from several bulbs, and having filtered away the starch, applied the test to the fluid; a beautiful blue colour was immediately produced. It was evident from this that the *gluten* was not the part acted on. The blue liquid was then heated well, the albumen coagulated, and the blue colour remained with the coagulum, whilst the remaining fluid was free from colour. On raising the heat to 212°, the blue colour entirely disappeared. On the other hand, the test was applied to the starch collected on the filter, but without effect. The experiment was varied in the following manner :—

The fluid was first filtered to separate *starch*, then boiled and filtered to separate *albumen*. On the test being applied both to the filtered fluid, and to the *albumen* on the filter, no blue colour was obtained.

From these experiments I drew the following conclusions :—

I. That albumen is the principle acted on.

II. That a heat above 180° destroys this action.

III. That the value of this test is to prove that the bulbs have been dried at a temperature not higher than 180°.

During the summer of 1849 I endeavoured, by another process, to determine the medicinal value of the drug at different seasons, by ascertaining the amount of alkaloid contained in the bulb at monthly periods, and thus to determine the fittest time for gathering for medicinal use.

Having expressed the juice of several bulbs, and separated the

---

[1] Thomson—London Medical Repository, vol. xiv.

[2] Battley—London Medical Repository, vol. xiv.

albumen and *starch*, I attempted by precipitation in a graduated test-tube with *tincture of galls*, to determine by the bulk of the precipitate the amount of active principle. This, however, failed, on account of the precipitate partly floating to the top, and partly remaining at the bottom. This I believed was occasioned by the fluids being of different densities. A similar experiment was tried with an alcoholic solution of the bulb, but with little better success, as the small quantity of active matter procured from a necessarily small amount of bulbs gave an almost imperceptible precipitate.

At whatever period the bulbs are taken up, they should be sliced into small pieces about the thickness of a half-crown, the outer membrane having been previously peeled off. They should be then spread out upon trays to dry, either at ordinary temperatures, or at any rate not exceeding that of 150° F., after which they must be carefully preserved from moisture. (Christison.)

Mr Houlton [1] recommends that the bulb should be stripped of its dry coating, carefully deprived of the bud or young bulb, and then dried whole.

CHEMICAL HISTORY.

Vegetable chemistry has of late years made great and rapid progress, and in no way more practically useful than in the discovery and examination of the organic bases which constitute the active principles of many vegetables used as medicines.

It is hardly necessary to do more than allude to *quina* and *morphia*, as examples of the valuable contributions which chemistry has made of late years to the resources of the medical practitioner.

It happens, however, in many instances that we may be quite aware of the existence, in a medicine, of an active principle, and yet, on the one hand, from the difficulty of obtaining it pure, or on the other, from the activity of the crude drug in small doses, it may, in a practical point of view, be of little consequence to ascertain the precise chemical and other qualities of the active ingredient. Yet assuredly the more we can investigate these active principles the more precise will be our knowledge of the properties of the original drug.

The active principle of COLCHICUM AUTUMNALE was, on the authority of Pelletier and Caventou,[2] supposed to be identical with that of Veratrum album,—namely, VERATRIA. Geiger and Hesse,[3] however, discovered in COLCHICUM an alkaloid differing from veratria, and which was named by them COLCHICIA.

---

[1] Houlton—Pharmaceutical Journal, vol. iv.

[2] Pelletier and Caventou—Journal de Pharmacie, tome vi.

[3] Geiger and Hesse—Geiger's Pharmacie, tome i. Annalen der Pharmacie, tome vii. Journal de Chimie, tome x.

The following was their process for preparing it:—

The seeds were bruised and exhausted by digesting them in *rectified spirit*, acidulated by *sulphuric acid*. The excess of acid was removed by the addition of *hydrate of lime*, and the fluid filtered to separate the *sulphate of lime* which was deposited. Any excess of *lime* which might remain in the spirituous fluid was removed by the careful addition of one or two drops of *sulphuric acid*. The fluid was then filtered and distilled to recover the alcohol, and the watery residue of the distillation was mixed with an excess of *carbonate of potass*. The precipitate which fell was dried between folds of bibulous paper, and then was taken up in *absolute alcohol*, the alcoholic solution decolorised by *animal charcoal*, and evaporated for crystallisation at a gentle temperature.

By this process, it is said, COLCHICIA may be obtained also from the flowers and corms.

During the winter of 1849, and again during that of 1850, I endeavoured to procure Geiger and Hesse's COLCHICIA by this method, but without success, although I followed their process exactly. I was particularly cautious in the application of heat; for it is well known that many of the vegetable bases, such as those from *Hyoscyamus* and *Stramonium*, are very easily destroyed by an undue elevation of temperature, which may probably account for those and other similar bases not having been accurately examined and analysed. COLCHICIA is perhaps as easily destructible as *hyoscyamia* and *daturia*.

I have not been able to find that any chemists have procured crystals of COLCHICIA, except Geiger and Hesse. My account of its properties, therefore, rests solely on their authority.

"COLCHICIA crystallises from its alcoholic solution, when that is mixed with water, in colourless prisms and needles. If the alcoholic or ethereal solution be evaporated, the COLCHICIA remains in the condition of a transparent varnish-looking substance."

It was in this state alone that I procured anything which could be considered to be COLCHICIA. I am very sceptical with regard to the crystalline nature of COLCHICIA at all; for having dissolved this matter successively in *alcohol*, *ether*, and *water*, and having left the solutions to spontaneous evaporation, no trace of crystallisation ever appeared, but the bitter matter was invariably deposited in the state of a brown resinous-looking mass.

This brown mass was without smell, and possessed considerable bitterness, the bitter taste being generally followed by a slight sense of irritation in the throat, but not with the intense acrimony of VERATRIA.

"COLCHICIA in its hydrated condition has but a feeble alkaline re-action, but neutralises acids completely, and forms with them salts, which are in part crystallisable, and which have a bitter, acid, and slightly acrid taste.

"COLCHICIA dissolves pretty easily in water. It is very soluble in rectified alcohol and ether.

"It strikes a yellow colour with solution of *chloride of platinum*, but does not form an insoluble *platino-chloride* with it. *Tincture of galls* causes a white precipitate, and with *tincture of iodine* it becomes rapidly turbid, producing a russet-brown colour.

"It is persistent in the air, melts easily at a gentle heat, and is destroyed by a higher temperature." (Geiger and Hesse.)

The alkaloid VERATRIA, discovered by Pelletier and Caventou, in the root of *Veratrum album*, and in the seeds of *Helonias* or *Asagræa officinalis*, is prepared much more easily. I found no difficulty in procuring it by the process of the Edinburgh Pharmacopœia, which is nearly the same as that described by Couerbe,[1] and is as follows:—

"Grind the seeds of *Cevadilla* in a coffee-mill, and form them into a thick paste with *rectified spirit*. Pack this firmly in a percolator, and pass *rectified spirit* through it till the spirit ceases to be coloured. Concentrate the spirituous solutions by distillation so long as no deposit forms, and pour the residuum while hot into twelve times its volume of cold water. Filter through calico, and wash the residuum on the filter so long as the washings precipitate with *ammonia*. Unite the filtered liquid with the washings, and add an excess of ammonia. Collect the precipitate on a filter, wash it slightly with cold water, and dry it, first by imbibition with filtering paper, and then in the vapour bath. A small additional quantity may be got by concentrating the filtered ammoniacal fluid, and allowing it to cool."

The Pharmacopœia, I think, has erred in not ordering the immense volume of water, into which the hot alcoholic solution is thrown, to be evaporated down. In one of my operations the quantity of water amounted to twenty-four pints; and it was found quite impossible to precipitate this immense quantity by *ammonia*. It was therefore evaporated down to four pints, when an abundant precipitate was obtained.

The quantity of impure VERATRIA which was obtained from *fifty-six ounces* of seeds was *fifty grains;* but Couerbe states that *a drachm* of VERATRIA may be obtained from *sixteen ounces* of seeds; but, as Dr Christison remarks, "the product to be so large must be very impure."

The impure *Veratria* which I procured was taken up in very weak *hydrochloric acid*, decolorised with *animal charcoal*, and re-precipitated by *ammonia*. It was then nearly pure white, and weighed *twenty-seven grains*.

That VERATRIA and COLCHICIA (if Geiger and Hesse's account of the latter be correct) are in no respect identical, is obvious from their great difference in properties, viz.,—

[1] Couerbe—Annales de Chimie et de Physique, tome lii.

(1.) VERATRIA is entirely incrystallisable.

(2.) VERATRIA possesses a powerful persistent acridity of taste.

(3.) VERATRIA, when it comes into contact with the nostrils, excites most violent sneezing. So powerful indeed is this property of VERATRIA, that when engaged in working with the seeds of *Cevadilla*, I have frequently been obliged to desist, on account of the sneezing induced.

(4.) VERATRIA is almost entirely insoluble in *water*. It is soluble in *alcohol* and *ether*.

(5.) VERATRIA, by the action of *nitric acid*, becomes first red and then yellow. *Sulphuric acid* produces first a yellow, then a blood-red, and lastly a fine violet colour.

(1.) COLCHICIA crystallises in delicate acicular prisms.

(2.) COLCHICIA, though extremely bitter, possesses none of this.

(3.) COLCHICIA possesses no sternutatory effects at all.

(4.) COLCHICIA is very soluble in *water*, *alcohol*, and *ether*.

(5.) On COLCHICIA *nitric acid* first produces a bright violet, then an indigo-blue colour, quickly passing into green and yellow. *Sulphuric acid* colours it yellowish-brown.

These comparative characters are sufficient to establish the complete dissimilarity of these two bases.

COLCHICIA has not yet been analysed, and therefore no formula of its constitution exists.

Geiger and Hesse, although they did not analyse their COLCHICIA, ascertained its physiological properties, and compared them with those of VERATRIA.

The following experiments are related by them :—

One-tenth of a grain of COLCHICIA dissolved in weak alcohol was administered to a cat eight weeks old. There formed immediately much froth at the mouth. At the end of about an hour there were abundant liquid dejections, and then followed, after an interval, several attacks of vomiting. The gait of the animal became staggering, it fell, rolled from side to side, uttered a plaintive cry, and appeared agitated by convulsive movements. These symptoms augmented more and more, and death took place in twelve hours.

On opening the body, the stomach and intestinal canal were found violently inflamed, and congested throughout their entire course.

There was given, for comparison, one-twentieth of a grain of VERATRIA to a cat a little younger. The poison showed itself with much greater intensity. The animal staggered, moved convulsively, and died in ten minutes.

On inspection, no inflammation was found, except at the inferior part of the œsophagus, which part of the digestive tube was not inflamed in the cat poisoned by COLCHICIA.

It appears, from these experiments, that the poisonous properties of COLCHICIA are essentially those of the *irritant* class, whilst, from the rapid action of VERATRIA, and the absence of post-mortem appearances, it seems to have produced death by an action on the *nervous system*.

No other important principle has been announced as existing in

COLCHICIA, except the alkaloid discovered by Geiger and Hesse. It was the substance taken by Pelletier and Caventon for VERATRIA; and, according to their statement, it was combined with *gallic acid.*

The following is their analysis of the corms :—

(1.) Fatty matter, composed of { Olein. Stearine. Volatile acid.
(2.) Supergallate of VERATRIA (*Colchicia.*)
(3.) Yellow colouring matter.
(4.) Gum. (5.) Starch. (6.) Inulin. (7.) Lignin.[1]

Stoltze[2] also a nalysed the bulb of COLCHICUM, and found that it contained in

	March.	October.
Volatile acrid matter,	a trace.	rather more.
Soft resin,	0·04	0·06
Crystallisable sugar,	0·41	1·12
Incrystallisable sugar,	5·91	2·72
Bitter extractive		2·17
Difficultly soluble extractive,	1·30	0·52
Gum,	0·81	1·65
Starch,	7·46	10·12
Lignin,	2·32	1·61
Extractive soluble in potass,	0·61	0·52
Water,	81·04	80·31
	99·90	100·80

We find by this analysis that in *March* the *starch* is diminished 3 per cent., and the bitter extract greatly increased.

Another analysis was performed by Melandri and Moretti.[3]

The *seeds* have only been analysed qualitatively by Buchner,[4] but his notice of the constituents is not worthy of remark.

PHARMACY.

Much discussion has been held respecting the comparative value of the different preparations of COLCHICUM, and especially as to whether the seeds or corms yields the most eligible forms for its administration.

"It might be reasonably expected," says Dr Barlow, "from the virtues of COLCHICUM being found to reside in the seeds as well as in the root, that the former would yield a medicine of greater uniformity, being in a state of more perfect and determinate maturity, requiring less care in the collection and preservation, and

---

[1] Pelletier and Caventou—Journal de Pharmacie, tome vi.
[2] Stoltze—Thomson's Organic Chemistry.
[3] Melandri and Moretti—Bulletin de Pharmacie, tome ii.
[4] Buchner's Repertorium, xliii.

being less liable to have their powers impaired. My experience of the several preparations fully confirms this supposition."[1]

This is upon the whole the opinion most generally entertained by practical physicians.

The employment of the *seeds* in preference to the *root* was first insisted on by Dr Williams of Ipswich.[2]

There are at present seven preparations of the bulb and seeds of COLCHICUM officinal in the three British Pharmacopœias; but of all these there are perhaps only four which can be considered as in use at the present day. These are,—*Acetum Colchici*, E. L. D.; *Extractum Colchici Aceticum*, E. L. D.; *Tinctura Colchici*, E. L.; and *Vinum Colchici*, E. L. The *acetic* forms answer best as *diuretics*, perhaps because they are weaker preparations, and not so apt to act upon the bowels.

The *tincture* and *wine* are the preparations employed for general purposes; but when it is desirable to exhibit COLCHICUM in the form of pill, the *acetic extract* is certainly the most eligible form.

PHYSIOLOGICAL EFFECTS.

It is very difficult to state in a precise and definite manner what the action of COLCHICUM is, or to what class of medicinal agents it ought to be referred.

In medicinal doses it seems to have the property of lowering the heart's action, and to affect most of the secretions of the body; and hence, according to the circumstances under which it is administered, it may produce *diuretic*, *emetico-cathartic*, or *diaphoretic* effects.

It has been regarded by some as an *expectorant;* and by others it is supposed to have a peculiar effect in stimulating the *hepatic system.*

It has also been regarded as having a powerful influence over the *uterus.*

From the marked effects which it produces in GOUT and RHEUMATISM, it has been regarded as possessing a *specific* action in those diseases; but this may be said to be a mere statement that it acts powerfully and successfully, for it does not appear that it often alleviates those diseases, without producing in a well-marked degree some of its ordinary physiological effects, such as lowering the pulse, causing *diarrhœa*, *diuresis*, or *diaphoresis.*

In large doses there can be no doubt that it is an active *narcotico-acrid* poison; but its action seems to be more due to *acrid* than to *narcotic* properties, as the effect on the brain is generally secondary to that on the intestines.

One of its most remarkable physiological effects was discovered

[1] Cyclopædia of Practical Medicine—Gout.

[2] Williams—London Medical Repository, vol. xiii.

by Chelius of Heidelberg.[1] He found that the URIC ACID contained in the urine of those taking COLCHICUM was nearly doubled in the space of twelve days. In one case the urine before taking COLCHICUM contained 0·069 per mille of URIC ACID; four days after commencing to take the COLCHICUM the proportion was 0·076; on the eighth day, it was 0·091; and on the twelfth day, it was 0·102. Chelius obtained the same results in other instances.

Dr Christison[2] examined the urine of a patient taking COLCHICUM, and he found that in two days the quantity of UREA was nearly doubled. In the urine before taking COLCHICUM there was no deposit of *lithate of ammonia*. Its density 1020. It contained above forty-seven parts of solid matters in the thousand, and of this quantity twenty parts were UREA. The specimens of urine passed on the first and second days after commencing to take COLCHICUM were exactly alike. They were very turbid, and their turbidity disappeared with a gentle heat; the deposit was evidently *lithate of ammonia*. The density of the first was 1033·5, and that of the second was 1034, which are both very unusually high for urine not *diabetic*. As they were obviously identical in their nature, Dr Christison only analysed the second. It contained seventy-nine parts of solid matters in a thousand, and of this quantity thirty-five were UREA. Dr Christison suspected that the quantity of urea was even greater, for not having added an excess of *nitric acid*, some of the *nitrate of urea* might have remained in solution.

Through the kindness of Dr Halliday Douglas, I had an opportunity of examining the effect of COLCHICUM on the urine of a sailor, who was a patient in the Royal Infirmary. He was under treatment for secondary syphilis, but was otherwise healthy. I was permitted to give him a few doses of COLCHICUM, in order that I might ascertain the physiological action of that agent on the kidneys, but before doing so I examined his urine. The density was 1025. It contained no deposit, nor was it affected by *heat* or *nitric acid*. It contained :—

Total solids, ...		27·500
Water, ...	972·500	
Urea, ...	12·360	
Uric acid, ...	0·281	
Inorganic salts, ...	7·436	
Organic matter, ...	7·423	
Total, ...	1000·000	

[1] Chelius—Archives Générales de Médecine, tome xviii. Chelius—Annales Cliniques de Heidelberg, 3me vol. *Vide* paper by Dr Lewins—Edinburgh Medical and Surgical Journal, vol. lvi.

[2] Christison—Lewins, op. cit.

Here it will be perceived that both the UREA and URIC ACID were slightly deficient, if we compare it with the standard of healthy urine, as given by Becquerel.[1] Density 1018·9 Contains :—

Total solids,		31·185
Water,	968·815	
Urea,	13·838	
Uric acid,	0·391	
Inorganic salts,	7·695	
Organic matter,	9·261	
Total,	1000·000	

On the third day, after commencing to take COLCHICUM, the urine was examined. It possessed a slight turbidity, which, however, was dissipated by heat. Density 1030. It contained :—

Total solids,		29·650
Water,	970·350	
Urea,	15·500	
Uric acid,	0·491	
Inorganic salts,	6·350	
Organic matter,	7·209	
Total,	1000·000	

Here, it will be observed, the UREA was increased by *one-fourth*, the URIC ACID nearly *doubled*, and the inorganic salts and inseparable organic matters were considerably decreased.

The urine was again examined on the sixth day after commencing to take the COLCHICUM, with the following results. Turbidity rather increased. Density 1034. It contained :—

Total solids,		33·460
Water,	966.540	
Urea,	18·341	
Uric acid,	0·750	
Inorganic salts,	7·436	
Organic matter,	6·933	
Total,	1000·000	

Here, then, the physiological action of COLCHICUM in increasing the UREA and URIC ACID was well marked.

Having obtained these results from this case (which are only corroborations of many others), it was not considered justifiable to proceed further with the administration of COLCHICUM with this patient.

It has been supposed that under the use of COLCHICUM a remarkable change takes place in the system,—namely, that the URIC ACID becomes converted into UREA ; but this has not at all been substantiated, and from the above cases of Chelius, and the analyses which I have just noticed, we must be led to suppose that

[1] Becquerel—Semeiotique des Urines.

no such change occurs, but that an increase in both those principles is the result.

Dr Graves[1] states, that the beneficial action of COLCHICUM is not owing to its producing a more rapid excretion of *lithates* through the kidneys, but to the remarkable property the plant possesses of altogether putting a stop to the morbid formation of *lithates*.

Dr Gairdner[2] says, that he has always found that the increase of UREA was accompanied by a corresponding diminution of the *urates* in the urine. But, from the above experiments, I am inclined to believe that both of these suppositions are erroneous.

With a view to ascertain the power of COLCHICUM as a *sedative* I made the following experiments,—in the first, my pulse being 87; in the second, 84. On both occasions twenty minims of *Tinctura Colchici* were taken.

At 8 P.M.	...	...	Pulse 87	At 6 P.M.	...	...	Pulse 84
9 „	...	...	„ 87	7 „	...	...	„ 84
10 „	...	...	„ 80	8 „	...	...	„ 78
11 „	...	...	„ 75	9 „	...	...	„ 72
11½ „	...	...	„ 70	10 „	...	...	„ 66
12	...	...	„ 65	11 „	...	...	„ 60
12½ A.M.	...	...	„ 65	12	...	...	„ 62

No other physiological action was manifested, except slight nausea.

POISONOUS ACTION.

In large doses, COLCHICUM proves a violent *irritant* poison. The symptoms occasioned by it are,—"Severe vomiting and urgent diarrhœa, with a sense of burning in the throat, excessive colic, and heat in the abdomen; great depression of the circulation, and sometimes suppression of urine.

"Sometimes no other symptoms exist, and death takes place from *exhaustion*, the effect of *inflammation* of the bowels. Sometimes death is preceded by *headache*, *delirium*, *stupor*, and *insensibility*, denoting an action upon the *nervous system*." (Christison.)

POISONOUS ACTION ON ANIMALS.

Orfila[3] states, that he has frequently given two or three bulbs to dogs in *June*, without producing any sensible effects; this induced him to believe that climate and the season of the year have great influence on their deleterious properties. This, however, is the season of the year when the bulbs are supposed to possess their greatest activity.

---

[1] Graves—London Medical Gazette, vol. vii.
[2] Gairdner on Gout. London, 1849.
[3] Orfila—Toxicologie, vol. ii.

Mr Want states, "that cattle are affected by it only at the spring of the year, when the seed-vessel is fully mature."

"It happens that the seed, if swallowed, adheres to the coat of the stomach, producing at the several points of its adhesion spots of inflammation, which occasion the death of the beast."

"It is a curious fact, that they are affected by the recent plant only; for when dried and made into hay it loses its deleterious property, and is then eaten by them with impunity."[1]

Sir Everard Home[2] injected 160 drops of the vinous infusion of COLCHICUM into the jugular vein of a dog. The animal immediately lost all power of voluntary motion; the breathing became extremely slow, and the pulse was hardly to be felt.

In ten minutes the pulse was 84; inspirations natural (40 in a minute). In twenty minutes the pulse was 60; inspirations 30; and a tremulous motion had taken place in the hind legs. In an hour the pulse was 115, and irregular. The animal was capable of sitting up, but was in a state of violent tremor; the inspirations could not be counted. In one and a half hours, the tremor had gone off; pulse the same; the animal made ineffectual attempts to vomit, and continued to do so for ten minutes, with great langour; inspirations, 54. In two hours the pulse was 150, and very weak; the animal had voided an ounce and a half of urine; had vomited twice, each time bringing up a quantity of mucus, tinged with bile, and had two liquid stools. In three hours had vomited again, and had another stool; pulse too weak to be counted. In four hours continued extremely languid; and in five hours vomited some bloody mucus, and expired.

Autopsy.—The *stomach* contained mucus tinged with blood, and its internal surface was *inflamed.* The *duodenum* had its internal surface universally *inflamed.* The same appearance was met with in the *jejunum* and *ilium*, and also in the *colon*, where it was more strongly marked than in the *ilium.*

Another case, with somewhat similar results, is recorded by Sir Charles Scudamore.[3] He injected 160 drops of the vinous infusion of COLCHICUM into the jugular vein of a strong dog. For the first fifteen minutes he did not seem to suffer the least inconvenience. In an hour and a quarter he was sitting on his hind legs, the eyes were bright, and there did not appear any remarkable change. In five hours he was still capable of sitting up, but appeared much dejected; and there was some difficulty of breathing, attended with an occasional husky cough. His pulse was 113; the inspirations were 56. In five hours and a quarter he vomited some bloody mucus, and expired.

---

[1] Want—London Med. and Phys. Journ., vol. xxxii.; Annals of Philosophy, vol. iv.

[2] Philosophical Transactions, vol. xvi., part 2.

[3] Scudamore on Gout and Rheumatism, 1819.

Dissection.—The *stomach* was in a state of *gangrene.* The *duodenum*, *jejunum*, and *ilium*, were in a high state of *inflammation*, approaching to *gangrene.* About two ounces of highly offensive grumous blood were found in the stomach; there was also blood in the duodenum, but not offensive. The *colon*, *cœcum*, and *rectum* were much *inflamed;* with here and there deep rose-coloured spots, of the size of a pea. The bladder was nearly full of urine, of a deep saffron colour; the gall-bladder was much distended with bile. There was an effusion of bile upon the liver.

Scudamore made an experiment also with the expressed juice of COLCHICUM.

At forty minutes past 1 o'clock P.M., 120 drops of the expressed juice of the fresh roots of COLCHICUM were injected into the jugular vein of a young terrier. Immediate signs of great debility were produced; he passed a natural alvine discharge.

In about two minutes he vomited half an ounce of frothy bile, of a gamboge colour. Rising upon his legs he staggered as if intoxicated, and immediately fell. In five minutes the respirations were 52. In seven the eyes were fixed, the pupils contracted; the extremities were stiff, and drawn up.

In ten minutes he was upon his legs, passed a scanty bilious discharge, walked about, and soon again made great efforts, without effect, to relieve himself, uttering at the time a cry of distress. He ran into a dark part of the room. In lying down he had some rigors, and there was a profuse salivation.

Three o'clock.—He was on his legs, with his back raised, and showing signs of pain in the bowels. The inspirations were 42. He seemed capable of walking, but was very languid.

At *four o'clock* the lassitude had increased, and he supported himself with difficulty on his legs; a copious discharge of mucus and saliva was proceeding from the mouth.

A little before *five o'clock* he appeared to be suffering much, was moaning continually, and took no notice on being disturbed.

At *six o'clock* he was still much distressed on his inspirations, being only 12 in a minute; the eyes were fixed, the teeth firmly closed, and he appeared to be dying.

At *seven o'clock* there was no perceptible change, except that his inspirations had increased to 14 in a minute.

Nine o'clock.—He respired only 8 times in the minute with much difficulty. His moaning was much fainter.

Ten o'clock.—He was found dead, and was quite cold.

Dissection.—The *stomach* was highly *inflamed*, containing about an ounce of dense mucus, mixed with grumous blood; the *duodenum*, *jejunum*, and *ilium*, were highly *inflamed;* the inflammation decreasing in the *colon*, *cœcum*, and *rectum.*

The following experiment with *Eau Médicinale*, whose principal constituent is supposed to be some preparation of COLCHICUM, probably the *flowers*, is narrated by Scudamore:—

Sixty minims of Eau Médicinale, with the sediment which it forms shaken up, were given to a very strong rough terrier, at half-past ten A.M.

Two o'clock.—He was lying down, and looked lethargic.

Half-past Four.—Pulse 96, vibrating and intermitting every five beats.

Eight o'clock.—Pulse was softer; he had vomited some frothy mucus, and appeared very languid.

Next morning.—Pulse 164, and irregular. The dog had recovered his strength.

Ten o'clock.—A further dose of 160 drops was given.

Half-past Two.—He looked dejected; the pulse was 104, and very irregular.

Four o'clock.—Had brought up some opaque, viscid mucus.

Six o'clock.—Had vomited a quantity of frothy slime mixed with blood, and appeared altogether very ill. Rigors; pulse 80, and small, with intermissions after every five or six beats.

Half-past Nine.—Seemed dull and languid; pulse 106.

On the following morning, at ten o'clock, he was found extended on the ground; had voided a quantity of urine. He was quite insensible, and now and then stretching out his limbs. His inspirations were 6 in a minute.

At *one o'clock* he did not seem to suffer.

Half-past Two.—He was stretching himself out, as if in the act of dying. There were slight convulsions of one of the legs. The pulsation of the heart was not to be felt; and no distinct respiration could be observed.

At *three o'clock* he had expired.

Dissection.—The *stomach* was highly *inflamed*, and contained a dark-brown fluid. The marks of *inflammation* increased in the *duodenum*, and through the *jejunum;* decreased in the *ilium*, and increased again in the *colon*, which appeared in a state of general ecchymosis, from venous blood extravasated under the mucous membrane. The *cæcum* and *rectum* were highly *inflamed.*

Various other experiments with the different preparations of COLCHICUM are noticed in Scudamore's work on gout, but these it is unnecessary to detail.

Dr R. Lewins,[1] in an essay on COLCHICUM, describes several experiments which he made on the lower animals with different preparations of that plant.

On the 15th December, 70 minims of the *Vinum Seminum Colchici* were administered to a middle-sized dog, without any immediate sensible effect.

A short time afterwards it devoured a large quantity of meat very greedily, and continued gnawing bones for some time with great avidity.

---

[1] Lewins—Edin. Med. and Surg. Journ., vol. lvi.

At *seven* P.M., nearly four hours after he had swallowed the COLCHICUM, he ate another large quantity of beef, and lapped barley-broth with great eagerness. He was visited for the last time that night about *nine* P.M., when he appeared in his usual state of health.

Next morning, at *half-past eight*, sixteen hours after the COLCHICUM had been given, he seemed weak, low, and very sick. He remained prostrate on some straw in the corner of the room, and could not be induced to move. During the night he had been most severely purged, and in every part of the room were egesta from the stomach, in some places mixed with a fluid of a dark brown colour, in other places mixed with food; and near where he was lying there were large watery evacuations, intimately mixed with a fluid resembling blood. No attempts were made to vomit during the visits on this day, nor did he express by outward symptoms any indication of suffering acute pain. The eyes were hollow, and had a dull glazed appearance. On presenting food of the same kind that he devoured greedily the previous afternoon, he would not touch it. The prostration of strength and insensibility to external impressions became greater and greater throughout the day.

On visiting him next morning, at a little after *eight o'clock*, he was quite dead, cold, and stiff. The food which had been placed before him yesterday morning had never been touched.

Dissection.—The body, examined at *half-past eleven*, presented the following appearances:—The *intestines* were found very much *contracted* and *vascular*. The internal surface of the *stomach* presented rather more rugæ than natural, and was lined with mucus; its cavity was nearly full of dark brown coloured bile, and the pyloric orifice was contracted. The *duodenum* was much injected, and coated with mucus; the whole course of the *jejunum*, and upper part of the *ilium*, were of the most intense red colour. On the lower part of the *ilium* were observed a number of dark streaks, slightly raised, running in a longitudinal manner. The *large intestines* were likewise very *vascular*, although not so vivid as in the small intestines. In the *ilium* a large tapeworm, measuring considerably upwards of a foot, was found dead. There was not the slightest appearance of food or fœcal matter in any part of the intestinal canal; a large quantity of bloody serosity, mixed with thin mucus, lined the parietes in almost their whole extent. The *gall-bladder* was full of dark-brown bile, and the *urinary bladder* was distended with natural coloured urine. All the other organs were perfectly normal.

Dr Lewins found that, notwithstanding the powerful effects which COLCHICUM produced upon men and dogs, it acted with very different degrees of energy upon other classes of animals, and with the view of ascertaining in what respect this action is different, he made the following experiments:—

On 7th December, half a drachm of the *Vinum Seminum Col-*

METAMORPHOSIS OF THE BLOOD CORPUSCLES Plate 2

Fig 1

Fig 2

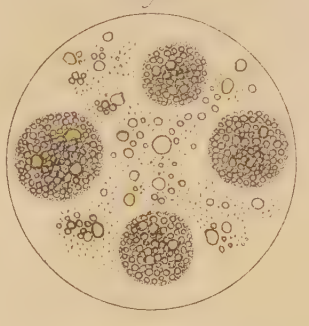


Fig 3

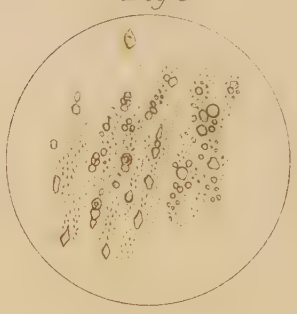


Fig 4

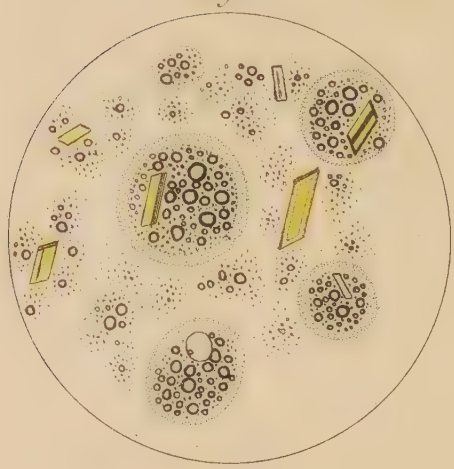


Fig 7

Fig 6

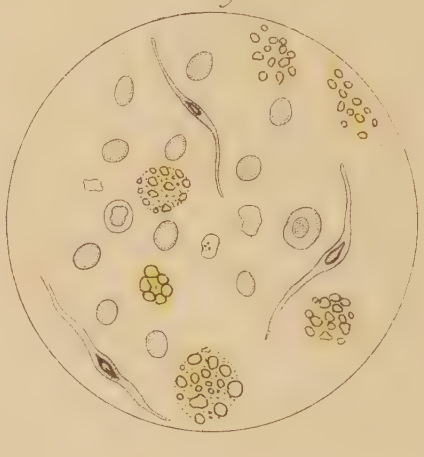


Fig 8

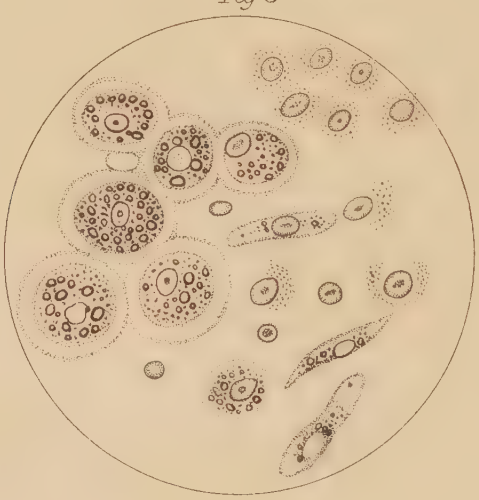


Fig 5

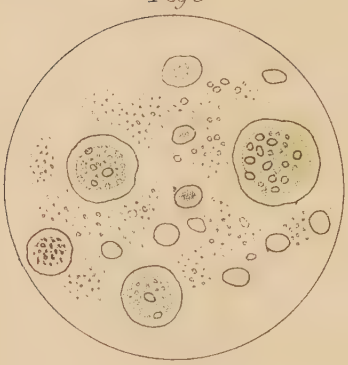


Fig 9

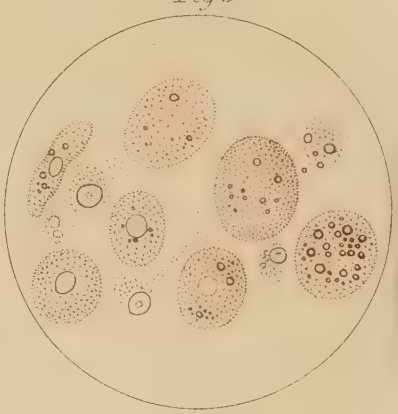


Fig 12

Fig 10

Fig 11

chici was administered to a small rabbit. It commenced eating immediately afterwards; and on the following day nothing unusual was observed, except that it appeared to have passed a larger quantity of urine than natural.

On the 8th, at *six* P.M., twenty-four hours after the last dose, one drachm was administered, and with the exception of a *diuretic* effect, no particular change was observed.

On the 9th, no perceptible effect having resulted from yesterday's dose, at *nine* P.M., twenty-seven hours after the last experiment, two drachms more were swallowed, and no evident change was observed in the animal.

On the 10th, at *six* P.M., twenty-one hours after the last dose, three drachms more were given. After one drachm had been introduced, the rabbit began to squeak, and struggle violently; and on being let loose ran about as if in pain. The same effect followed after the exhibition of the second dose; but after the third drachm it began to eat, and appeared quite as lively as usual.

Thus to a very small animal, in the course of four days, *six drachms and a half* were given (more than six times the quantity which proved fatal, with the most violent symptoms, to a dog), with little effect except acting upon the kidneys.

Another case is mentioned, where *seventeen drachms* were given in *six* days, without any material change being produced on the animal. An experiment was also made on a cold-blooded animal,—viz., a frog. In this case, in the course of *six* days, 350 *minims* were administered, with comparatively very slight effects being produced, and which might very well be attributed to the quantity of wine swallowed.

From the results of these investigations, Dr Lewins says, it appears that we are warranted in concluding, that the action of COLCHICUM is most decided upon carnivorous and omnivorous animals, whilst its action on herbivorous, graminivorous, and cold-blooded animals is comparatively feeble.

(*To be continued.*)

ARTICLE III.—*On the Metamorphosis of the Coloured Blood-Corpuscles and their Contents in Extravasated and Stagnant Blood.* By JOHN SCOTT SANDERSON, M.D., Vice-President of the Physiological Society of Edinburgh. — (*Continued from p.* 229.)

IN my former communication, were contained a series of observations relative to the origin and structural peculiarities of the so-called "cells containing blood-corpuscles," along with which were

included some speculations as to their physiological and pathological import. Those ulterior changes which have been made the subject of such careful observation by Professor Kölliker, as occurring in the parenchyma of the spleen, were also referred to, and towards the termination of the paper the attention of the reader was drawn to the rather important consideration, that the phenomena in question were not peculiar to that organ, but were observable in all organs and tissues whatsoever, when placed under the same *pathological* conditions.

All that is contemplated at present is, to continue the illustration of the same subject, by adding to the facts already related, and to state more in detail the general conclusions to which they lead.

(*m.*) We have already seen that the form which the contents of the coloured blood-corpuscles in extravasated and stagnant blood most frequently assume when surrounded by living tissues is that of yellow molecular pigment. To assure ourselves that this metamorphosis is not dependent for its production on cell-structures, we need only reflect on the fact, that all its stages are discoverable in situations where no such structures exist. Examples may be frequently met with in which such pigment, presenting its usual characteristic appearances, is observable in coagula which have for some time occupied any of the great cavities of the body, and in which, at the same time, little or no traces of commencing organisation can be detected.

Intermediate between such cases and those in which the perfectly formed cell-like bodies are observed we have a third class, in which part of the granular pigment which exists remains in the free condition, while the rest is aggregated into conglomerates, approaching more or less to the spheroidal form.

Thus in an apoplectic cyst, measuring about three inches across, which was removed from the brain of a middle-aged man, and which, as well from the history of the case as from the firm consistence and well-defined external surface of the tumour, seemed to have been of considerable duration, the following observations were made :—

The contents consisted of a soft pulp of blood-globules, which were most of them in a state of dissolution preparatory to absorption, along with cholesterine crystals and flakes of broken-down fibrin. The cyst itself consisted of an external firmer portion, about a quarter of a line in diameter, which was perfectly colourless, and an internal softer and more friable layer, of a pale yellowish-pink colour. The external layer was found to be composed of smooth bands of fibrillated substance, running more or less parallel with each other, in which were embedded numerous minute granules, but no pigment. These did not present the appearance of fully formed white fibrous tissue, nor did they display any nuclei, or so-called nuclear, fibres.

The internal layer presented under the microscope a homogeneous gelatinous-looking substratum, without appreciable structure, in which numerous fine granules were also embedded. Besides these, a considerable number of conglomerates, of various sizes from $\frac{1}{1500}$th to $\frac{1}{1000}$th of an inch in diameter, were observed, which consisted of yellow granular pigment. (See Fig. 1.) None of these were surrounded by a membrane, and could only be considered as attempts at the more distinct formations observed under similar circumstances in other situations.

In a case of acute softening of the corpus striatum, which fell under my observation during last winter, in which the posterior three-fourths of that body at its outer border were so far affected as to be almost diffluent, imperfect conglomerates, such as those above described, and well-formed cell-like structures, also containing yellow molecular pigment, occurred simultaneously. At one part of the softening where it was most diffluent, a few masses of inconsiderable size, but which possessed a firm consistence, and a bright yellow colour, were observed. On examining microscopically the creamy fluid obtained from this part, the following elements were found, —namely, 1st, Compound granular corpuscles, $\frac{1}{1500}$th to $\frac{1}{1000}$th of an inch in diameter, and containing oleo-albuminous granules, of very various sizes. 2d, Similar corpuscles, containing in addition finely granular pigment, of a bright yellow colour, diffused among the granules last alluded to. 3d, Similar corpuscles, containing, besides both the above constituents, pigment grains, spherical or spheroidal in form, and of a brick-red colour. These last appeared to be altered blood-corpuscles. 4th, Free oleo-albuminous granules and pigment. (See Fig. 2.) It was observed that the red pigment always assumed the form of the spheroidal grains, like contracted blood-discs, while the yellow was always in the amorphous or finely molecular condition. Both forms were unaffected by acetic acid, caustic potash, or water. On examining the firm yellow material above alluded to, it was found to consist of a resistent substratum, resembling some forms of coagulated fibrin, in which were embedded oleo-albuminous granules and pigmentary matter. It might probably be considered as an indication of the former presence of an apoplectic cyst in the situation in which it occurred. (See Fig. 3.)

Comparing the above with what was related in another section as having been observed in the brains of pigeons, in which extravasations had been produced artificially, there can be little doubt that the cell-like bodies, including pigment, had originally included blood-corpuscles in the unaltered state, which had undergone a series of changes analogous to those actually observed in the pigeons, and had assumed the appearance which they presented on the death of the patient. The peculiar forms assumed by the pigment will be again alluded to.

To the above I must be allowed to subjoin the following observa-

tion, which is related by Carl Bruch in his memoir on pathological cyst-formations :[1]—

The patient who was the subject of the observation had, three months before his death, received a severe blow on his left testicle, which was attended with much pain and swelling. On post-mortem examination, the organ and epididymis of the same side were found to be healthy. The *tunica albuginea* was unaltered, but between it and the *tunica vaginalis* there was found in contact with the former, and pressing slightly upon it, a clot of the size of a nut, and of a brownish-red colour, with a yellowish firm peripheral portion, and a pulpy broken centre. The former displayed a concentric arrangement of laminæ, and was about a line in thickness, blending externally with the surrounding loose cellular tissue. Besides this larger cyst, a smaller one was found, which resembled it altogether, except in containing nothing but yellowish serum.

Microscopic examination showed that the central portion, which was of very friable consistence, was formed of a loose network of fibrinous material, including numerous blood-corpuscles and nuclei, along with masses of yellow pigment, and clump-like bodies as large as pus-corpuscles, which were rendered clearer, but not dissolved by, acetic acid. Many of the blood-corpuscles had become insensible to the action of acetic acid, and all stages of transformation between them and the pigment granules were seen. The fibrine displayed less and less organisation as you passed in examining it from the peripheral to the more central and fluid parts, which consisted of altered blood-corpuscles only. The external portion, on the contrary, consisted of concentric layers of fibrine, the blood-corpuscles having disappeared, of which the most internal displayed no nuclei, but simply a net-work of flattened fibres, while those most external, and consequently most developed, possessed long small nuclei and distinct isolated fibres, the characters of which blended insensibly into those of the normal tissue of the part.

The above description is peculiarly interesting, not only because it affords us an excellent illustration of the whole process of the formation of the yellow molecular pigment, but because in the smaller cyst is shown the ultimate result which invariably happens in similar circumstances, namely, the conversion of the whole into a serous sac.

(*n.*) I shall next proceed to describe another kind of sanguineous cyst, differing from the last in some important particulars, and, among others, in exemplifying another mode of transformation of blood-pigment,—that in which it appears to combine with a crystallisable material to form coloured crystals. (See former paper, section (*i*).)

---

[1] Carl Bruch, Zur Entwickelungsgeschichte der pathologischen Cysten-bildungen. Zeitsch. für rat. Med. VIII. Bd. s. 91.

On removing the scalp, for the purpose of examining the brain of a pigeon, I observed, embedded in the intervening loose cellular tissue and connected with it, a tumour, about the size of a millet-seed, and of tolerably firm consistence. On removing and examining this tumour, I found it to present the following structure:—It consisted of two parts. The external portion, or cyst, was of a dull yellowish colour, and resistent texture. Under the microscope, it presented the structure of fully-developed wavy white fibrous tissue, with a very few of the cell-like bodies to be next described. The internal portion, or nucleus, was of softer consistence, and of a dark sap green colour. It contained the following elements:—

First,—Cell-like bodies. These were of a round, oval, or occasionally irregular, form. They presented the appearance of a distinct membrane, and well-defined margins, and measured from $\frac{1}{2200}$th to $\frac{1}{750}$th of an inch in diameter. In almost all, a vesicular clear nucleus was visible: besides which they contained,—1st, Highly refractive oleo-albuminous granules, perfectly spherical, and apparently colourless, of very various sizes. 2d, Amorphous molecular matter, in various-sized masses, and of a yellowish-green colour. 3d, Coloured crystals. These crystals not only occurred included in the cell-like bodies, but in great numbers in the free state. They were of a deep yellowish-red colour, and of a rhombohedral form, the angles formed by their sides being, as far as my means of measurement allowed me to judge, about 118° to 62°. They were remarkably regular in their form, and it was observed that, while the thinner examples presented more of the yellowish tinge, in the thicker the red was more prevalent. (See Fig. 4.)

In the second place, all the above-described bodies were also found in large quantity in the free state.

At that part of the tumour where the external and internal portions blended together, *transition* stages between the cell-form and the fibre were observed. These occurred in the form of fibres, which, while they contained the green granules above described in their central portion, had split into fibres at their ends. This circumstance would seem to point, not only to a similarity in mode of origin in the two forms of structure, but probably to a similarity in chemical composition. A sufficient number of similar observations will, at some future period, probably enable us to connect together the cell-like bodies which we have been considering with another group of structures,—namely, the so-called "fibre-cells," which are produced by the process of "splitting up" of coagulated fibrine, as described by Professor Henle, and which ought perhaps as little to be considered as actual cells as the others.

There can be little doubt that the tumour above described actually originated in an extravasation in the lax areolar tissue of the part. In numerous cases, however, extravasations take place into the cavities of previously existing cysts, the resulting coagulum undergoing changes precisely similar to those to which reference has been seve-

ral times made above. Thus, in the cysts which occur so frequently under various circumstances in the cortical substance of the kidney, it is very common to find evidences of extravasation either in the form of more or less altered blood-corpuscles in a free state, or, on the other hand, of more or less cell-like structures, containing such altered blood-corpuscles or granular pigment. In Fig. 5, I have represented cell-like structures similar to those which we have before considered, derived from the kidney of a patient who came under my observation last winter. They correspond in most respects with description and figure given by Virchow of the contents of a similar cyst, and seem to require no special description.[1]

(*o.*) If we rightly direct our inquiries as to the occurrence of pigment derived from the blood in the spleen, viewing it from the first as a pathological phenomenon, several facts worthy of notice present themselves. In that most common condition of the organ to which the term "hemorrhagic infarction" has been applied, which occurs in some forms especially of typhus fever, and in the pernicious intermittent fevers of hot countries, as well as in connection with certain other acute dyscrasic diseases, the tissues of the organ are often eventually destroyed, and the whole reduced to a pultaceous mass, enclosed in the fibrous capsule. In most cases, from our only seeing them in their recent stage, the changes which the blood-corpuscles undergo are not observed. In others, however, principally owing to the circumstance that frequently repeated congestions, attended with hemorrhage, have taken place, these may be seen without any difficulty. Thus in every case of the chronic tumour of the spleen, so well known as a permanent result of ague, such indications may be expected, inasmuch as every such tumour, in its acute stage, has presented a hemorrhagic character. In Fig. 6, I have given the microscopic appearances which such a spleen presents. Numerous spherical or spheroidal aggregate bodies are represented, which appear to be held together by a transparent matrix. None of these are surrounded by a distinct membrane-like margin, and their forms are irregular. The pigmentary grains are evidently broken down blood-globules, with which some of them correspond in size.

Metamorphoses of the Contents of the Coloured Blood-Corpuscles in the Corpus Luteum.

(*p.*) In accordance with the doctrine first promulgated by De Graaf, and since supported by the authority of Professor Henle and others, that *corpora lutea* originate from the transformation of blood which is effused into the Graafian follicles, it occurred to me that in them the best opportunities would be afforded for examining the class of changes which are the subject of this memoir. On inquiring into the evidence on which this doctrine is founded, and making the requisite observations, I soon began to discover that it was untenable,

[1] Virchow—Archiv. für path. Anat. und Phys. Bd. I. s. 387.

and that all the steps of a process of development, quite different from that which it supposes, could be traced without difficulty. De Graaf's theory owed its origin to the undoubted fact, that in the ovary of the human female, as well as in those of some of the lower animals, an extravasation of blood into the cavity of the Graafian vesicle takes place to a great extent at or about the period of the escape of the ovule. That this is not in these animals an essential accompaniment of the process, seems indicated by the observations of Bischoff, who has asserted its absence in the rabbit and dog.

In relation, however, to the history of the changes which this extravasated blood undergoes, in those instances in which it does exist, there are certain circumstances which afford illustrations of several peculiar modifications of the process of the metamorphosis of the contents of the blood-corpuscles, which are not the less interesting as being less essential to the development of the *corpus luteum* than they were supposed to be by De Graaf. With respect to these, I shall therefore detail some of my observations in conclusion.

If the Graafian vesicle be examined in the sow at a period when it is not fully developed, it will be found that the albuminous fluid which it contains, when submitted to the microscope, displays several elementary organic forms. Of these the most frequent in occurrence is that of round, ovoid, or fusiform particles, of about $\frac{1}{2500}$th of an inch in diameter, finely granular in their appearance, and each enclosing a vesicular nucleus little less than themselves. These bodies are observed sometimes to swim free in the fluid, but more frequently they connect themselves together in the form of laminæ or flakes of membrane. Besides, there occur occasionally large spheroidal cell-like bodies, which possess a nucleus of the same size and appearance as those last-mentioned, and are likewise finely granular. On examining the wall of the Graafian vesicle, it is found to be lined by several layers of particles which altogether resemble those which are described above, and are connected so as to form a continuous tissue, which corresponds to what is termed by anatomists the *membrana granulosa*. This cannot, however, be properly considered as having any existence as a distinct membrane; for, in continuing the observation outwards, it is found that external to it other layers exist, in which the form of the elementary particles, as well as of their nuclei, undergoes a gradual change, the spheroidal or ovoidal form which occurs in the more internal layers insensibly passing into the spindle form, displayed in the more external.

The structure of the outer part of the wall of the vesicle constitutes a fibrous tissue, which cannot be distinguished from that of the stroma of the ovary in the neighbourhood; and between this and the epithelium-like structure of the internal lining of the vesicle the transition is perfect.

The external fibrous layer is richly supplied with vessels, the minute capillaries of which ramify in an irregular net-work in its substance. By the internal layer the albuminous fluid is secreted

which the vesicle contains. It frequently projects at intervals in the form of villi, or folds into the cavity, and imbedded in one of these the ovule may sometimes be detected.

A short time before the period of the expulsion of the ovule, the wall of the Graafian vesicle becomes much congested, and the structures of which it consists undergo further development. In the substance of the membrane towards its internal aspect, very numerous nuclei are developed, each of which presents the appearance of a delicate vesicle. These, when fully formed, are about $\frac{1}{3500}$th of an inch in diameter, or rather larger. They contain, for the most part, two or three granules in their interior, and are but little affected by acetic acid. Around many of these finely granular material is observed to be accumulated in greater or less quantity. This is in some cases indefinitely arranged, the constituent granules being in contact with the nucleus-vesicle on one or two aspects only, while in others it is completely surrounded, and the granular matter assumes a more or less spherical form. Besides these, delicate cell-like bodies are observed in great numbers, each of which includes a nucleus altogether similar to those described above. These bodies, which eventually constitute, by their multiplication, the whole substance of the *corpus luteum*, consist, in their early condition, in the ovary of the sow, of a colourless, gelatinous-looking, intermediary substance or matrix, in which are included at intervals a few minute highly-refractive granules. Like the smaller particles of which the *membrana granulosa* in the Graafian vesicle, before the thickening of its wall, consists, these bodies manifest a tendency towards the outer part of the membrane to assume the character of fibre-cells; and all transitions may be observed between the spheroidal form already described, and ribbands and fibres of various degrees of elongation. (See Fig. 8.)

The cell-like bodies in question are not hollow vesicles containing a fluid, but solid spheroids. This appears from the consideration of several circumstances.

(1.) The appearance which these bodies present, when viewed by transmitted light, is the optical expression, not of a hollow vesicle containing a fluid less refractive than its wall, but of a flattened, solid spheroid, consisting of a homogeneous material.[1]

(2.) On rolling the bodies in question between the two plates of glass, which are employed in the examination, it is observed that

---

[1] In examining in water a transparent and homogeneous solid spheroid, and a transparent hollow vesicle containing fluid, after adjusting the microscope in such a manner as to obtain perfect definition, the following are the differences in the appearances observed:—In the solid body, supposing it to be more refractive than the surrounding medium, it will be observed that the margin, which is dark in both cases, appears less decidedly so than in the vesicle; and, in the second place, the darkness of the edge passes more gradually into the brightness of the central part, in the former than in the latter.

neither the nucleus nor the granules imbedded in the transparent matrix change their relative positions to each other.

(3.) When dilute acetic acid is added, it dissolves more or less completely the transparent matrix. This dissolution commences at the margin, and extends gradually towards the centre.

(4.) Certain circumstances to be detailed below indicate that the cell-like bodies grow by successive additions to their surface.

At the same time with, or shortly before, the expulsion of the ovule, there occurs, in the ovary of the sow, a considerable extravasation of blood into the cavity of the Graafian vesicle. It appears to me as if this extravasation may take place without the expulsion of the ovule, as in one ovary I found at least four Graafian vesicles filled with a recent coagulum, some of which were so far from the surface that no expulsion could have taken place. This fact is of importance, as showing that the extravasated blood cannot be, as supposed by many, derived from the ruptured vessels at the point at which the ovule escapes. However produced, the clot which results presents no special characters, except that it adheres, including the blood-corpuscles, to the internal surface of the *membrana granulosa*, and includes in its centre a cavity which is filled with serum. The adherence, however, is so feeble, that the whole may be removed by the forceps from the cavity in which it lies with the greatest facility. In the cow, although I have never been successful in obtaining ovaries immediately on the expulsion of the ovule, I have met with some in which the clot was but little altered. At a later period, in this animal, the cavity which exists in the centre of the completely-formed *corpus luteum* is lined by a pale-coloured membrane of rather tough consistence, and about one-fourth of a line in diameter. This membrane presents the structure of condensed coagulated fibrin, and is the decolorised remains of the extravasation which takes place at the expulsion of the ovule.[1]

Let us now consider those further changes which the thickened wall of the Graafian vesicle undergoes in being converted into the complete *corpus luteum*. In correspondence with the total absence of colour which we observe in the *corpus luteum* of the sow, the nucleated cell-like bodies of which it principally consists are formed simply of a transparent matrix, in which a greater or less number of fatty granules are imbedded. In the cow, on the contrary, they contain, a short time after the expulsion of the ovum, an abundant finely-molecular matter, of a light brick-red colour. This is always disposed with great uniformity through the transparent matrix, though more abundant in some cases than others. It is most so in the immediate neighbourhood of the central coagulum, whence we may assume it as probable that it owes its origin to the colouring

[1] Zwicky asserts that in the cow there is no trace of extravasation. This is contradicted by my observations.—(See Zwicky in op. cit., p. 28.)

matter of the corpuscles, which the coagulum in the first instance included.—(See Fig. 9.) At a somewhat later period we observe that a large deposition of fat takes place throughout the intermediary substance or blastema with which the cell-like bodies are surrounded. This appears most frequently in the form of minute amorphous granules, which are so minute as to make their colour imperceptible, except when they are accidentally aggregated into masses. They are extremely refractive, and dissolve on the addition of ether, to which they communicate a yellow colour. Less frequently the fatty constituent appears in the form of distinct spherical oleaginous, or more probably oleo-albuminous granules, which not only float free in the blastema, but occur in greater or less numbers involved in the substance of the solid cell-like bodies.—(See Fig. 12.) To one or other of the forms of this yellow fat, the colour of the *corpus luteum* seems to be invariably owing, so that when it is removed by treating the structure with boiling ether, it becomes completely decolorised.

In those *corpora lutea* in which the fat occurs in the finely molecular form, the cell-like bodies present a peculiar appearance, which depends upon its presence. As I have already indicated, they include at first only a finely molecular matter, as shown in Fig. 9. As they subsequently enlarge by the addition of new layers of the transparent intermediary substance to their surface, the fatty molecules become involved in these, so that we have in the *corpus luteum* of the cow the cell-like bodies so constituted that the central part of each is of a brick-red colour, while the external layers by which it is surrounded are loaded with yellow fatty molecules.

In some cases in the *corpus luteum* of the cow evidences of extravasated blood having been diffused throughout its substance are observable. Thus, altered and contracted blood-corpuscles, which are insensible to the action of acetic acid and water, often occur; and in some instances I have discovered large numbers of the coloured rhombohedral crystals, such as I have represented in Fig. 11. These appearances are by no means constant. It seems to me, however, extremely probable that the yellow colour of the fat which was alluded to above is also derived from the colouring matter of blood which has been extravasated.

In the human female, at the period of the expulsion of the ovule, as I have already hinted, there occurs a considerable extravasation of blood into the cavity of the Graafian vesicle. The changes which this coagulum undergoes, so far as my limited means of observation permit me to judge, seem considerably different from any which have hitherto fallen under our notice. It does not become decolorised for a much longer period; while the *corpus luteum* is correspondingly slow in becoming developed. In a *corpus luteum* which occurred in the ovary of a young female who had poisoned herself, and which, from the circumstances of the case, was calculated to be of from five to six weeks duration, the coagulum presented the following appearances on examination:—On section of the whole, it was found to

occupy a very considerable space in the centre. It was of a bright-red colour, similar to arterial blood, of tolerably firm consistence, and easily separable from the rest. On microscopic examination, it presented a substratum of fibrillated material (coagulated fibrin), which was otherwise structureless. Imbedded in this substance, besides decolorised blood-corpuscles in considerable numbers, there were others which were more or less completely converted into grains of pigment, spherical or ovoidal bodies about $\frac{1}{4000}$th of an inch in diameter, and of a brick-red colour. Besides these, there were crystals of a rhombohedral form, and very regular, but of small size.—(See Fig. 10.) Intermediate between these there seemed to be semi-crystalline bodies, as well as irregular masses, all of which consisted of the same brick-red pigmentary material, which was unaffected either by acetic acid, dilute mineral acids, or caustic potash.

The thickened wall of the Graafian vesicle, which was becoming developed into a *corpus luteum*, the width of which did not exceed a quarter of that of the whole vesicle, was separable from the coagulum by a distinct border. It presented, on examination, a substratum, which, towards the exterior, was becoming organised, and displayed fusiform nuclei. Fatty granules, to which it owed its yellow colour, were infiltrated throughout it. As seen under the microscope, these were, however, almost colourless.

EXPLANATION OF THE PLATE.

Fig. 1.—A transparent substratum, including conglomerates of yellow granular pigment from cerebral apoplectic cyst. To the right are seen crystals of cholesterine, contracted and altered blood-corpuscles, and granular pigment.

Fig. 2.—Conglomerates, including oleo-albuminous granules and pigment grains, which appear to be altered blood-corpuscles. Floating free are seen similar pigment grains, of a brick-red colour, along with yellow molecular pigment and oleo-albuminous granules. From softening of *corpus striatum*.

Fig. 3.—A portion of transparent coagulated fibrine, involving similar forms of pigment. From softening of *corpus striatum*.

Fig. 4.—Cell-like bodies, including oleo-albuminous globules, sap-green molecular pigment, and occasionally coloured crystals. All these elements are also seen in the free condition. From hemorrhagic cyst of scalp.

Fig. 5.—Cell-like bodies, containing yellowish-green molecular pigment. From cyst of cortical substance of kidney.

Fig. 6.—Conglomerates, including yellow pigment grains, probably equivalent to altered blood-corpuscles. From hemorrhagic infarction of spleen.

Fig. 7.—Coagulated fibrine, in which are imbedded oleo-albuminous granules and numerous coloured crystals. A small portion of white fibrous tissue is involved in the coagulum. From an artificial subcutaneous extravasation in a pigeon, five days after the infliction of the injury.

Fig. 8.—Various organic forms from the *corpus luteum* of the cow. On the left is a group of cell-like bodies, including nuclei and oleaginous granules, along with finely molecular pigment, of a brick-red colour. These are represented as they appear when slightly beyond the focal distance. Above are seen a group of nuclei imbedded in a granular blastema; these correspond to the general structure of the granular membrane in the earlier condition of the Graafian vesicle. (*n*) Nuclei either isolated or partly surrounded by aggregations of granular material. (*f*) So-called "fibre-cells."

Fig. 9.—Cell-like bodies, including brick-red molecular pigment. From *corpus luteum* of cow.

Fig. 10.—Coagulated fibrine with crystals, altered blood-corpuscles, and pigment. From human *corpus luteum*.

Fig. 11.—Granular matter, crystals and altered blood-corpuscles. From *corpus luteum* of cow.

Fig. 12.—Cell-like bodies, including brick-red molecular pigment and yellow oleaginous globules, along with fibre-cells, altered blood-corpuscles, and granules. From *corpus luteum* of sheep.

ARTICLE IV.—*Case of Poisoning with Corrosive Sublimate.* By F. THOMSON, M.D., Perth, Visiting Surgeon to the City and County Infirmary.

ALEXANDER BROWN, æt. 50, a brassfounder, of intemperate habits, was admitted into the Perth Infirmary on the 9th of July last, at nine A.M. Complained of severe and constant pain in epigastrium, which was increased on pressure; also of intense thirst, and great difficulty in deglutition and respiration. He had vomiting and bloody diarrhœa before admission, but none since. The pulse was soft and regular, but feeble. Temperature of the body natural; intellect entire. He presented all the appearances of one suffering from profuse salivation. The face was much swelled, especially the lower part of it, as also the submaxillary region. The interior of the mouth had a gray sloughing appearance; and the tongue and mucous membrane were very much swelled. There were also patches here and there of a deep violet colour, interspersed with the gray sloughing. The lips were of a deep colour, and there constantly flowed from the mouth a considerable quantity of a yellowish coloured saliva.

On account of the great swelling of the tongue and mucous membrane of the mouth, the state of the fauces could not be ascertained. On his admission, he stated he had taken arsenic, but it was afterwards found that he had swallowed two drachms of corrosive sublimate the previous day, about breakfast time,—viz., about twenty-four hours before admission. The poison was taken in substance, by licking it up with his tongue.

According to his statement, it appeared that in a short time (the exact time he could not tell) after taking the poison, severe pain in the mouth and throat came on, accompanied by swelling of the mouth and fauces, which rendered respiration and deglutition difficult; also a constant burning sensation in the stomach, with paroxysms of pain, vomiting, and bloody diarrhœa.

Between four and five hours after having taken the poison, he was seen by the surgeon of the police, who applied the stomach-pump, and gave him some milk, not being aware at the time what the poison was. At this time he was found with the face swelled, as described above.

The treatment after admission consisted in the administration of the white of eggs and milk, with some anodyne and leeches to allay pain. About half-past one A.M., the day after admission, the house-surgeon was suddenly called to see him, but when he reached the ward, found he was dead The nurse reported that he had not slept at all,—that the breathing had continued regular, and had not increased in difficulty till a few minutes before death.

Sectio, 36 *hours after death*.—The mouth, fauces, and superior part of the pharynx were much inflamed and swelled. They presented the same gray sloughing and violet-coloured appearances observed during life. The mucous membrane of the larynx was turgid and vascular. The stomach was of a bright red colour, but chiefly at the cardiac extremity. The mucous membrane of the duodenum, upper portion of the ileum, and various portions of the great intestines, were of a bright red colour. This appearance was most marked at the caput-cœcum and sigmoid flexure of the colon. The lungs were much congested, but crepitated on being cut into. The bronchial tubes were filled with frothy mucus; and the mucous membrane was highly injected; the liver, spleen, and kidneys were to all appearance healthy; the brain was not examined.

This case is a good example of the effects of corrosive sublimate in large doses. His having lived for nearly forty hours after having taken such a large quantity as two drachms, although no remedial measures were applied for several hours, may be accounted for by the poison being discharged by the early vomiting; and this was the more likely as it was taken in substance, and could scarcely have been all dissolved by the time the vomiting took place. It was quite evident, on admission to the Infirmary, that nothing could be done for him with any hope of cure. But notwithstanding the length of time which had elapsed from the period when he took the poison, he felt no small relief from the administration of the white of eggs.

It was not ascertained whether the flow of saliva observed in this case began when the swelling of the submaxillary region took place, or at a later period. There was no retention of urine.

Part Second.

REVIEWS.

Translation of the Pharmacopœia of the Royal College of Physicians of London, 1851; *with Notes and Illustrations.* By RICHARD PHILLIPS, F.R.S., &c. 8vo. London: Highley and Son. 1851.

A Translation of the New London Pharmacopœia, including also the New Dublin and Edinburgh Pharmacopœias, with a full account of the Chemical and Medicinal Properties of their Contents, forming a Complete Materia Medica. By J. BIRKBECK NEVINS, M.D. London, &c. London: Longmans. 1851.

A Compendium of Materia Medica and Pharmacy, adapted to the London Pharmacopœia, embodying the New French, American, and Indian Medicines; and also comprising a Summary of Practical Toxicology, &c. By J. HUNTER LANE, M.D., F.L.S., &c. Second edition. London: Churchill. 1851.

THE appearance of numerous translations and works explanatory of the London Pharmacopœia sufficiently testify to the absurdity of continuing to publish it in Latin. We have, however, already fully expressed our sentiments on this subject, and need therefore allude to it no further. No one need want a translation of the "Pharmacopœia Londinensis;" the difficulty lies in the choice; and our present object is to express our opinion as to the respective qualities of the books whose names head this article, for the benefit of those who have yet to make their selection. We have no intention of criticising any portion of the Pharmacopœia, as such.

No one was more entitled than the late Richard Phillips to be the translator of the London Pharmacopœia, or rather to be the editor of the English, in which doubtless it was originally written by himself, and from which it was done into Latin to suit the taste of the Royal College. He was one of those men, who, though they leave behind no renown for brilliant additions to science, are yet to be remembered with respect, on account of much quiet and useful work done by them. He did little in the way of original discovery or invention, but he laboured much as a corrector and expositor of what had been done by others, as the frequent reference to him in chemical and pharmaceutical works, and as the very book now before us, abundantly testify. It is true he was a little too confident in the value of the Pharmacopœia of which he was the author, and a little jealous of any commendations bestowed on other works of a similar nature; but we are ready to look with some indulgence

on any instance, even of misplaced, parental attachment, and we willingly forget the asperity of the critic, whilst we bear in remembrance the laboriousness of the chemist.

Mr Phillips was at the time of his decease engaged upon the work now under review, and had already put a portion of it to press. It has been allotted to his friend and former pupil, Mr Denham Smith, to complete the publication; and we feel bound to say, that under his editorship the work has in no respect suffered deterioration.

In the mere translation of the processes in the Pharmacopœia, this book is, as might be expected, clear and plain enough, and will, on this account, be infinitely preferred by those who practise pharmacy, to the Latin edition published by the College. It is, however, in the explanatory notes and remarks that the real value of Phillips' translation resides; and as these furnish a full and accurate account of the chemical actions and changes which occur in pharmaceutical processes, the book will thus be valuable to medical students. The statements regarding the medicinal actions and uses are hardly subject for criticism, inasmuch as Mr Smith tells us that he is not a medical practitioner, and therefore does not give them on his own authority. There are several articles of the Pharmacopœia which, in the edition of 1851, have been placed in the Materia Medica, and for which no processes are given by the London College, because they are generally prepared on the large scale by the manufacturer. We took occasion formerly to express our disapprobation of this measure. Mr Smith, conceiving that the giving of processes for these might be useful, not only to such as may be desirous of preparing their own chemicals, but to the student of operative chemistry and pharmacy, as practice in the use and management of chemicals and of apparatus, has printed in italics certain formulæ for their preparation—generally those given in the Pharmacopœia of 1836; but in some instances, when he thought that he could give better processes, he has inserted formulæ which never were in the London Pharmacopœia. His processes for ammonia and strychnia are good examples of this. He might have safely carried this a little farther, and introduced methods of preparing sulphate of quina and muriate of morphia, better than those of the Pharmacopœia of 1836 which he has inserted. We would point out the explanations of the theory of the processes for chloroform, ammonio-chloride of mercury, sulphuret of potassium, &c., as examples of the care bestowed on making this work useful to the student.

We can safely recommend this new edition of Phillips' translation, as one which will be found highly valuable to those whose duty it is more specially to study the London Pharmacopœia.

Of Dr Nevins' book we are sorry that we cannot speak in terms of commendation. It bears unmistakeable marks of having been hastily got up, and is consequently in many respects inaccurate. It is not a mere translation of the London Pharmacopœia; but includes also those of Edinburgh and Dublin, and aims at being, as

we are informed by the title-page, with more comprehensiveness than modesty, a "Complete Materia Medica." From thus aiming at too much, and not taking time to do it, Dr Nevins has fallen into numerous mistakes. A confusion runs through his nomenclature from beginning to end. He does not seem to be aware of the meaning of appending to the name of a drug or a chemical product the initial letters L. D. E., which indicate not merely that the substance is officinal in the Pharmacopœia whose initial is added, but that it is there recognised under the particular name given. Thus we have,—

"ANTIMONII OXYSULPHURETUM, L. D. E.

OXYSULPHURET OF ANTIMONY.

Synonyme.—Antim. Sulphuretum Precipitatum, D. Antim. Sulphuretum Aureum, E. Golden Sulphuret of Antimony. Liver and Crocus of Antimony. Kerme's [*sic in orig.*] mineral (not quite identical)."

Now any one reading this would suppose that Antimonii Oxysulphuretum was the officinal name in the Dublin and Edinburgh Pharmacopœias, which it is not, and that the other names were given there as synonymes; and in like manner the reader would suppose that the absurd name of Antimonii Potassio-Tartras was the Edinburgh and Dublin title for tartar emetic; for thus does Dr Nevins give the names of this important compound:—

"ANTIMONII POTASSIO TARTRAS, L. D. E.

POTASSO TARTRATE OF ANTIMONY.

Synonyme.—Antimonium, D. Antimonii, E. Tertarigatum. Tartar Emetic"

—a series of blunders, obviously typographical, but the non-correction of which shows the haste and carelessness with which this volume has been got up.

In the formula for preparing tartar emetic we have another example of carelessness. We are told that after igniting the sulphuret of antimony along with some sulphuric acid, and thus obtaining a whitish pulverulent mass, we are to "wash till nothing can be perceived, and dry it." We submit that if we are to wash the product till "nothing can be perceived," the drying will be a work of supererogation. Mr Phillips has translated this more intelligibly, by ordering us to wash until no acid can be detected.

At page 629, and again at page 633, we are told that *syrupus hemidesmi*, D., is a synonyme for *syrupus sarzæ*, L. D. E. If Dr Nevins had looked properly at the Dublin Pharmacopœia, he would have seen that these are two different preparations,—one used as a tonic,—the other, as Dr Neligan tells us, chiefly used as a flavouring vehicle. We have no great opinion of either; and we daresay no great harm would follow should Dr Nevins' book lead somebody into the error of prescribing the one when he meant the other; but still, if they are in the Pharmacopœia, and are to be noticed at all, they may as well be given correctly as in this blundering fashion.

Had Dr Nevins taken six months more to the compilation of this book, he would have acted more wisely, than in thrusting it out in this crude and immature form to catch the market.

Dr Lane's little volume is a compendium adapted to the wants of students grinding up for Apothecaries' Hall, and of those persons who like to carry their knowledge of the Materia Medica in their pockets. It appears upon the whole to be accurate, but is too concise to be of much use to any other class of persons than the above. There is an appendix, which professes to be a summary of practical toxicology, which, however, is very imperfect, as we find under arsenic no notice of the well known Reinsch's process, and under hydrocyanic acid no allusion to the equally notorious "sulphur test."

Le Climat de l'Italie sous le Rapport Hygiénique et Médical. Par Le Docteur Ed. Carriere. Paris: 1849. 8vo. Pp. 582.

Bradshaw's Companion to the Continent; with Observations on the Influence of Climate and Travelling. By Edwin Lee, Esq. London: 1851. 12mo. Pp. 408.

We feel that we have been too tardy in introducing to our readers the former of these two works,—the original, interesting, and complete treatise of Dr Carrière on the "Climate of Italy in its Hygienic and Medical Relations." Long before this, however, indeed immediately after their publication, the labours of Dr Carrière were acknowledged and appreciated by the medical press of his own country, and of that other of whose climate he has written. The work is extensive, including not only climatology in general, but the topography, geology, meteorology, and agricultural conditions of the favoured country of which it treats. Though minute in details, and, so far as we can judge, most accurate, Dr Carrière never is tedious; and while his work, as a whole, is better adapted for the medical than for the general reader, the latter too may peruse it, and seldom be arrested by the employment of unintelligible terms.

Mr Lee's work is of an entirely different character; it is intended, though not exclusively, as a descriptive hand-book to the chief places of continental resort, professing, moreover, to supply information to invalids and to their medical advisers. The former reason entitles the work to no notice in our pages, but on account of the latter, we have felt it our duty to peruse it. We have found it a pleasing, unassuming, and by no means uninteresting little volume, much too brief, however, and unsatisfactory in information to be of much use to enthusiastic travellers; such as invalids probably are not, but medical men generally are. We must further

caution our medical friends against trusting too implicitly to Mr Lee's expressed statements and opinions. What, for example, will any one familiar with the existing state of medical science on the Continent think of Mr Lee's observation, that the condition of medicine in Munich is superior to that in Vienna! But we have no desire to deprive either class of the perusal of Mr Lee's book, and repeat our belief, that it is upon the whole an interesting and well written little volume.

We return to the "Climate of Italy." Commencing with some interesting historical considerations, which, together with the remarks on the general topography, &c., occupy the first part of the volume, the author proceeds to consider the climate of the Italian peninsula, divided into three regions,—the southern, middle, and northern. To each of these, one of the three remaining parts of the work is devoted. The first, including the climate of Salerno, of Naples, with the towns situated on either side of its bay, the island of Ischia, and the climate of the Gulf of Gaeta. The second, the climate of the Papal and Tuscan States, with particular reference to Rome, Florence, and Pisa. And lastly, the third, including Sardinian and Austrian Italy, and treating chiefly of the climate of Nice, Genoa, and Venice. A continued residence in all parts of Italy frequented by invalids, and careful observation of the changes produced in individual cases of disease, enable Dr Carrière to speak with great confidence on the claims of every locality. In the main, his opinion seldom differs from that of the observer who in this country is deservedly regarded with the greatest respect,—we mean Sir James Clark. But we conceive not the least interesting feature of Dr Carrière's researches to be, that, besides dwelling on the advantages of the ordinary places of resort, he has investigated others less frequented, and from ample experience is able to express a decided opinion on these. For example, Dr Carrière speaks very highly of the climate of Salerno, and shows how, from natural position, it possesses all the advantages, while it loses the evils belonging to its popular and populous neighbour Naples. We admire the honest and straightforward manner in which, never yielding to prejudice, and disregarding preconceived notions, the author states plainly the evidence in favour of or against certain climates or localities. Such is a short indication of the manner in which Dr Carrière has treated his subject. We cordially recommend his work to our readers, feeling certain that, from its perusal, they will derive both pleasure and instruction. There is a grievous fault appertaining to most treatises on climate: their authors seem actuated by no desire for the advancement of science; but, writing in the most laudatory style, extol the excellences and curative advantages of some particular spot or spots, their reasons for so doing being generally as apparent as they are sordid. So glaring is this feature in the case of the numerous monographs with which the press is at present teeming, both in this country and abroad, that we feel it in-

cumbent upon us openly to denounce all such unprofessional emanations, and the conduct which prompts their publication. From such a category, it is pleasing to point to exceptions so widely different as the works of Dr Carrière and Sir James Clark, both of which may be accepted as models for authors desirous of writing on similar subjects.

Die Quarantäne Reform und die Pestfrage: Beobachtungen und Anträge. Von Karl Ludwig Sigmund, M.D., u.s.w. Wien: 1850.

On Quarantine-Reform and the Plague Question: Observations and Proposals. By Professor Sigmund. Vienna: 1850. Pp. 148.

Dr Sigmund has enjoyed peculiar opportunities of studying the subject of his memoir. Born on the banks of the Danube, at one of the most eastern points of the Austrian frontier, he became early familiar with the proceedings of the sanitary cordons of his native country. At a later period,—especially in 1828, when Cronstadt was visited with plague, and in the years 1829–30–37, when pestilence prevailed in the Danubian principalities, he had occasion to inspect the quarantine establishments, not only on the military frontiers of Hungary and Croatia, but in Italy, France, and England. Still more recently, Dr Sigmund was, in 1849, selected by the imperial government at Vienna to proceed on a mission to the East, and report on various sanitary questions. There is probably no physician in Europe better qualified to offer an opinion on all that relates to quarantine. It is consequently with great satisfaction that we find Dr Sigmund advocating most radical reforms in the old-fashioned systems of lazzarettos and sanitary cordons, and basing his suggestions, not upon mere speculative grounds, but upon the sure foundations of observation and experience. In his treatise he has accumulated a vast mass of information on the quarantines of the Danubian principalities, Wallachia, Moldavia, and Servia; on similar institutions in Turkey, Egypt, Greece, Malta, and the Ionian Islands; on the sanitary restrictions practised on various parts of the coast of the Mediterranean, and especially in Italy. He demonstrates the total inefficiency of most of these institutions for the attainment of the objects proposed by the policy which introduced them, shows how costly they are in their maintenance,—how repugnant to common sense and to medical reasoning in their restrictions,—how injurious to commerce,—how easily evaded, and if not evaded, how cruel and hostile to personal liberty.

Dr Sigmund's work is one which hardly admits of analysis. His suggestions for reform in quarantine establishments are summed up as follows, at pp. 128–9.

"I. Quarantines should be enforced only during periods of epidemic pestilence, recognised as such by 'experts.' All regulations which prescribe detention or quarantine at other times should be abolished. In times of pestilence the period of detention should be reduced to seven days, in which the time occupied by the voyage or journey should (with exceptions to be presently alluded to) be included, as also the so-called 'Sciorino' 'airing.'

"II. The processes of purification applied to merchandise, vessels and vehicles, might at all times be omitted.

"III. Rags, clothing, and bedding should, in suspicious circumstances, be still purified by the simplest means, such as airing, heating, and washing with water; when sickness prevails among cattle, skins, hides, and offal, should be treated in a similar way.

"IV. There need be no purification of letters or of coin in ordinary circumstances. In times of pestilence a temperature of 122° Fahr., which will injure neither seal nor paper, may be applied to them.

"V. All other regulations which have been heretofore included under the improper term of 'regulations for purification,' may be totally dispensed with.

"VI. Medical inspectors should be established at proper points of those countries in which the plague is suspected to originate, and their general duties should consist in acquiring, according to a plan prescribed, a knowledge of all pestilences or epidemic disorders among the inhabitants, or among their cattle; in examining travellers and their equipments when on the eve of a journey, obtaining evidence of their healthy condition, and reporting to the proper authorities.

"VII. Distinct and accurate information about all contagious epidemics among men and brutes, should be made as public as possible, in order to combat erroneous doctrines and prejudices.

"VIII. Sanitary establishments, whether at a land or maritime frontier, should be associated with the quarantine service, and this again with the general sanitary service of the state,—the whole being under the direction of properly qualified individuals.

"IX. Ships which pursue a legitimate traffic with suspected ports should be watched, and hygienic precautions enforced on board of them.

"X. There is room for improvement in the quarantine establishments themselves, and in the condition of the physicians and other officers attached to them. At each station there should be an hospital.

"XI. Distinctions should be made between the codes of regulations applicable to plague, cholera, and yellow fever. The probationary periods prescribed for individuals during epidemics of cholera and yellow fever are justified *by no evidence in the case of the former disease*, and by very varying indications in the case of the latter. If retained, out of deference to prejudice, *one* day would be enough for cholera, and three (Rush) for yellow fever. There need in these cases be no special purification of wares, utensils, vehicles, &c."

Although it is not our intention to comment upon these conclusions of Dr Sigmund, we cannot permit this last section to pass, without recording our conviction, that the author totally misstates the case with regard to cholera. Dr Sigmund is well versed in English medical literature, and even had his reading been confined to the reports of Mr Grainger, Dr Sutherland, and other officers of the British Board of Health, he must have seen evidence, by no means contemptible, of the expediency and efficiency of isolating suspected persons, removing them from infected localities, and applying to them proper hygienic treatment, during an epidemic of cholera. He might further, by reading the reports published in our own pages, have informed himself of the lamentable conse-

quences of neglecting such isolation. If, however, he merely means his readers to infer, that the process which would be pursued at a Tuscan or Neapolitan lazzaretto, on the advent of a vessel from an infected port, with a healthy crew on board, supplied with all the comforts conducive to health, is ridiculous, nay possibly pernicious, then we cordially agree with him; but to his unqualified statement we certainly cannot assent.

The author terminates his proposals for reform by,—

"XII. Recommending more just and humane punishments for infraction of quarantine regulations."

With this summary, our notice of Dr Sigmund's work must terminate. We trust his enlightened advocacy of sanitary reforms will tend to mollify and change the character of the cruel and absurd quarantine regulations of certain countries of Europe; and we believe that a British physician cannot more easily master the whole subject of these foolish restrictions than by availing himself of the information condensed into this little volume by Dr Sigmund.

Lectures on the Principles and Practice of Surgery. By BRANSBY B. COOPER, F.R.S., &c. London. 1851.

WE feel that a serious responsibility attaches to us in expressing an opinion of any surgical work, in consequence of the importance which is attributed by the London press to Edinburgh criticism in this department. During the last twelve months, the operative surgery of Mr Skey has been constantly advertised, with an intimation that it has received the approval of our venerable contemporary, the "Edinburgh Medical and Surgical Journal;" and a similar publication, by the Professor of Surgery in King's College, has been no less assiduously recommended to public notice, on the ground of having, in its original form, been favourably noticed by this Journal before it came under our management.

Mr Bransby Cooper here presents us with the thickest volume of surgery which has appeared since his namesake's dictionary of heavy memory. Fully subscribing to the doctrine, that "a great book is a great evil," we feel inclined to ask the author, how it was that he ventured to inflict such a bulky work on the patience of his readers? For our own part, we cannot discover why the book was published at all. It contains not one new idea of the slightest value, and gives an account, no less confused and imperfect than miserably ill-arranged, of the knowledge previously possessed. Any attempt to condense or analyse the substance of nearly a thousand pages, is plainly quite out of the question; but we may select a few passages as specimens of the grounds upon which our opinion is founded.

Does Mr Cooper really mean to allege, that either the great or little pox can be acquired through inoculation in the dissecting-room?—

"A virus may in some few cases prove the exciting cause (of irritation): a subject which had died of small-pox or venereal disease would doubtless affect you, but the train of symptoms would indicate the nature of the poison; unless, indeed, the mere wound induced the same train of symptoms as if no poison had been present, under which circumstances nothing could be attributed to the influence of infection."—P. 45.

The next passage we particularly recommend to the attention of young gentlemen tired of their classes, and desirous of visiting some object of interest in the country:—

"When you have reason to believe that you have become inoculated by virus from a diseased subject, you will be right to apply caustic or nitric acid to the part, for the purpose of destroying the poison; but when you cut your finger under the usual circumstances in dissection, I repudiate that plan, believing it to lead to most injurious effects, by increasing the local irritation, and consequently the liability to constitutional disturbance. Wash the wound well in tepid water, put a large poultice on immediately, carry your arm in a sling, gently open your bowels with a small quantity of calomel, James's powder, and rhubarb, followed up with saline draughts, and go at once into the country for a week, and depend on it very few of you will suffer any harm."—P. 45.

The following passage affords an example of the originality and condensed expression of the author's views:—

"In the healing of wounds of all kinds, the repetition of the application of dressings is always necessary; and let me urge the necessity of gentleness in this procedure; try to diminish as much as possible the sufferings of your patient; and remember, that by the violent removal of plaisters, although you may save your time, you are very likely to undo nature's curative processes, and not only protract the period of your patient's recovery, but probably prevent the adhesive union which, but for your hasty interference and roughness, was all but completed. It is said a surgeon should possess a lady's hand, a lion's heart, and an eagle's eye; but even these gifts would avail him nothing without he possessed humanity enough to direct them."—P. 58.

In reading the next fearful passage, we felt equally at a loss to imagine the operator's motive for the incredible procedure of laying open the rectum of a *female*, to extract a stone from the bladder, or to understand how a surgeon of Guy's Hospital should happen to assist on such an occasion.

"I once assisted Mr Costello in an operation for the stone, in a female at Greenwich, in whom the stone was so large as to preclude the employment of lithotrity; and he therefore performed the operation of lithotomy. On passing the forceps into the bladder, and seizing the stone, the difficulty of removing it was so great, from its size, that he intentionally laid open the rectum to facilitate its extraction. The patient recovered without a dangerous symptom, but afterwards had permanent incontinence of urine and fæces. The stone in this case weighed seven ounces."—P. 605.

Whether the following passage is cruel waggery or simple nonsense, we leave the reader to determine:—

"When, therefore, the constitution of the patient is restored, and the local inflammation completely subdued, surgical means may be resorted to; the

head and neck of the femur may be sawed off, and as the trochanter major, from its form, can adjust itself to the surface of the bone it rests upon, a very effective supplementary joint will be established, and the condition of the patient much ameliorated.

"This operation has been successfully performed by Mr Fergusson, among others, and is neither difficult nor dangerous, if the period for its performance be judiciously chosen."—P. 321.

That is to say, that "*when the constitution of the patient is restored, and the local inflammation completely subdued,*" in other words, when a spontaneous cure has been accomplished, the surgeon may step in, and, cutting away a *part* of the bone concerned, proclaim a successful excision of the hip-joint!

We entertain a similar doubt as to the spirit in which the author has expressed himself with regard to the treatment of aneurism by pressure. If *we* had ventured to say as much, there would have been an angry intimation from the other side of the water, that it only showed "we did not know how to do the thing":—

"Many years ago (about 1796) Sir Astley Cooper attempted to cure a popliteal aneurism by compression. The apparatus he employed was an iron ring with a cushion, which was placed on the outer side of the limb, and a screw with a pad so placed as to compress the artery at the will of the surgeon; but, to employ his own words, 'the use of it was worse than the operation.' In a few hours after its application, the patient declared that he would submit to any operation rather than bear the excruciating and protracted pain. Sir Astley Cooper concludes his account of the case as follows:—'The plan of pressure on arteries does not succeed, and therefore ought to be abandoned.'

"I am inclined to believe, however, that Sir Astley Cooper committed the same error in the application of the compress, which I have myself fallen into,—viz., that of considering it necessary to apply the pressure with sufficient force to stop the pulsation in the tumour; while, on the contrary, it only appears necessary that the current of the blood should be checked, which seems to tend much more to the coagulation of the blood than when its flow is wholly arrested.

"A few months since I admitted a patient with popliteal aneurism into Guy's Hospital; and as I considered the case well fitted for compression, I sent to Dublin for Mr Milliken's clamp-compressors, and applied the larger one in the upper third of the thigh, and the smaller one below, so as to compress the artery at the point where it pierces the tendon of the adductor magnus muscle, desiring the patient himself to adjust the pressure, and when it became intolerable at one point, to transfer it to the other, by screwing down the second pad, and relaxing the first,—and thus to employ them alternately as the pain became excessive at either part.

"The patient, however, notwithstanding the diminution of the tumour, and its evident increasing hardness, would not submit to a continuance of this protracted mode of cure, and begged of me to tie the artery, which I did, and he perfectly recovered.

"My colleague, Mr Cock, had a patient about the same time, on whom he tried the use of this instrument; but in his case also the patient would not submit to the pain produced by the instruments, and requested that the operation should be performed. It should be remarked, that in these cases the pressure was not applied with sufficient force to stop the circulation, but only to a degree sufficient to check the current.

"In 1833 I admitted a patient into Guy's Hospital with an aneurism in each popliteal region. The one on the left side was very small, and I therefore thought it a good case for compression. I tied the right femoral artery for the

cure of the larger aneurism; and upon the smaller tumour on the left side, I applied a graduated compress, so as to fill the whole of the popliteal space. I then bandaged the whole of the lower extremity, commencing with the toes separately, with a view of modifying the flow of blood through the sac, the pressure on the sac being greater than on the rest of the limb. The bandage was adjusted three or four times in the period that the patient was confined to bed during the healing of the wound on the opposite side. On the eighteenth day the ligature came away; at the end of five weeks the wound had perfectly healed; and upon removing the compresses from the limb, no other remains of the left aneurism existed than a small, hard, non-pulsating tumour; the patient was discharged cured; but in about six months after, he came back to the hospital, the aneurism on the left side having returned. I then tied the femoral artery, and he perfectly recovered.

"I have not recited these cases with a view of condemning the application of compression as a cure for aneurism; for there is much too high authority for its efficacy to warrant any surgeon's condemnation of it; but still, as they occurred in my own practice, I consider it right to describe them. A good account of this plan of treatment, and numerous cases showing its favourable result, have been published by Dr O'Bryen Bellingham, of Dublin."—Pp. 753, 754.

Blistering the leg for the remedy of indolent ulcers, although it originated in Edinburgh, is a practice so eminently useful, and so generally adopted, that we thought it might have been mentioned; but instead of this simple process, we find the following curious specimen of ingenious trifling:—

"When these ulcers prove very stubborn, and resist all constitutional and topical remedies, I have lately witnessed the best results from stimulating their surface, by subjecting it to a stream of negative electricity by a method I shall describe, and which was recommended by my colleague Dr Golding Bird.

"At a convenient distance from the indolent ulcer (not less than five or six inches) a portion of cuticle is raised from the cutis, by the application of a small blister, of the size of a half-crown. The cuticle is removed by a pair of scissors, and on the exposed cutis a piece of zinc-foil is placed, of the same size as the denuded spot. A plate of silver-foil is laid on the original ulcer, and the two plates connected by means of a thin copper wire. The size of the silver plate is immaterial. Both zinc and silver are now covered with pieces of moistened lint and oil skin, and the apparatus, as recommended by Dr Bird, is complete. In a few hours the surface beneath the zinc becomes white, and a slough begins to form, which in a short time is thrown off, leaving a healthy ulcer behind. In the meanwhile a great change has taken place in the original ulcer: it has thrown aside its indolence; the granulations are sprouting and contracting; new skin is forming at the margin, and the whole surface looks healthy and animated. The rationale of the action of this contrivance is simple enough. There occurs a decomposition of the water and chloride of sodium naturally existing in the blood and tissues; the result of this is the formation of soda (oxide of sodium) and the evolution of hydrogen and chlorine. The former escapes at the silver plate, while the latter unites with the zinc to form chloride of zinc, which produces the above speedy escharotic effect."—Pp. 148, 149.

Mr Cooper still thinks that "layer after layer of granulations is produced until they fill up the cavity to be healed."—P. 143. In regard to mortification, he tells us that stimulating poultices are generally required to assist in throwing off the sloughing portion. Such as the following are generally employed:—"Port wine, stale beer-grounds poultices, nitric acid lotions, and the epithema plumbi

diacetatis."—P. 172. Mr C. makes no distinction between fractures of the neck and through the trochanter of the thigh-bone in advanced age, as a ground of prognosis or treatment.—P. 281. In speaking of nasal polypus, he informs us that the malignant sort generally spring from the mucous membrane; and makes no mention of the fibrous growth in this region, which is one of the most interesting and important subjects of surgical practice.—P. 393. He insists upon the old-fashioned employment of a ligature on the spermatic cord to prevent its retraction when divided in castration.—P. 635. In amputating the fingers at their metacarpal joints, he recommends drawing the knife "in a semicircular direction from the heel to the point, along the side of the phalanx, from the lateral flap," (p. 868), which *must* leave an unseemly redundance of integument; and expresses his opinion, that "for those to whom the unsightliness is more important than the diminished use of the hand, it is a reasonable operation" to have the extremity of the metacarpal bone cut away, which no less certainly leaves an unseemly deficiency. In short, whether Mr Cooper discourses of great or small matters, he teaches a sort of surgery that in this part of the world would be thought hardly suitable for the practice of the present day.

Modern improvements in surgery, and their history, seem indeed to have engaged little of the author's attention. In regard to "Fissure of the Anus," he tells us that he is indebted to Mr Copeland for a knowledge of the disease and its remedy (p. 154); while it is pretty well known that M. Boyer described the disease, and a surgeon of Edinburgh pointed out the remedy now in use. Excision of the elbow-joint, which has been performed by one surgeon in Edinburgh upwards of ninety times, he never so much as mentions. The division of obstinate stricture of the urethra by external incision upon a grooved director, is also treated with profound silence; and amputation at the ankle-joint is minutely described, without any allusion to the surgeon who introduced this operation; but with the truly original remark, that "it is true that there is some difficulty in healing the stump, in consequence of the great thickness of the cuticle in this region; and this forms to a certain extent an objection to the operation."—P. 880.

It must not be supposed, however, that Mr Cooper is insensible to the merits of all his professional brethren; since his pages are profusely ornamented with names, of which, we venture to affirm, many never before appeared in print, except in the "London and Provincial Medical Directory." These gentlemen have earned their honours by calling the author into consultation; and it is certainly quite right that their names should go down together to posterity, so far at least as this ponderous vehicle may be able to convey them.

Supplement to the Principles of Surgery. By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, &c. Edinburgh. 1851.

MR SYME'S objects in submitting this short Supplement to the profession, are concisely explained in the following preface. We simply transfer it to our pages, leaving other journals, with whose direction the author has no connection, to criticise his labours.

"The circumstance of ten years having elapsed since the last edition of my 'PRINCIPLES OF SURGERY' was published, may at first sight seem to render it unfit for students of the present day; but when more carefully considered, will, I trust, lead to a different conclusion. This book was written at the request, and for the use, of my pupils, when I taught systematic surgery. It never professed, or was intended to set forth, all the opinions which were, had been, or might be entertained in regard to each point of surgical practice, but was limited to the statement of what I wished to explain and impress upon the gentlemen who attended my lectures. As the value of such a work must essentially depend upon the opportunities which its author has had for maturing his opinions, and subjecting them to trial, I feel no shame in adverting to the fact, that the edition of this book, which is still in the hands of the reader, has, during the last ten years, been daily tried by the results of my practice in the clinical wards of the Royal Infirmary. Fourteen years' previous hospital experience had corrected the views which it gave in many respects, as will appear, if the first and subsequent editions are compared; and the Supplement now added, contains all that is requisite to explain the changes in my sentiments which have taken place within the more recent period.

"In order to show how necessary such a digest of practical principles is for the advantageous prosecution of a clinical course, I have prefixed an introductory lecture, explanatory of the objects and method I pursue in teaching."

Part Third.

CLINICAL REPORTS, LECTURES, ETC.

CLINICAL MEDICINE.—PROFESSOR BENNETT.

REPORT OF THE CASES OF PULMONARY DISEASES TREATED IN THE CLINICAL WARDS OF THE ROYAL INFIRMARY DURING THE LATTER HALF OF THE SUMMER SESSION 1851.

Condensed from Clinical Lectures.

From the middle of June to the end of July 1851, we have had an opportunity of studying in the clinical wards twenty-seven cases of pulmonary disease. Among these were three cases of bronchitis; two cases of pleuritis; eight cases of pneumonia; eleven cases of phthisis; one case of hydrothorax, with general anasarca, dependent on nephritis; one case of cancerous, and one case of carbonaceous lung. In the August and September Numbers of the Journal I have drawn attention to some of these cases. At present, I propose

giving a general summary of the whole, and dwelling more particularly on some points which have not been previously noticed.

It may be well, before proceeding to comment on the cases, to state shortly the diagnostic general rules which have resulted from previous accurate investigations into the physical signs and morbid anatomy of pulmonary diseases. They are as follow :—

1. A friction murmur heard over the pulmonary organs indicates pleuritic exudation.

2. Moist, or dry râles, without dulness on percussion, or increased vocal resonance, indicate bronchitis, with or without fluid in the bronchi.

3. Dry râles accompanying prolonged expiration, with unusual resonance on percussion, indicate emphysema.

4. A moist râle at the base of the lung, with dulness on percussion and increased vocal resonance, indicates pneumonia.

5. Harshness of the inspiratory murmur, prolonged expiration, and increased vocal resonance confined to the apex of the lung, indicate incipient phthisis.

6. Moist râles, with dulness on percussion, and increased vocal resonance at the apex of the lung, indicate either advanced phthisis or pneumonia. The latter lesion commencing at or confined to the apex is rare, and hence these signs are diagnostic of phthisis.

7. Circumscribed bronchophony or pectoriloquy, with cavernous dry or moist, râle, indicates a cavity. This may be dependent on tubercular ulceration, a gangrenous abscess, or a bronchial dilatation. The first is generally at the apex, and the last about the centre of the lung.

8. Total absence of respiration indicates a collection of fluid or of air in the pleural cavity. In the former case there is diffused dulness, and in the latter diffused resonance on percussion.

9. Marked permanent dulness, with increased vocal resonance and diminution or absence of respiration, may depend on chronic pleurisy, on thoracic aneurism, or on a cancerous tumour of the lung. The diagnosis between these lesions must be determined by a careful consideration of the concomitant signs and symptoms.

The general diagnostic indications now noticed as being derivable from physical signs, admit of several exceptions, which, however, it would be difficult to systematise. Hence they can only be acquired from a careful study of individual cases. It is important also to remember that these signs should never be relied on alone, but be invariably combined with a minute observation of all the concomitant symptoms. Thus the signs indicative of incipient phthisis may be induced by a chronic pleurisy confined to the apex, or to retrograde tubercle. In either case, the previous history, age, &c., may enable you to determine the nature of the lesion. Again, it may be impossible at the moment of examination to distinguish between two diseases. For instance, there may be general fever, more or less embarrassment of the respiration, and pain in the side, accompanied with no dulness on percussion, but with a decided abnormal murmur, difficult to characterise, as being a fine moist rattle, or a gentle friction sound. Under such circumstances, the progress of the case will soon relieve you from any doubt as to whether a pleurisy or a pneumonia be present. The alterations which occur in the physical signs during the progress of the case also will indicate to the pathologist the changes which occur in the physical conditions and morbid lesions of the lungs. Thus the fugitive dry, or mucous râles heard during a bronchitis, point out the occasional constrictions and obstructions in the bronchial tubes. The fine crepitation of incipient pneumonia, passing into absence of respiration, and this again into crepitation, will satisfy him as to effusion, solid coagulation, and subsequent softening of the exudation. In the same way, by an accurate appreciation of physical signs and a thorough knowledge of morbid anatomy, the practised physician can tell the anormal conditions produced by phthisis, pleurisy, &c., and judge from the symptoms the effects of these upon the con-

stitution, with a degree of accuracy that to the tyro must appear to be marvellous. All such knowledge can only be acquired by constant examination of the patient on the one hand, and by a careful study of morbid anatomy in the pathological theatre, on the other.

An observation of the mode in which the various parts of the chest expand may still further serve to guide our diagnosis ; and the extent of this expansion is now capable of being accurately determined by the chest measurer of Dr Sibson, or the stethometer of Dr R. Quain. The laborious researches of the former physician have done much to determine the exact mechanism and extent of the various movements of the chest in health and disease ; but my own experience of this mode of investigation has hitherto been too limited to enable me to speak confidently with regard to the value of his method in diagnosis.

BRONCHITIS.

CASE I.—*Acute Bronchitis.*

History.—Catherine Mulvie, æt. 21, a servant, admitted into the clinical ward, July 21, 1851. She states that two weeks ago, when in a state of perspiration, she took a bath in the open sea. The same evening she was attacked with rigors and other febrile symptoms, and on the next day there was dry cough, difficulty of breathing, and a sense of oppression in the chest. The cough has continued since, with more or less expectoration, but the febrile symptoms have abated.

Respiratory Signs.—On percussion, there is no dulness anywhere. On auscultation, there is harshness of the inspiratory murmur anteriorly ; and posteriorly and inferiorly, on the same side, coarse crepitation.

Concomitant Symptoms.—There is frequent cough, with slight muco-purulent expectoration ; general debility ; headach ; soreness in the limbs ; occasional palpitations of the heart, the sounds of which organ, however, are healthy. Pulse 62, full. Digestive and genito-urinary systems normal.

Progress of the Case.—Under the use of antimonials with opiates, followed by expectorants, the pulmonary symptoms rapidly diminished. On the 25th the respiratory murmurs on the right side were healthy, and she was dismissed.

Commentary.—This was evidently an uncomplicated case of acute bronchitis, which had nearly run its course before admission. The repose of the hospital, aided by expectorants and opiates, sufficed for the cure. Bronchitis, like laryngitis, consists of an exudation infiltrated into the various tissues constituting the bronchi, terminating in the transformation of this exudation into matters which permit of its being either absorbed into the blood or expectorated. At first the lesion causes increased dryness, narrowing, and rigidity, and subsequently moisture, dilatation, and relaxation of the tubes. Owing to these changes, the vibrating sounds caused by the passage of air through the bronchi undergo variations, which indicate pretty clearly the dry or moist nature of the disease, or, as some term it, dry or moist catarrh.

Acute bronchitis may differ in intensity, from an affection very trifling and scarcely regarded, to one which very nearly approaches in severity a decided attack of pneumonia. It may be epidemic, and constitute what is called *influenza*. It may follow or precede a similar lesion in the lining membrane of the nasal passages, that is *coryza*. These affections are so common as to be generally treated by domestic medicines only, or it may be totally disregarded. But there can be no doubt that a disposition to attacks of this kind, though they may often occur for a long time with impunity, frequently leads to the incurable and distressing change of pulmonary texture known as *emphysema*, with its fearful accompaniment of spasmodic asthma and consecutive disease of the heart. Bronchitis, therefore, is an affection which the judicious practitioner will do all in his power to check and prevent.

To check the progress of an acute bronchitis or coryza, no remedy seems so good as taking a full dose of morphia on the first, or at latest second night, on going to bed. In the morning the patient should breakfast in bed, and keep

himself warm at home during the next day. This treatment, which was first recommended by Dr Christison, has the effect of cutting short the progress of the exudation, and preventing its return. Should, unfortunately, the disease progress, patience is perhaps the best remedy. But if the bronchi become clogged, sudorifics and expectorants, especially ipecacuanha, will be useful, and a blister will sometimes dissipate any lingering trace of the disease. The chief caution to be given should be to get perfectly rid of the disorder before any exposure to cold air be allowed. It is the disregard of this point, and the getting "cold upon cold," which serves so much to keep up the affection and at length induces the chronic form of the disease.

Case II.—*Chronic Bronchitis; Acute Peritonitis.*

History.—Mary Nicol, æt 21, a servant, admitted into the clinical ward, July 8th, 1851. Has suffered more or less from cough for the last two years. Occasionally it has been very troublesome, but not accompanied by much expectoration. Seven weeks ago experienced severe pain in the epigastrium, and since then the breathing has become short and hurried.

Respiratory Signs.—Anteriorly the chest is everywhere resonant on percussion. On auscultation, the inspiratory murmur is harsh, and towards its termination fine sibilant râles are heard. Posteriorly, the right side is more dull on percussion than the left. This is more marked towards the apex. At this point there is harsh inspiration and increased vocal resonance. There are also, over the whole right back, sibilant râles during inspiration.

Concomitant Symptoms.—Cough with trifling mucous expectoration; respirations short and hurried; great tenderness over epigastrium, increased on taking a deep inspiration; appetite tolerably good; no nausea or vomiting, and, with the exception of constipation, digestive system healthy; pulse 80, soft; heart sounds natural; catamenia regular; urine voided with pain, and in small quantity, otherwise healthy.

Progress of the Case.—The dry râles accompanying the inspiration continued for some days; but on the 21st they became moist, and coarse crepitation was audible over the inferior third of right back. The cough became more loose also, and the expectoration increased. On the 24th, the moist rattles were converted into deep sonorous murmurs, and great variations were heard from day to day, evidently in consequence of the greater or less amount of fluid in the bronchi. The cough and expectoration also varied greatly in intensity. Her principal complaint, however, was the epigastric pain, which, notwithstanding the application of leeches, warm fomentations, opiates, and counter-irritants, continued to increase. On the 28th, there was diffuse swelling of the abdomen, general tenderness of the surface, and all the symptoms of peritonitis from intestinal perforation. Latterly, there was dulness and absence of respiration over the lower third of right lung. She died August 10th, 1851; but unfortunately no dissection could be procured.

Commentary.—This girl laboured under a chronic bronchitis of some standing, which presented, during the progress of the case, most of the physical signs characteristic of the disease. Her chief complaint, however, was a fixed pain in the epigastric region, which proved in no way amenable to treatment, and which, as the event proved, was evidently connected with an ulcer either in the stomach or neighbouring intestinal viscera, probably the former, considering the frequent occurrence of ulcers in that viscus among servant girls. But in the absence of the facts which a dissection only could have afforded, all speculation on such a point is evidently useless. The dulness on percussion at the apex of the right lung, the harsh inspiration and increased vocal resonance, point to the existence of some condition of the organ at that point giving it increased density. They constitute the signs of incipient or of cretaceous tubercle. But percussion over the whole of right back was impaired; and towards the close of life, as weakness appeared, there was dulness and absence of respiration over the lower third of right lung. These physical signs indicate collapse

of the organ in this situation, or a condition which has been variously called by pathologists "condensation"—"infarction"—"hypostatic pneumonia"—"peripneumonie des agonisans," &c. &c.

In a very able series of observations on bronchitis, by Dr W. T. Gairdner, the pathologist of this Infirmary,[1] he points out, as one of the most common results of the disease, more or less collapse of the vesicular tissue, dependent on obstruction to the passage of air during inspiration, by glutinous or inspissated mucus. This collapse is often confined to individual lobules, which are condensed, comparatively heavy, indurated to the feel, of dark colour, and present the usual characters of the unexpanded portions of lung in the newly-born infant (*atelectasis*). Doubtless, also, such collapsed lobules have often been mistaken for lobular pneumonia, or pulmonary apoplexy in children. Dr Gairdner has further recorded facts, which render it highly probable that this collapse becomes more diffused in chronic cases of bronchitis, when a large bronchus is obstructed, and when, from the weakness of the individual, from abdominal disease, or want of resistance in the thoracic walls, the patient is unable to clear the air-passages by a strong expiratory effort. Hence why this lesion is common in fever, in bronchitis accompanying peritonitis or ascitis, and in young children. The case recorded is evidently one where, from the physical signs and other symptoms, we can have little doubt that collapse in the right lung occurred to a considerable extent.

CASE III.—*Chronic Bronchitis ; Emphysema ; Acute Laryngitis.*

History.—Edward Jackson, aged 22, a robust negro, cook to a vessel, admitted into the clinical ward February 14, 1851. He says that three months ago, when at sea, he first began to suffer from cough, expectoration, and shortness of breath, which symptoms, notwithstanding various remedies given him by his captain, have continued to increase up to the present time.

Pulmonary Signs.—Anteriorly the thorax is unusually arched from above downwards. On percussion, there is everywhere loud resonance, especially in front. On auscultation, the expiration is much prolonged, and accompanied by sibilant and sonorous râles, louder and more general on the right side.

Concomitant Symptoms.—There is frequent and prolonged cough, accompanied by copious frothy mucous expectoration, great dyspnœa on making an exertion, and occasionally coming on in paroxysms without any obvious cause. Cardiac sounds normal. Pulse 80, strong. Frequently vomits after a severe fit of coughing ; but the digestive and other systems are otherwise healthy.

Progress of the Case.—In addition to the dry râles heard when he was first examined, it was soon ascertained that copious coarse moist râles appeared posteriorly and inferiorly, especially on the right, but also on the left side. These râles were occasionally absent, but continued tolerably constant. The dry râles also underwent from time to time several variations in tone, intensity, and situation. During February, May, and June, he was tortured by severe and prolonged attacks of dyspnœa, during which he gasped for breath, and appeared on the point of suffocation. The attack generally terminated by violent cough, expectoration, and vomiting, after which he always felt relieved. These attacks came on every second or third night, and were sometimes occasioned by an unusually full meal. In May there was noticed, in addition to the other physical signs, a coarse moist tracheal rattle, so loud as to mask the pulmonary sounds. On one occasion, during this month, the attack of dyspnœa lasted four hours, producing partial asphyxia, delirium, and stupor. On the 24th of May, he was attacked with sore throat, and difficulty in deglutition, followed on the 30th by laryngitis and partial aphonia, which greatly aggravated the asthmatic attacks. During all this time, expectorants, antispasmodics, anodynes, counter-irritants, with occasional emetics, and cupping, were employed, with temporary, but no permanent benefit. In April and May the

[1] Papers in Monthly Journal for 1851.

smoking of stramonium evidently afforded him considerable ease. He also experienced marked relief from a draught containing Tr. Lobeliæ Æther. ʒiss. Towards the end of June, a sponge, saturated with a strong solution of nitrate of silver, was passed into the larynx several times, with marked benefit; indeed, so much so, that on the 11th of July, his condition was greatly improved, the attacks of dyspnœa ceased, and the cough, expectoration, and other symptoms, were much abated. On the 16th, he was dismissed at his own request, to resume his occupation as cook on board ship. The sore throat and laryngitis had then disappeared, but the chest was still unusually resonant on percussion; there was loud tracheal breathing, prolonged expiration, and occasional sibilant râle. Respiration, however, was comparatively easy, and he considered himself, as he certainly was, greatly relieved.

Commentary.—This man presented all the physical signs and symptoms indicative of extensive emphysema dependent on chronic bronchitis, accompanied with the most severe asthmatic attacks. These attacks were of a spasmodic character, referable to irritation of the afferent branches of the pneumo-gastric nerve, and to reflex action by means of the afferent branches whereby the bronchial tubes were contracted, the glottis closed, and the muscles of inspiration rendered incapable of dilating the chest. Violent cough and vomiting were always induced towards the close of the attack, followed by relief. The dyspnœa during the course of the disease was alleviated by antispasmodics, and the laryngitis by topical applications, of which I have previously spoken. I consider, however, that his recovery was mainly due to the advance of summer and a change of temperature—circumstances which should never be overlooked in estimating the effects of treatment in such cases. Indeed, of all the causes which excite asthmatic paroxysms in individuals labouring under emphysema, the effect of certain seasons and changes of temperature are the most unequivocal, and yet the most mysterious. Thus some persons who are martyrs to the disease in winter are perfectly well in summer, and *vice versa*. Some are immediately affected by the foggy air of London, and are well in the country; others are attacked when the wind blows from a particular quarter, especially the east. However difficult it may be to explain such idiosyncrasies, there can be no doubt that a knowledge of these circumstances will enable those who can change their residence to alleviate their sufferings in no small degree.

Emphysema is characterised anatomically by a permanent enlargement of the air-vesicles of the lung. These may frequently be seen through the pleura, with an ordinary lens, like groups of minute pearls. One or more of these may break into each other, and produce larger distensions, say, the size of a millet seed, and this process of enlargement may go on, by the breaking down of the intervening partitions, until every size of emphysematous cavity may be formed from the minute dimension alluded to, to that of a large orange. The walls of such cavities remain permanently open, having lost their elasticity; they are also evidently atrophied, and the paleness of the parts affected proves that the capillaries have been so compressed as to be either obliterated or impervious to the passage of blood.

In order to account for emphysema, numerous theories have been advanced, of which I shall allude to only the first and last. Laennec supposed that the fine bronchial tubes became rigid and more or less impervious from swelling of their lining membranes or impaction of mucus. He conceived that inspiration was a more powerful action than expiration, so that while air could be drawn through the obstructions it could not be breathed out. In consequence, it accumulated in the ultimate pulmonary vesicles, became expanded by heat, and so acted mechanically as a dilator, distending them from within, and causing them to enlarge more and more according to the duration of the disease, and extent of the respiratory efforts. Dr Gairdner, however, has lately pointed out that expiration is a much more powerful act than inspiration, and that there is never any difficulty in causing expulsion of air. It is the inspiration

which is laborious in all bronchitic cases, and, as has been previously stated, when the tubes are obstructed, so far are the air-cells beyond them from being dilated that they are in truth collapsed. Emphysema, then, does not occur in the vesicles connected with obstructed tubes, but in those healthy ones which are adjacent. When the lungs are in a normal state, the column of air presses equally on all the tubes and vesicles, but when one portion of the obstruction is collapsed or otherwise diminished in bulk, then the neighbouring portion is over-expanded, so as to occupy the space previously filled by the former. Hence why emphysema occurs not only as a result of bronchitis, but of chronic phthisis, or any other disease which causes contraction and hypertrophy of the pulmonary fibrous tissue. This theory is certainly most consistent with known facts, and may be therefore considered as not only probable, but as tolerably well established.

PLEURITIS.

CASE I.—*Acute Pleurisy.*

History.—Mary Harvey, æt. 21, a robust servant girl, was admitted into the clinical ward July 23, 1851. She enjoyed good health until seven days ago, when, after unusual exposure to cold and wet, whilst washing clothes, she was seized with difficulty of breathing, and a sharp cutting pain on the right side. She shortly afterwards experienced headach, general soreness, and the usual symptoms of fever, but does not remember having had rigors. The dyspnœa and local pain have increased in intensity, although the febrile symptoms on admission have somewhat abated.

Pulmonary Signs.—On percussing the right lung anteriorly, there is complete dulness over its lower half, and, posteriorly, the dulness extends over the two lower thirds of the lung. On the left side the lung is everywhere resonant on percussion. On listening over the dull portion of right side, there is complete absence of respiration, with loud pealing vocal resonance. In the centre of lung posteriorly ægophony. No friction or crepitating murmur can be distinguished. On the left side respiration is puerile.

Concomitant Symptoms.—Slight cough, but no expectoration; dyspnœa, but not urgent, and sharp cutting pain in right side, increased on taking a deep inspiration. Considerable headach and general soreness; the skin of natural temperature, but dry. Pulse 100, of natural strength; tongue furred; face flushed; no appetite; thirst; functions otherwise well performed.

Progress of the Case.—On the following day the dyspnœa and pain had diminished. On the morning of the 25th there was considerable sweating, and next day a copious sediment of phosphates and lithates appeared in the urine, and it was observed that the febrile symptoms had disappeared. The pulse was 84, soft. On auscultation, a friction murmur could be heard at the upper margin of the dulness on the right side. On the 3d of August the pulse was 72 and weak. The pain still continues, and the physical signs are the same. On the 6th, the extent of the dulness, the ægophony and vocal resonance, began to diminish, and the friction murmur to increase. On the 9th, no friction could be heard, and the respiratory murmurs were audible in the primarily dull portion of lung. On the 27th, with the exception of slight dulness, she was quite well, and was dismissed by her own desire.

Treatment.—On admission, twelve leeches were applied to the affected side. Two purgative pills were administered, and a third of a grain of tartrate of antimony, with ʒss. of solution of morphia, ordered to be taken every four hours. Subsequently a succession of blisters were applied to the right side. On the 3d of August she was ordered a pill of calomel and opium three times a-day. All the six prescribed were not taken, and no physiological action of the drug resulted.

CASE II.—*Chronic Pleurisy; Bronchitis.*

History.—John O'Neill, æt. 40, a writer, admitted into the clinical ward November 28th, 1850. Three weeks before admission, he was suddenly seized

with a severe pain in his left side, which impeded breathing. Three days afterwards feeling better, he returned to his employment, but in the evening he experienced distinct rigors, and the pain returned. Strong febrile symptoms followed, with cough and expectoration. He has been under medical treatment since then, and now, on admission, is considerably better.

Pulmonary Signs.—On percussion there is complete dulness over the whole of left side, anteriorly and posteriorly, with the exception of the infra-clavicular region, where the dulness is incomplete. Over the whole of right side there is unusual resonance. The expansion of the chest is greatly diminished on the left side, with absence of vocal fremitus. On auscultation the respiratory murmurs are inaudible over left side, except at the apex, where there is prolonged expiration and accompanying sibilant râle. On the right side anteriorly loud sibilant râles, both with inspiration and expiration. Posteriorly the respiratory murmurs are puerile. Increased vocal resonance, amounting to bronchophony, heard over whole of left side, assuming an ægophonic character posteriorly over scapular region. On right side vocal resonance normal.

Concomitant Symptoms.—Frequent and severe cough, followed by copious expectoration of frothy mucus. No pain in chest on taking a deep inspiration, but occasional "stitches" in left side. No dyspnœa. The apex of heart beats in the epigastrium, immediately below ensiform cartilage, and its sounds (which are healthy in character) are heard most distinctly on right side of sternum inferiorly. Pulse 68, small. Urine turbid, with deposit of lithates. Appetite good. Other systems normal.

Progress of the Case.—On the 9th of December a friction sound was heard below the left clavicle, and the resonance on percussion was more diffused. On the 26th, loud friction sounds had extended from above on the left side down to an inch below the nipple, and dulness on percussion was confined to the two lower thirds of the lung. The bronchitis, also, was diminished, and on the 17th of January the cough and expectoration had ceased, and the bronchitic râles had disappeared. On the 23d of February a careful examination showed that there was complete dulness over left lung, from the nipple downwards, and that on auscultation there were loud double-friction sounds with absence of respiration. He now complained of dull pain on the right side of chest inferiorly, and on the following day there was heard in that situation a double-friction sound, which, however, disappeared on the 5th of March. It returned every now and then, accompanied by "stitches" more or less severe. On the 2d of April there was dulness over both sides of chest, anteriorly from the nipple downwards, together with double-friction sounds on both sides. The expansion of both sides of chest is now equal—that on the left side having greatly increased, and the respiration being audible over its two superior thirds. During the whole of April the physical signs underwent no change, and he suffered considerably from dyspnœa. In May the dulness was more circumscribed on the left, and more extended on the right side. The dyspnœa, however, was diminished, and his general health so improved that he was enabled to take walks in the green. During the months of June and July he continued to improve, but complained of occasional pain in the chest, and cough, with slight expectoration, originating apparently from imprudent exposure to cold. The respiration, however, insensibly extended itself inferiorly on the left side, and towards the end of July the dulness was greatly diminished on the right side also. At this time he was so well that he was dismissed August 4.

Treatment.—At first expectorants and anodynes were employed to mitigate the bronchitis, together with a course of the iodide of potassium, and frequent blisters applied to the pleuritic side. In February pills of calomel and opium were ordered, which caused slight salivation for a period of ten days. On the appearance of pleurisy on the right side, it was combated by frequent applications of leeches, followed by counter-irritation. The latter remedy was con-

tinued from time to time during his subsequent residence in the house, together with occasional expectorants, anodynes, anti-spasmodics, and purgatives, according as his symptoms required them.

Commentary.—The first case was one of uncomplicated pleuritis. The fever terminated by crisis through the skin and kidneys, on the tenth day. The physical signs commenced to disappear on the seventeenth day, but had not wholly vanished until the thirty-fifth day. On admission, there must have been a considerable amount of exudation, with separated serum compressing the lung, so as to destroy the respiratory murmurs inferiorly. At the upper margin of the dulness, however, ægophony was heard, a sign as often absent as present in pleurisy, and certainly not deserving the importance which Laennec attached to it. The diminished action in the compressed lung was evidently counterbalanced by increased action in other portions of the pulmonary organs, as determined by the puerile respiration. Lastly, it was very instructive to observe how, as the fluid became absorbed, and the pleural surfaces were thereby allowed to come in contact, friction sounds were developed, and ultimately disappeared, as union between these surfaces may be supposed to have taken place.

In the second case the disease ran a more chronic course, beginning on the left side, and subsequently attacking the right. As the one declined, the other increased, and in both the physical signs determined with great exactitude the extent of the fluid exudation, its subsequent absorption, the rubbing of the diseased surfaces against each other, and, lastly, their subsequent adhesion. These changes occupied a period of upwards of eight months.

The physical signs of pleurisy can scarcely be understood without an accurate acquaintance with the appearances which the exudation assumes on the pleural and other serous surfaces. Generally it coagulates, and whilst the fibrinous portions solidify and attach themselves to the membrane, the serum is set free, and occupies the intervening space of the shut cavity. The coagulated portion is now usually termed lymph, and presents different appearances according to the rapidity with which it is thrown out, and the extent of the membrane affected.

In very acute cases of pleuritis, which have proved rapidly fatal, I have repeatedly observed the following appearances:—On carefully elevating the sternum, care having been taken not to disturb the body for some hours, the pleural cavity on the side affected has been found full of an apparently clear fluid of a yellowish or greenish tint. On removing this by means of a small shallow cup, the first portions seem perfectly clear and transparent. On continuing to empty out the fluid, it has been observed that the deeper we descend the more turbid it becomes, until at length nothing but a semifluid mass is removed. It will frequently be found that large portions of this mass, although tolerably consistent, are semi-transparent, resembling a light-coloured calf's-foot jelly, whilst other portions present the usual opaque appearance. Sometimes, when the body has been undisturbed for twenty-four hours, the whole exudation is separated into two distinct portions,—the upper, fluid and perfectly transparent, whilst the lower is composed of a pultaceous mass, resembling a bread-and-water or oatmeal poultice. In all such cases, the fibrinous portions, from their superior specific gravity, have sunk to the bottom, whilst the supernatant serum remains clear. The semi-transparent lymph is the portion most recently exuded, in which very few of the plastic corpuscles, formerly described, have been developed.

When the progress of the exudation is less rapid, the lymph assumes a more consistent appearance, and forms, over the part inflamed, flocculi of different sizes, or a distinct lining, varying in thickness from half a line to an inch. Occasionally it is smooth, but sometimes it presents a honey-combed appearance, or hangs in the serum in loose flakes of a dendritic appearance. The surface is generally smooth on lymph effused in the subarachnoid cavity. It is frequently honey-combed on the pericardium, which is owing to the repeated

closing and separation of opposed surfaces. It is often dendritic or stalactitic on the pleura and peritoneum, which is probably owing to one drop of plasma being exuded on a portion which has previously coagulated, as Vogel suggested, in the same manner that stalactites of carbonate of lime are formed in a cavern. A serous membrane, when inflamed, frequently resembles a mucous surface, from the villous appearance the effused lymph assumes; and, in point of fact, it performs the functions of a mucous membrane for a time, and is very active in absorbing the serum, which, if the inflammation ceases, gradually disappears. This villous aspect of lymph is very commonly seen on the pericardium and peritoneum. Occasionally also it assumes a lamellar arrangement, attributable probably to repeated exudations of blood-plasma at different times. This may be frequently observed on the pleura, and layer after layer readily dissected off. Sometimes there is more or less blood extravasated with the exudation, causing it to assume various tints of red, mahogany, purple, &c., according to the amount thrown out, and the period which has elapsed before examination.

When the inflammation has been less acute, or is of longer standing, we find, after death, that the coagulated blood-plasma or lymph has become more consistent. It assumes a more distinctly fibrous appearance, often extending between opposed serous surfaces in the form of bands, which have considerable tenacity and strength. These bands have a great disposition to contract, and ultimately become shorter and shorter, and assist in forming a dense substance, which at length firmly unites together the serous surfaces. This uniting substance becomes more and more dense, and not unfrequently resembles ligament in toughness and general aspect. In this form it may frequently be seen in phthisical cases, uniting together the lobes of the lung and pleural surfaces. Occasionally it assumes even a cartilaginous hardness, resembling the fibro-cartilage of the intervertebral substance. In this state it may frequently be observed on the pleuræ, and I have seen it thus half an inch thick, intimately uniting the lungs to the ribs. When it has been very slowly developed, it produces white indurated patches, of a glistening cartilaginous appearance, varying in extent, the surface of which has assumed the character of a serous membrane, and in no way interferes with the movements of neighbouring organs. Such patches are exceedingly common on the surface of parenchymatous organs, as the lungs, heart, liver, spleen, and kidneys. Lastly, false membranes on the pleural surfaces, but especially on the costal one, may assume a stony hardness, from the deposition of calcareous matter; and patches of this alteration may be scattered over the serous membrane, or may exist in disseminated points.

The minute structure of the coagulated exudation, composed of plastic or pyoid corpuscles and molecular fibres, has been described and figured in a previous lecture. These fibres become more and more tenacious and aggregated together the more dense the lymph becomes, and, in cases of calcareous deposition, are associated with molecules and irregular masses of earthy salts, mingled with crystals of cholesterine, and it may be numerous fatty molecules and granules.

It must not be forgotten, that on some occasions, when the exudation has been very abundant in the pleural cavity, it does not perfectly coagulate. The fibrinous and serous portions are not fully separated, and, under such circumstances, pus corpuscles are formed. The exudation, in these cases, passes into pus, although some of the fibrous element attaches itself to and lines the membrane. This termination of pleuritis is denominated *empyema*.

It results from our knowledge of the morbid anatomy, that if a large quantity of fluid is interposed between the pleuræ, the respiratory murmurs will be lost, while the vocal resonance is increased. If the amount of fluid be small, the murmurs are obscure, and the vocal resonance assumes a peculiar vibrating character, said to resemble the bleating of a goat. This is *œgophony*. If strings or bands of chronic lymph exist, which are stretched during the movements of the chest, then the rubbing round will assume a leathery or creaking character and if there be calcareous deposition, a filing or grating

noise may be produced, although this is very rarely heard over the pleuræ. Not unfrequently dense adhesions, with thickening of the fibrous tissue uniting the pleuræ, may occasion partial dulness, and increase of the vocal resonance, a result not uncommon at the apices of the lung, but which must be carefully distinguished from phthisis.

With regard to the treatment, it is essentially the same as that of pneumonia. There are the same general indications, and the same means adopted to meet them. It is rare that a case enters an hospital in its incipient stage, that is, when the serous membrane is unusually dry, and before much exudation has occurred. But in private practice such cases are more common, and occasionally they may come on in the ward of an hospital. In either case, a large bleeding followed by a dose of opium may cut short its progress. When, however, exudation has been poured out to any extent, and has coagulated, bleeding is injurious, and we must depend upon checking its onward progress, and diminishing the force of the pulse, by means of tartar emetic combined with opium. When the acute symptoms have subsided, we should favour the absorption and excretion of the exudation, by means of blisters, sudorifics, and diuretics. The urine especially should be carefully watched, as the sediments it contains will serve as an index to the amount of exuded matter excreted. Care should also be taken, at this period, not to allow the general strength to sink, for it is only by keeping up the nutritive functions that we can assist the vital powers in making those transformations which are essential in procuring the disappearance of the fluid, and adhesion of the solid exudation. By some, calomel is considered to be directly indicated as a means of favouring this absorption from the serous cavity. It was given in both the cases recorded, but in neither could it be said to have produced the effect; and although I have frequently seen the drug employed for this purpose, I have not yet been fortunate enough to meet a single instance where its good effects have been unequivocal.

PNEUMONIA.

CASE I.—*Acute Pleuro-Pneumonia; Bronchitis.*

History.—Mary Dickson, æt. 34, admitted into the Clinical Ward, July 2, 1851. Eight days before admission she was seized with rigors; pain in the right side of chest, increased on taking a deep inspiration; dyspnœa and inability of reclining on the right side. There was also cough, but no expectoration; nausea and vomiting; loss of appetite; headache and other febrile symptoms. These are still present, but somewhat abated in intensity.

Pulmonary Signs.—On percussing the right side anteriorly there is complete dulness extending from the third rib downwards. Posteriorly, on the same side, the dulness reaches as high as the middle of the scapula. Above these limits, both anteriorly and posteriorly, the lung is resonant. Two lower thirds of lung on right side immoveable during respiration. Left lung every where resonant on percussion. On auscultation over dull portion of right lung there is absence of the respiratory murmurs, distinct double friction sounds, and great increase of the vocal resonance. Under right clavicle both inspiratory and expiratory murmurs are harsh, and with the latter there are loud sibilant râles. On the left side of chest cooing and sonorous râles everywhere accompany the expiration.

Concomitant Symptoms.—Great dyspnœa; the respirations being rapid and performed with difficulty; considerable pain on the right side, increased on taking a deep breath; suppressed cough; no expectoration; pulse 104, of good strength. Heart sounds healthy but distant, and considerably masked by the bronchitic murmurs. Tongue dry and covered with a brown fur. No vomiting at present. Febrile symptoms strongly pronounced. Urine scanty, but otherwise normal.

Progress of the Case.—The friction sounds gradually extended over the whole of the right lung, and these, together with sibilant râles on expiration, were very loud on the 4th. The febrile symptoms had, however, abated, and the pulse sunk to 90. On the 5th, crepitation could be heard posteriorly over base of right lung, and was more pronounced and distinct on the 6th. On the following day, the urine was loaded with lithates. On the 13th, the friction and crepitation had much diminished, and the urine was clear. Expectoration copious. The appetite had now returned. On the 25th, it is reported that all dulness and abnormal sounds were removed. On the 27th, she was perfectly well, and was dismissed.

Treatment.—Twelve leeches were applied to the right side when the patient was admitted, and she was ordered to take half a grain of tartar emetic, and a quarter of a grain of opium, every second hour. On the 7th, the antimony was omitted, a diuretic mixture ordered, and a blister to the right side. On the 12th, to favour expectoration, a sudorific and expectorant mixture was ordered.

Case II.—*Acute Pneumonia; Chronic Bronchitis.*

History.—Agnes Donally, æt. 20, a hawker, admitted into the clinical ward, July 14, 1851. Says that she has had a cough, with expectoration for the last twelve years, but otherwise has enjoyed good health. Two days ago, after unusual exposure to wet and fatigue, she was seized with severe pain at the base of the right lung, with difficulty of breathing, and an increase of her usual cough and expectoration. On the following day, she experienced rigors, followed by severe febrile symptoms, under which she labours at the time of admission.

Pulmonary Signs.—On percussion, the two lower thirds of the right lung are dull, but more so posteriorly than anteriorly. The upper third of the right lung and the whole of the left lung are resonant. A fine moist rattle is heard over the dull portion of right lung on inspiration, and at the upper margin of the dulness there is slight friction. There is also increased vocal resonance over the whole of the dull portion. Over the upper third of the right lung, the respiration is puerile, with harshness of the inspiratory murmur. Over the whole of the left lung, the expiration is prolonged, accompanied with wheezing, sonorous, and cooing sounds. Coarse moist rattles are also heard inferiorly and posteriorly with the inspiration.

Concomitant Symptoms.—There is frequent cough, with discharge of copious purulent sputum, which, owing to the pain in the right side, there is some difficulty in expectorating. The skin is warm and moist. Febrile symptoms are present, but are not high. Pulse 100, full and regular. Other functions normal.

Progress of the Case.—On the evening of the 15th, the dyspnœa and pain in the side had greatly abated. On the 17th, these symptoms had disappeared, and the fine were replaced by coarse moist rattles on the right side. On the 18th, the urine was loaded with lithates and phosphates, and the skin became cool, and the pulse 88, and soft. From this time she rapidly improved. The appetite returned, the dulness and moist rattles on the right side gradually disappeared; but the bronchitic symptoms and signs continued. She was dismissed July 25th.

Treatment.—On the two first days, eight and twelve leeches were applied to the affected side, and a third of a grain of tartrate of antimony, with twenty minims of morphia, were given every four hours. On the evening of the 15th, half a grain of tartar emetic, with one-twelfth of a grain of powdered opium, was given every hour, and continued up to the morning of the 17th. The subsequent treatment consisted of blisters to the chest, and the following mixture :—℞. *Sp. Æther. Nit,* ʒiij.; *Potas. Acet.* ʒij.; *Sol. Mur. Morph.* ʒij. *Mist. Scillæ,* ʒv. *Ft. Mist. Two tablespoonfuls to be taken every four hours.*

CASE III.—*Acute Pneumonia; Chronic Bronchitis.*

History.—Maria Cowan, æt. 26, a washerwoman, admitted into the clinical ward, July 18th, 1851. For the last eight months, she has been subject to cough and expectoration, with dyspnœa in foggy or damp weather. Four weeks ago, after unusual exposure to cold and wet, was seized with a cutting pain in the left side, rigors and febrile symptoms. These symptoms afterwards diminished; but some days before admission, they again came on.

Pulmonary Signs.—On percussion anteriorly, there is a slight dulness over the whole of left side, but posteriorly, the dulness is complete over the two inferior thirds of the lung on the right side. On auscultation, coarse moist râles are heard on inspiration over the dull portion on the right side posteriorly. Superiorly the respiration is puerile. Anteriorly on right side, prolonged expiration, with sonorous murmurs, and on the left side, harsh inspiration, with occasional sibilant murmur. No friction or crepitation can be detected on this side.

Concomitant Symptoms.—There is frequent cough with tenacious sputum, here and there of a rusty colour. The breathing is difficult and hurried, and there is a cutting pain at the inferior margin of lung on the left side. There is headach — hot skin—furred tongue—loss of appetite—thirst—pulse 100, weak. Other functions normal.

Progress of the Case.—On the 20th, the headach and febrile symptoms had abated. Pulse 100, soft. Still complains of pains on left side. Crepitation continues on right side, together with the cough and gelatinous sputum. On the 23d, distant friction could be heard over the seat of pain on the left side. On the 28th, crepitation was still audible on the right side, but friction had disappeared. On the 3d August, all pain had ceased, crepitation was inaudible, and from this time, the physical signs of pneumonia were not heard, although dry bronchitic sounds continued. She was now seized with conjunctivitis of left eye, for which she was treated till the 18th, when she went out to make a visit, and did not return.

Treatment.—The same remedies were employed as in the last case, with the exception that leeches and blistering were applied on the left as well as on the right side.

Commentary.—The three cases now given were very similar to each other. In all, the pneumonia was well characterized, not only by the general symptoms, but by the physical signs, which indicated that the affected lung was in a state of hepatization on the patients' admission into the Infirmary. In all three, there was also well marked bronchitis, and in the first, there was in addition, pluerisy; yet all three cases did well with the treatment I have previously recommended to you, viz., tartar emetic and opium, followed by counter-irritation locally, and expectorants to relieve the bronchitis. Case I. was a very severe instance of pleuro-broncho-pneumonia, which, however, arrived at a favourable termination in twenty-three days. In Case II., the cure was effected in eleven, and in Case III., in sixteen days.

Of the five other cases of pneumonia which entered the Clinical Ward, during the period embraced by the report, and which are given in the August Number of the Journal, four recovered, and two died. But one of these latter proved fatal from cardiac and renal disease, and the other from a complication of anemia, fatty heart, liver and kidneys, with curvature of the spine. This recovery of six out of eight cases of pneumonia, and the two fatal ones, dying from other causes, offers a favourable contrast with the results of hospital practice in many other localities.

The morbid anatomy of pneumonia is comprised in a former lecture on exudation, the lesion consisting of liquor sanguinis poured into the air vesicles, minute bronchial tubes and parenchyma of the lung. It may be well, however, to dwell a moment on the fact that the exudative process may be very limited, indeed confined to a few vesicles, and the minute bronchial tubes connected with

them. This is *vesicular pneumonia*. We know it may be confined to a lobule or occupy an entire lobe, constituting the so-called *lobular* and *lobar pneumonia*. In either case the essential phenomenon of inflammation, that is exudation, has occurred, at first distinguishable on careful examination of the pulmonary tissue, by the blocking up of a few air vesicles in the form of minute granulations. Occasionally this lesion may be felt on handling the lung, in the form of minute indurations, varying in size from a millet seed to that of a pea—often red, but occasionally yellow, and in the latter case very liable to be mistaken for tubercles. Such small indurations, however, at length soften, and are converted into pus, like the lobar and lobular forms of pneumonia.

Microscopic examination of the pulmonary tissue, shows us, in the first instance, that the vesicles, the minute bronchi, and the areolar tissue, are infiltrated with a molecular and granular exudation, which often forms a complete cast or model of the vesicles and bronchi, easily separated mechanically, by washing and pressure. Not unfrequently, as shown by Remak, they are expectorated entire, and may be disengaged from the gelatinous matter with which they are associated, by throwing the contents of the spit-box into water, and teazing out the branched filaments. These, when magnified, present a fibrous exudation, in which are embedded commencing pus corpuscles, with a greater or less number of epithelial cells. Such portions of exudation which remain in the lung, are transformed into pus in the usual manner, become ultimately disintegrated and absorbed, and are at length excreted from the system, principally by the kidneys. If, from the extent of the disease, or weakness of the patient, this process is checked, the patient may die, either from inability to excrete the effete matter in the blood, or from interruption to the respiratory functions. The former, however, is by far the more common cause of the fatal result. If the exudation be limited in extent, or has been poured out slowly from the commencement, it may become what is called chronic. Under such circumstances, the ephithelial and pus corpuscles of the pulmonary tissue may undergo the fatty degeneration, and numerous compound granular cells be the result. If blood should have been extravasated, mingled with the other formations described, there will be often found red crystals of hematine, blood corpuscles surrounded by an albuminous layer, and the numerous transformations which the blood corpuscles are known to undergo after extravasation. (See Dr Sanderson's Papers, Monthly Journal, 1851.)

A rational treatment of pneumonia, therefore, must necessarily depend, 1st, upon a correct diagnosis ; and 2d, upon a knowledge of the natural progress of the disease ; at an early period it consists in the employment of those means which are best calculated to check exudation ; at a subsequent period, in doing all we can to favour absorption of pus ; and latterly, favouring its ultimate evacuation from the system by means of expectorants, sudorifics, and diuretics. These points I have discussed in previous lectures, and need not again allude to.

(*To be continued.*)

CLINICAL SURGERY.

REPORT OF SOME OF THE MORE INTERESTING CASES OCCURRING IN THE CLINICAL SURGICAL WARDS OF THE ROYAL INFIRMARY. BY E. R. BICKERSTETH, ESQ., *Resident Clerk.*

Displacement of the Extensor Tendon of the Middle Finger.

Late one evening, in the middle of October, a strong muscular man applied at the Infirmary, with the above injury. About an hour before, while in the act of falling, he had struck the knuckle of the half-closed fist violently against the corner of a chair, and had thus, as he said, put his finger out. On examination

of the hand, nothing more was observed than a slight degree of fulness over the dorsal surface of the metacarpo-phalangeal articulation of the middle finger. There was no abrasion or discolouration of the skin, and no fracture or other injury to the joint could be detected. The case appeared to be one of simple bruise, and I was on the point of dismissing it, when my attention was attracted by the man stating, that he was unable to extend the finger, without the assistance of the other hand. When he attempted it, the other fingers were all stretched to their utmost, but this remained flexed nearly at right angles with its metacarpal bone. Upon examining it again more carefully than before, I found that the tendon was in the hollow between the knuckles of the middle and ring fingers. In this situation it could be traced distinctly for rather more than an inch, and be felt starting under the hand placed upon this spot, whenever he attempted extension. When the finger was placed nearly straight, he was able to complete the extension without further assistance, and then the tendon returned almost to its natural situation, but the moment he bent it again it slipped aside into its former site.

A small splint was placed in the palm of the hand and along the finger, in order to maintain extension, and then a pad of lint was put on the ulnar side of the tendon, and so secured by a bandage as to prevent any future displacement. He was made an out-patient, and directed to call at regular intervals; but unfortunately has never returned, so that it is impossible to give the result of the case.

Acute Necrosis of the Shaft of the Tibia—Peculiar deformity of the Leg—Amputation at the Knee-Joint.

John Culbert, æt. 9, a sickly and emaciated boy, admitted the 2d October 1851. The right inferior extremity presents the following peculiar appearances:—While lying on his back he supports the right foot upon the anterior part of the opposite thigh, but, instead of resting upon its outer side, as is naturally the case when this position is assumed, it rests entirely upon its inner surface, the heel being placed about two inches above the knee, while the toes are directed upwards towards the groin. The thigh is abducted, and the knee-joint bent at an acute angle, thus accommodating themselves to the situation of the foot. The condyles of the femur and the patella are prominent and well-marked, and bear their natural relationship with regard to the thigh and pelvis. About three inches below the knee, the upper part of the shaft of the tibia projects obliquely downward through the skin, to the extent of not less than three inches. It is black, but retains its normal form and firm texture, and resembles exactly a macerated bone that has been long exposed in some foul situation. Its extremity is rough, and obliquely fractured, and no trace remains of the medullary cavity. Upon taking hold of it, it can be felt to move slightly, independently of the leg. Below the point at which the bone protrudes, both the leg and the foot appear to be in a sound condition, with the exception of two or three fistulous openings a little above the ankle-joint. The malleoli, and the tibia and fibula, for about two inches upwards, can be distinctly traced, and bear a proper relationship with the foot. Thus the lower part of the leg and the foot are completely turned round, the anterior part being posterior, and *vice versa*. On the outer side of the leg, an inch below the knee, and in a situation corresponding with a line continued upwards from the projecting portion of the tibia, is a hard, rounded prominence; and near the same spot are numerous cicatrices, the result of former incisions. Slight motion appears to exist in the knee-joint, but, as the slightest movement causes great pain, this point cannot be satisfactorily ascertained. On comparison, the leg is found to be an inch and a half shorter than its fellow of the opposite side.

The following history was obtained from the boy's father:—The disease commenced suddenly two years ago. He was standing in the open air, watching some men at work, when he first complained of severe pain in the leg. The

limb became greatly swelled and red, and after eight days' suffering (during which time he was very ill and confined to bed) the skin broke in several places, and a large quantity of matter came away. By this he was relieved, but afterwards fresh abscesses formed and burst, all around the upper part of the leg. These continued discharging for twelve months, when the bone began to protrude from one of the openings on the anterior part of the leg. At the same time three pieces of bone came away, and ever since the deformity of the limb has been gradually increasing, and the bone projecting more and more. During the last few months most of the openings have spontaneously closed, and the pain and severity of the symptoms have gradually passed away.

On the 4th of October, Mr Syme performed amputation at the knee-joint, making a large flap from behind, and sawing off the articular surfaces of the condyles of the femur. Several sutures were introduced, and then a piece of wet lint placed over the end of the stump. No bandage of any kind was employed.

The stump went on satisfactorily, and healed in great part by the first intention. The boy's health also improved rapidly, and he has now become fat and strong, and is able to walk about, bearing considerable weight upon the firm solid cushion formed by the posterior flap.

On examination of the amputated limb, the parts were found in the following condition:—The knee-joint in a healthy state, there being no traces of inflammatory action, and no displacement of the bones concerned. The shaft of the tibia was separated from the superior epiphysis, and in a necrosed condition. Its upper half was undergoing the process of exfoliation, protruding through the skin in the manner before described. The lower half was enlarged, its walls thin, and perforated with several minute openings leading into the medullary cavity. Connecting the lower fragment of the shaft with the superior epiphysis, was a thick, white, dense, ligamentous substance, interspersed among which were minute granules of osseous matter. The superior portion of the shaft was still in contact with the epiphysis, though separated from it by a quantity of pus and granular ossific matter. The ligamentous material connecting the lower half of the bone with the epiphysis was small and cordlike (about the thickness of the finger) at its junction with the lower fragment, but gradually became flattened and spread out, and at its superior attachment was only united to the circumference of the epiphysis. In this manner it formed a funnel-shaped cavity, having a long oval opening on its anterior part, through which the necrosed portion of the shaft of the bone protruded. On making a section of the lower fragment of the tibia, the walls were found so thinned as to be easily cut with a knife. The interior was completely filled with a highly vascular cancellated structure, and lying loosely among this were two long spiculæ of dead bone. The fibula was perfectly sound, and its attachments undisturbed. It was nearly straight in its direction, but was twisted upon its axis, thus adapting its extremities to the altered relative positions of the knee and ankle-joints.

Acute Inflammation and Death of the Fibrous Tissue of the Breast.

Elizabeth Dott, æt. 26, a full and plethoric-looking woman, admitted on the 22d of September, suffering from intense pain in the right breast. She states, that she has been nursing for the last twenty-two months, and has enjoyed good health during the whole of that period. Last night she went to bed in perfect health, and slept soundly until three o'clock this morning, when she was awakened with severe lancinating and throbbing pain in the right breast, and upon putting her hand to the part she found it much swollen. The child was applied, but without relief; and the pain becoming more urgent, and the swelling increasing, she at once sought admission into the Infirmary. Upon the first examination, only six hours after the commencement of the attack, the entire breast and the neighbouring parts were found to be greatly swelled, the skin tense and slightly œdematous, but without discolouration. The swelling extended upwards as high as the clavicle, and inwards across the mesial line. Pressure greatly

aggravated her sufferings, already most severe. She was in a state of great mental agitation, and exceedingly restless, tossing about on the bed, and crying out with excessive pain. The pulse was sharp and full, 120; tongue clean and moist.

The breast-pump was immediately used, and about four ounces of milk extracted with the effect of affording some relief. A sharp cathartic was given, and warm fomentations directed to be applied. In the evening the pain had returned with its former severity, and the appearance of the part had in no way improved. The pump was again used, but only half an ounce of milk could be obtained.

Little or no change took place in the state of the case until the third day, when, upon carefully examining the breast, an indistinct sensation of fluctuation was detected towards the upper part of the gland, and when a bistoury was introduced deeply in this situation, a small quantity of sero-purulent matter escaped, and was followed in a few hours by a much larger quantity of the same sort of fluid. Still no relief was experienced, and the swelling and tension of the surrounding parts were as great as before. The next day—the fourth from the commencement of the attack—the skin had become red, and of a dark mottled colour, over a space the size of the palm of the hand, on the outer side of the breast. At this part two or three incisions were made through the skin, and a small quantity of dark fœtid pus flowed from beneath it. The following day the skin had sloughed in the neighbourhood of the incisions, leaving several ragged openings, through which an ashy-grey slough of the deeper parts presented itself. This was seized with the forceps, and dragged away without difficulty. It evidently came from among the substance of the gland, from which it had not altogether separated; for it required a slight effort to complete the detachment, and it was felt to give way at one point after another as the traction was continued. A large portion was thus removed, but still more remained so firmly adherent as to render it imprudent to attempt the extraction of the whole *en masse*. The protruding part was therefore cut off on a level with the skin, and the remainder permitted to retract again within the edges of the orifice. After this had been done, there was still a small piece, about the size of the end of the little finger, which hung pendulously from one of the openings. To all external appearances, it precisely resembled any other portion of the slough, but, upon taking hold of it between the finger and thumb, it was found to be solid, and of firm consistence. As it could not be retained within the opening, and was unconnected with any of the neighbouring parts except at its base, the protruding portion was cut across with the scissors, and removed. On the following day the appearance of the breast had greatly improved, the swelling and tension had almost disappeared, the redness and sloughy condition of the skin had ceased to extend, and there was a free discharge of bloody purulent matter from the openings. She had slept well for the first time since her admission, and expressed herself as comparatively free from pain.

September 30th.—The remaining portion of the slough separated, and was taken away in the same manner as before. It proved to be nearly equal in size to that first removed.

The examination of the sloughs and of the small solid substance was of high interest, and requires a minute description.

Both the sloughs presented the same characters, and therefore an account of one will be sufficient. It was of a yellowish-white colour, loose and flocculent on its exterior, while the bulk of its substance consisted of a firm and tough mass. When allowed to float in water, the surface was seen to contain a number of small ragged orifices, which opened into minute spherical cavities, formed by the peculiar arrangement of the more flocculent substance composing the exterior. Some of these spaces were large enough to contain a small pea, while others were not of more than half that capacity. Their walls were very thin, and internally consisted of a soft villous-looking membrane, disposed with perfect regularity. In the spaces between these orifices there was no arrangement whatever in the fibrous laminæ,

which, meeting at all angles, formed a complicated network Upon slitting open with the probe-pointed scissors some of the small cavities just described, many were found to end, not in mere cœcal pouches, as supposed, but to be in fact the openings into tubes, which could be traced for a considerable distance into the firmer substance of the slough, and be seen to give off at intervals other tubes, of a somewhat smaller size, which also being traced, were found to end by a number of blind sacculi, closely placed together. The appearance of these terminal pouches did not differ in any way from those seen on the surface of the slough. The greater number were entire, having no other opening into them than the common duct, while others were rendered incomplete by a ragged deficiency in their delicate walls. The coats of the tubes were much thicker and stronger than those of the terminal sacculi, and for the most part were complete in their entire circumference. Their internal surfaces presented the same characters as those observed on the interior of the pouches. A microscopical examination of scrapings and of sections made through the more solid parts of the slough failed in discovering more than simple fibrous tissue, mingled with oil and milk globules.

The small solid substance, removed at the same period as the first slough, proved on examination to be a lobule of the glandular tissue of the breast, apparently in a healthy condition, but surrounded by a thin tough layer of dead fibrous tissue. When this was pulled off, which was easily done, portions of the fibrous partitions from between the ultimate lobules also came away, being attached to its internal surface.

After the separation of the second part of the slough the inflammation quickly subsided, and the swelling rapidly decreased; the skin became retracted at various parts, and the naturally rounded form of the breast was completely lost. As the cure advanced, the puckering-in of the skin became still more strongly marked, and by the time she left the hospital (on the 15th of October) had attained such a degree that the nipple was entirely obscured from view, being retracted within a long and deep longitudinal furrow, which completely traversed the breast. The glandular substance was everywhere adherent, and could be felt lying, without any definite form or arrangement, immediately beneath the skin.

ADDRESS

DELIVERED TO THE STUDENTS OF THE EXTRA-ACADEMICAL SCHOOL OF MEDICINE, SURGEONS' HALL, EDINBURGH, ON THE OPENING OF THE WINTER CLASSES FOR 1851-52. BY DR DOUGLAS MACLAGAN.

Gentlemen,—At the request of my colleagues, I appear here to address you on the commencement of another winter session. I have undertaken this duty with much diffidence, because I have failed to discover in myself any special qualification for the office, unless it be the somewhat unenviable circumstance of my happening to be the oldest medical lecturer in our extra-academical school. I especially regret that I do not possess any aptitude for that sort of discursive composition which is called speaking to the general question, and that I lack that inventive faculty which might enable me to strike out some new light to guide you in your future path of study. I thus despair of being able, by novelty or originality, to render my address attractive to you, and can only hope, by honesty of purpose, to make it useful.

I purpose shortly to offer you a few words of advice as to the feeling and disposition of mind with which you should enter upon and prosecute your professional education, and I shall be more than sufficiently rewarded if I can bring myself to believe that any suggestions of mine can aid, to however small an extent, in directing you how to go through your curriculum with the best prospects of ultimate success.

There are many forms into which I might throw the hints that I purpose now to offer you, but it appears to me that I shall best arrive at the result at

which I aim, by correcting some misconceptions, which I believe frequently to prevail, as to what constitutes success in studentship.

It is often thought that by a successful student is meant, one who gets good class certificates, and ultimately passes his trials before the examining boards. Certainly no student can have any pretensions to be called successful who is not in possession of good certificates, and who does not in the end obtain his degree or diploma. But the mere possession of certificates and a diploma are only the attainments of a student who has not failed; they alone do not entitle him to be called successful. It is not until after his curriculum is ended, and his trials have been passed,—it is not until he enter on the real business of his profession, that it can be told of him whether he has been successful or not. Keep this steadily in view,—remember that your function as students is to prepare, not for the passive endurance of an examination, but for the active exercise of a difficult and most responsible profession; in a word, look upon studies fixedly and constantly as the preparatives for the business of your future lives.

There are two sorts of trials awaiting you all: one at the end of your curriculum which commonly receives the name; the other posterior to your transactions with hall or college, which much more truly merits it. Let us for a moment glance at these, and see what is their comparative importance in respect to the preparation necessary for them.

At the former of these trials you are introduced into a certain room, comfortable, perhaps elegant, if you could bestow a thought upon its architecture, but having withal a somewhat solemn and business-like aspect. You are requested to take a chair, which is provided for you, and you find yourself in the presence of four or five gentlemen, whose countenances, if you pay any regard at all to their expression, give token of encouragement, and prompt you to believe that, while as your examinators they must deal with you firmly and impartially, they will not be awanting towards you in courtesy and fairness. On the table at which you are seated there are some human bones, which have an ominous look that you never perceived in bones before, though you have often handled them; and there are also one or two dried anatomical preparations, the red arteries of which appear, as you cast a furtive glance towards them, to have a complexity that you never before had thought to belong to the circulating system either of a superior or inferior extremity. I know that you are a little nervous. I would not have you otherwise; it would betoken either a presumptuous confidence in your own acquirements which would ill become you, or a stolid indifference to your reputation which would augur badly for your future prosperity. But a word from one of your examiners re-assures you, and soon puts you comparatively at ease. He begs you to be calm,—to take time,—to think well before you speak; and he puts to you a few questions, beginning with some the replies to which are self-evident, and gradually leading you on to others, which prove that the advice to reflect before you reply was neither inappropriate nor ill-timed. This lasts for some half or three-quarters of an hour, and your examination is ended; you retire, are re-called in a few minutes, and you are congratulated on now being a licensed practitioner of the healing art. A welcome sound this to your ears. You withdraw from the Royal College with an intense self-complacency of feeling, and a step infinitely more elastic than that with which you entered. You begin to wonder why you were so fluttered when you first went in,—it was not so difficult as you expected; and you unhesitatingly settle in your mind that he must be either a very idle or a very dull fellow who cannot get through there. I do not quite agree with you. You estimate your performance of this day a little too high; but, in consideration of your being victorious, I excuse this little vanity. You are sure you answered all your questions correctly,—you think you may have missed *one*, but that was all. There you are wrong; you made more than one mistake; your examinators observed your errors, though, from a wish not to disconcert you, they refrained from

telling you of them at the time; but these were comparatively trivial, and so far overbalanced by what was good in your replies, that they had no hesitation in putting into your hand the much-prized diploma. I must tell you, however, that you thought your appearance to be much nearer faultless than it really was,—nevertheless, as your examiners have congratulated you on having passed, I, as your former teacher, most cordially do so too; and, taking all things into consideration, I admit to you that if a man is not idle, and has a moderate share of ability, he may reasonably expect to achieve what you have done. And this is your much-talked-of examination; this has been the engrossing subject of your thoughts for three or four years; your whole duty, you conceived, was to prepare for this; you did prepare, you came to battle, and you are now flourishing your laurels. You have not forgot your Cæsar, and (a slight liberty taken with the original) your brief commentary on yourself is, *veni, visus sum, vici*—I came up the College, I appeared before the board, and I have passed triumphantly. Pause a moment; you have carried the outworks, but the citadel is yet before you. Let me remind you that I said you had two sorts of trials to undergo. One is over, but the other is yet to come. Are you in reality prepared for it? Now is the time to determine whether, as a student, you have been truly successful or not.

It is not now transacted in a hall spacious and well lighted, but in the dingy chamber of sickness, from which the very rays of heaven are excluded, as if they were something too joyous for admission there. It is not in the stillness and quiet, so conducive to the collecting of thought and fixing of attention, but amidst the clamours and distractions of alarm and distress. You are not now in presence of three or four gentlemen of learning and discernment, who sympathise with your anxiety, and encourage you by their manner of addressing you; but you are surrounded by a household, ignorant of the real difficulty of your duty, and by their very solicitude to aid and further your exertions, becoming unwittingly the means of perplexing and retarding you. There is not before you a table, with a few dry bones and an inanimate piece of anatomy, but the bed of suffering, on which lies living and imploring humanity in its throes and agonies. You are not now met with a kindly word, telling you to take time, and to reflect before you utter; but you are assailed with importunate calls to delay not a moment, and to act when you scarcely have had time to think. It is not now a plain question proposed to you, to which you may respond slowly yet rightly, though with a faltering tongue and a tremulous lip, but your answer to that which is now put to you must be by action, on the instant, with quick eye, firm nerve, and steady hand. Your judges in the one case are a select few, who, though not unobservant of your deficiencies, have no desire to urge them hardly against you, and who infinitely prefer being able to congratulate you on your attainments; your judge in the other case is the undiscriminating world, ready enough to pounce upon any shortcomings it may observe in you, and scarcely thinking of your successes, except to envy you for them. Your trial in the one instance is the work of an hour, undertaken at the very time when you have specially prepared for it, and have concentrated all your energies upon that one thing. In the other instance it lasts your lifetime; it is by day, by night, when you may be jaded and weary, and when your mind may be distracted by a multiplicity of objects, each demanding your attentive regard. These are the two sorts of trials you have to undergo; and be assured that it is in the active exercise of your profession, far more than in the mere passing of a formal examination, that the success of your studentship will really be tested. Say which of these trials requires the greatest amount of preparedness in him who undergoes it. Tell me whether it is the man who responds to the questionings of a College examination, or the man who is equal to the emergencies of daily duty, who can most appropriately be held to have been a successful student. Is there any great probability that he who has diligently prepared himself for the greater trial, should find any difficulty in fitting himself for the lesser? Keep this,

then, in view during your period of study,—let it be your endeavour to fit yourselves for the active duties of your profession, and in the accomplishing of this you will become fully qualified for every College examination.

Let no one torment himself with the thought, that in thus fixing his eye upon what is required to fit him for active professional duty, he may in the course of his studies overlook something which may prove a stumbling-block to him on his appearing before an examining board. Such a false idea is founded upon a complete misapprehension both of the nature of the examinations which he has to undergo, and of the duties which will afterwards be required of him. Let me briefly set you right regarding these.

And first let me observe, that an idea often prevails regarding these examinations, that, though they nominally are intended to ascertain a man's fitness for the discharge of practical duty, yet they really do not so much do this, as determine what has been his capability of amassing, during a certain number of years, a certain amount of multifarious information; that he has to learn a good deal for no other purpose than to be examined upon it, and that consequently, if he does not make it his chief business to prepare for these dreaded examinations, he may never be able to get into the position to show whether he has acquired practical knowledge or not; that the kind of information he requires for passing before a college differs from that which he needs to make him a good practical medical man, and that he must first acquire that which will enable him to pass his examination, and then he can set about getting what he wants to establish him in the world. I know that it is often conceived by, and not unfrequently instilled into, students of medicine, that they are obliged to study, and must be prepared for being examined upon, or, as the phrase goes, must "get up" for examination, a great many matters which will be of no use to them in their future career, and which they may as well get rid of and forget as soon as possible after they have got their diplomas. Now, against this doctrine I protest, as being at once most pernicious to you as students, and most unjust to those whose business it is to direct your studies. I do not now speak so much of us, your teachers, as of those to whom we are in a certain sense under authority—the public bodies whose duty it is to determine what your professional education shall be, and to ascertain that it has been properly attended to. Do you suppose that all those curricula of study which have been laid down by them for you, which have been deliberated upon and determined in academic senates, royal colleges, and worshipful societies, have been adopted by men, many of them the most enlightened and sagacious in our profession, in sheer ignorance of the kind and amount of knowledge which medical men ought to possess? Or do you conceive that, with perfect apprehension of this, they maliciously impose unnecessary study upon you, as if you had committed a trespass in daring to enter the precincts of medicine, and merited hard labour as a punishment for your offence? Believe me that it is neither in ignorance nor in tyranny that these studies are demanded of you. If your curriculum is more extended than it was in the days of your grandfathers, it arises solely from this, that the seniors of our profession, who have become gray in our ranks, have become sensible of the deficiencies in their own opportunities of instruction, and are honestly desirous that you, their successors, shall have access to stores of knowledge which were not open to them in their own day. I maintain, then, that the doctrine that much or any of your study is ultimately to prove a useless incumbrance to you, is most unjust to those whose duty it is to superintend and direct your professional education.

But I hold it no less decidedly to be pernicious to yourselves. If you are to carry with you into your class-rooms and your closets the idea, that part of what you are to hear from your teachers, or learn from your books, is to be useless to you, believe me, that in spite of the most orthodox resolutions on your part now, you will soon come to hold very latitudinarian sentiments as to what it is necessary for you to attend to, and what you may venture to ne-

glect. You are not yet in the condition, of yourselves to determine, what part of the harvest which you are ordered to gather in, is wheat and what is chaff; and if the winnowing should thus be left to you, many a goodly grain will be scattered to the winds which ought to have been stored in your mental garners to be prized by you as richest nutriment, or to have been deeply planted in your minds, there to take root and grow, and in due season to yield increase, thirty, sixty, yea an hundred-fold.

Perhaps you will meet me with the reply, that, even though you are impressed with the belief that part of your labour is needless, you are still of a willing mind and an obedient spirit, and since the thing has been ordered for you, you will do it with honesty and fidelity. I respond that your resolution is praiseworthy, but that it is not possible for you to carry it into execution. Your minds, if worth cultivating at all, cannot surely be of so dull a temper that they will permit you slavishly to submit to the humiliation of wasting time and energy on work which you believe to be profitless. You surely will not consent, like a tame squirrel in a cage, ever moving but never advancing, to bestow your exertions on a labour that is not to forward you one step in the world. It cannot be so. If you start with the feeling that you are called to fruitless drudgery, your spirit, though obedient now, will become rebellious ere long; you will slacken your pace in a course which you think is to bring you to no good result,—in a word, if you fancy that any of what you have to learn is to be ultimately useless to you, you will never learn it properly, and will soon cease to attempt to learn it at all. I beg you, then, to enter upon your studies with a better feeling; carry with you to the work a hearty conviction that you are not asked to attend to anything that is ultimately to be unprofitable to you. Look upon everything that you are called upon to study, not as being a mere task to be learned, that you may be able to answer questions regarding it, but as something worthy of being known because of positive intrinsic value. View your whole studies in this light, and every part will bear a warm general tone of colour, which will impart a positive attractiveness even to those portions which, when viewed through the cold medium of a false prejudice, appear to many an eye flat, stale, and unprofitable.

Let me add two small bits of advice. Banish from your minds, and extirpate from your vocabulary, that most obnoxious expression "getting up" a thing for examination. A thing that is got up, means a thing that is not learned. It means the substituting for a good solid knowledge, of a superficial unsubstantial smattering. It means, moreover, time misspent and labour of a certain kind misapplied, because devoted to no ultimately useful end. It implies further, in the prospect of an examination, a sordid trusting to the luck of not being rigidly tested on this particular point, instead of the legitimate confidence of a candidate who is conscious that he has done his best. The man who has ploddingly studied runs little risk of being rejected. The man who has "got it up" has the odds against him.

Further, let me caution you against listening to the tales which are told of examiners and examinations by unfortunate candidates. They always would have you to believe that their want of success was owing to anybody's fault but their own. They are perpetually discovering that some one examiner showed an animus hostile to them, or that the whole board were somehow prejudiced against them. They would lead you to fancy, that a board of examinators is little better than a convocation of human vultures, ready to pluck upon the spot every unhappy bird that is turned into their cage; that they put questions which do not properly belong to medicine or surgery, and which, therefore, no candidate dreams of having asked of him. I will tell you how to put down these alarmists. Do not rest satisfied with their general statements; make them specify the individual questions which were put to them, and you will soon see what odd notions they have as to what is fair or unfair in an examination. I remember well—it was the week before I was examined for my surgical diploma—a fellow-student of mine being rejected at Surgeons'

Hall. He was one of those who spent more time in refreshment than in dissecting rooms, and whose knowledge of the Materia Medica was pretty much limited to the one article of tobacco, in which his erudition was complete and truly practical. He came with a woful tale of the horrid usage he had met with, and mentioned one examiner, who had put to him very unfair questions, and whom he believed to have rejected him for errors of trifling importance. And what do you think one of these trifling errors was? Neither more nor less than that he did not exactly know on which side of the orbit the lachrymal gland lay! An error which, I need hardly stop to remark, betrayed, not mere unacquaintance with a single anatomical fact, but an ignorance of the structure, arrangement, and physiology, of the whole appendages of the eye, and consequently of their pathology and diseases. Never listen, then, to the tales of the rejected about unfair examination. Never give ear, on the one hand, to those who tell you that you need not learn this or that, because it will not be asked of you at the College, nor, on the other hand, to those who tell you that you must "get up" this or that, because at the College it may be asked of you. If it is a thing which may be asked of you orally at an examination, it is a thing which may be asked of you practically in the exercise of your profession; and it is, therefore, to be learned thoroughly for practical purposes,—not to be got up by cramming, for the mere object of facing an examinator.

I have thus endeavoured to warn you against the false system of directing your studies as if they were merely meant to enable you to pass your trials, instead of being intended to prepare you for the active duties of your profession. I have said that this originates in a false estimate of what will be required of you at your examinations; and I have now further to remark, that it is also connected with erroneous ideas of what will be required of you in your professional duties.

If you understand the exercise of your profession to mean nothing more than the mere application of cures to the injuries and diseases of the body, then I do not wonder that you should hold erroneous opinions as to the value of many parts of your studies, and should thus be led to treat some subjects with most unmerited neglect. But I tell you, that, if you hold your profession to be worth nothing more than this, you are grievously mistaken regarding it, and you will soon be painfully undeceived by seeing yourselves outstripped by those, who entertain juster views. If you have not learned something at least, of the principles and practice of surgery and physic, you have learned nothing at all; but if your knowledge does not go beyond this, you have learned but little. True, you may acquire, like Captain Taffrail's surgeon's mate, the character of being "a clever fellow at caulking a shot-hole," or you may have a sort of reputation in the country side, shared in by many an old wife there, of being a "skilly body" with a woman in labour; or you may even rise to the dignity of becoming the Dr Hornbook of the parish, from the success of your prescription when

> "A countra laird had ta'en the batts,
> Or some curmurrin' in his guts."

But if you are really nothing more than a mere man of probe and scalpel, the short forceps, and Gregory's powder, you are at best but a mediocre sample of the profession after all. You may perhaps, as a practitioner, become what is called "respectable," which has been said to be indicated by keeping a gig; but it is unlikely that you will ever rise higher. Your country will require of you something more than the mere routine work of healing sores and curing aches and pains, before it will place you among its highly rewarded or highly honoured men. It will not merely expect from you efficient aid when individuals suffer, but it will put to you curious and intricate questions affecting the welfare of the community at large. It will ask you to tell how the pestilences which threaten, and the epidemics which decimate it, are to be averted or

arrested—How its paupers and its criminals are to be fed and housed with economy to the public purse, and yet safety to the health of these objects of its care. Nay, it will in a certain sense place in your hands the scales of justice; for oftentimes does it happen, that upon the testimony of the medical witness in a court of law, will depend, not only the decision of litigations involving large pecuniary interests, but the awfully momentous question, whether another human being is to be dismissed from the bar as innocent, or to expiate a crime upon the scaffold. You must be prepared, then, for these most responsible vocations, and it is upon such questions as these, that those branches of your studies, which, in relation to the mere cure of disease, are correctly enough styled the auxiliary sciences, will be found to have an important and practical bearing. Do not, then, neglect them, or leave them to be "got up" for an examination, because you do not see that they can help you as mere practitioners of physic or surgery. Be sure that, if you do so, you will one day be found wanting; and, while you are jogging on slowly in the respectability of your gig, your rival, Dr So-and-so, who has regarded them as matters of utility, and has studied them accordingly, will be galloping into wealth and eminence in his carriage.

But I am not content to place the exhortation, diligently to study the auxiliary as well as the more essential branches of your education, upon the mere ground that they may now and then help you to make money. It is one of the happiest characteristics of our profession, that many of the subjects which we are called upon to study are not merely useful as matters of knowledge, but are in themselves, when a little trouble is bestowed on mastering their rudiments, full of the most delightful interest as sciences; and I say, that it betokens a thanklessness to a kind Providence, which has made the lines to fall to you in pleasant places, if you treat with indifference studies so attractive, when they are thus thrown before you in your very path in life.

Let this, then, be the spirit in which you enter upon your studies,—let nothing which you are called upon to learn seem to you an irksome, useless task; be assured that each branch is intrinsically of some value, and will show itself as such in course of time; let none of them, therefore, be slurred over at the beginning, to be hastily crammed up at the end, from the paltry fear of an examinator's questionings; but let each be attended to now with painstaking diligence, as a matter which will prove useful to you in your sphere of life.

Let me address an especial word to those who are now entering on their first season of study. Let me tell them that the first six months of a student's life will make or mar his character for the remainder. The real difficulty in all scientific studies is to acquire the first rudiments and general principles; gain these, and the subsequent details are clearly understood, easily learned, and readily retained in the memory; let these pass uncaught, and all that follows is confusion worse confounded,—words that go in at one ear and fly out at the other. Chemistry is an amorphous compound of carbon, phosphorus, and oxygen, compound radicals and the metals, to him who in his first session does not make himself acquainted with the atomic theory. Botany is a repulsive chaos of endogens and exogens, natural orders and breakjaw names, to him whose first summer leaves him as ignorant of vegetable organography as it found him. Anatomy, that foundation of all medical knowledge, is a perplexing maze of arteries and veins, nerves and tendons, ligaments and aponeuroses, to him who in his first few months has not taken pains to study the skeleton. Beware of trusting to what your too partial friends have called your genius or your talents. It is perseverance, not cleverness, that tells here. The hare in the fable was beaten by the tortoise, though the one was nimble of foot, and the other waddled in her gait. For she of the horny shell, with slower breath and colder blood, less gifted by nature for progress, but strong in her indomitable energy, was quietly and steadily getting over the appointed ground; whilst

her antagonist—sleek, smooth, and conceited—of quicker action and of hotter temperament, made short and brilliant bursts along the course, but was at last left behind, and doubly distanced, when she lay secure and sleeping in her form.

If I might venture on a metaphor, suggested by the fable, but drawn from a subject of which I happily know nothing but a few phrases, I would say to you,—Start in *your* race with a determination to go in and win. Remember that it is bottom, not speed, that will gain the stakes. Do not bound off with an inconsiderate gallop, which, by overworking you at the first, may bring you up breathless before you have turned the corner; still less trust to lying behind at the beginning, that you may make a rush near the winning post. It is a long race, over some heavy ground, in four heats, and you must win them all to be the victor. Let the pace be only fair and steady throughout, and whether you be the very first or not, you will at least be among the leaders, and will be placed by the judges.

Let me, however, return to a language more appropriate to the occasion of our meeting. Let me sum up what I have attempted to urge upon you, in a simple exhortation to study now as preparing for your active duties in life; and have no fears, that if you do so in honesty and sincerity, you will prove your studentship successful by your appearance as a candidate, and your prosperity as a practical man.

Thus far, gentlemen, my observations as to the disposition with which you should enter upon and prosecute your studies, have referred solely to the acquisition of skill and science; but I do not in the least forget that something more is required for your future well-being than a mere cultivation of the intellectual powers.

It is essential that you should acquire and retain sound knowledge, but it is at least equally essential, I may say more so, that you should form and preserve good moral character. This applies to every young man, whatever be his future professional destiny; but it is especially true of those who aspire to the medical profession. No talents, however good, no acquirements, however rich and varied, will ever enable you to overleap that barrier to success which a tarnished reputation will raise against you.

I need no arguments to enforce a statement so consonant with what is right, and so abundantly established by what is daily seen; but I should only half discharge the duty of this day, if I did not call you to remember that you are at that period of life when you are to form the character that is afterwards either to sustain or ruin you; and did I not warn you earnestly against the possibility of your now acquiring habits, from whose slavery you may never afterwards be able to extricate yourselves.

I do not fear that any accusation will be brought against me of having, in alluding to this, gone beyond my province, or overstepped the limits of my present duty. But it happens that I am placed in circumstances, by force of which, even if I were inclined to shrink from the office, I could not conscientiously do so. I have been intrusted with a message to you of a very solemn nature, and I never can have so fitting an opportunity of delivering it as the present. I restrict myself to this, for I have already occupied your attention too long. If I limit my remarks to warning you against a single temptation which may assail you, it is not that I think that there are no other rocks a head which need a beacon, but that I feel that an abler and better pilot is wanted to steer you through them.

And since I come to speak of a matter in the truest sense plain and practical—it behoves me to use the plainest language. It is not a subject on which I need to be mealy-mouthed, or require to use the slightest circumlocution. I name it plainly and bluntly;—it is my present object to warn you against places which have for sometime existed, and still exist, in the neighbourhood of the University, and of this Medical School, calculated to lead students into habits of intemperance. I am not deterred from this by the apprehension that an al-

lusion to the existence of such moral nuisances may prove injurious to the reputation of Edinburgh as a place of study; I know that the evil is not peculiar to this city; unhappily no town can be large enough to be the seat of a medical school, and remain free from such contaminations, and there is neither town nor village in which a young man, if unguided by sound principle, will not find opportunities of acquiring vicious habits. For good or for evil, when there is a will there is a way. But I have been long enough a practitioner of medicine to have learned, that when a disease is known to exist, it is the worst of all practice to temporise with it, from a pusillanimous fear lest the naming of it should give alarm; and what is true of corporeal, is not untrue of moral maladies. I openly, then, denounce these places. I care not whether it be the publicly declared tap-room, or the insiduous establishment, ostensibly a confectioner's shop in front, but really a drinking and smoking place behind. I warn you against them all; and I say that the student who sets foot in one of them, especially in a forenoon, on whatever pretext, has taken the first step upon a road which may open into the broad way that leadeth to destruction. Do not tell me that it is only for refreshment, and that there is no fear for your habits. There may be no fear, but there is danger; there has been danger, aye, and fatal danger too. You have no need to cross their thresholds. You can, without going much out of your way, procure all that it is required to sustain you in health and vigour for your work. I frankly confess my desire that some better provision were made for you in this respect. I have long wished that there were some place properly and legitimately fitted for a students' refreshment-room. The comparatively limited numbers in our school here, would not enable us to do this with success; but I feel certain that if such an arrangement could be made within the neighbouring walls of the University, and under the control of its authorities, it would be a great boon to you. I repeat, however, that there is no necessity laid upon you to seek refuge in the places formerly alluded to. Be you well aware of this, that they cannot be entered either with safety or with secrecy; the atmosphere is infected there; sooner or later they will proclaim themselves by their effects; and if you wish to keep your present character as students good, and your future character as men uninjured, avoid them. I know no consideration too strong by which to conjure you to shun these and every other scene of evil repute. You are not, I trust, indifferent to the obligations of religion; and you cannot pretend that you give heed to them, if you deliberately enter where vicious practices are permitted, nay fostered and encouraged. If this be an argument too high for *me* to use, let me come down a little lower. Let me appeal to your bare sense of honesty, and say if it be consistent with this that you should squander in such a way the money which a fond mother may have pinched herself in saving, or an anxious father may have toiled in gathering, to fit you for an honourable profession. I will come down yet lower. Let me appeal to your self-esteem and pride. Do you think that these places are established merely for the convenience of the general public, and that it is only accidentally that they happen to be so near you? "Springes to catch woodcocks." They are placed where they are, solely to entice *you;* and are you so silly as deliberately to step into a trap that is set and baited before your eyes? The very fact of your doing so, is conclusive as to a feebleness in your mind. This is no new doctrine. It is well nigh to three thousand years since the wise king of Israel saw the victim of temptation lured to his destruction, and thus left the record of his character:— "For at the window of my house, I looked through my casement, and beheld among the simple ones, I discerned among the youths, *a young man void of understanding.*" Let me point out to you one part of their practice, that you may learn what their system is. You will find them, after a short acquaintance, ready to give you credit. It is all they want. Let them get your name once on their books, and they know how doubly strong the temptation is for you to become more deeply their debtor. And they speculate on this, that sooner than that your folly should come to light, either you will direct the

money into their pocket from some legitimate use, or your guardians, to save you from disrepute, will make good the deficiency.

But if you will not be persuaded, either by the considerations of religion, of common honesty, or of self-respect, at least you may be restrained by the voice of experience. Let me, then, in a short but sad tale, show you their effects.

I was sitting in my study about midnight in spring, when the door bell rang, and being the only member of my household awake, and certainly the one that was wanted, I answered the summons myself. I found there one of my own students, pale and haggard, his head bandaged, and his hair matted with blood. He had come to seek relief from the pain of a severe wound of the head, which had been rudely and unskilfully dressed in a police office; and his suffused eyes, parched lips, and tremulous tongue unmistakeably showed how the accident had been met with. I was distressed to see it, because it tallied too well, as such habits invariably do, with his frequent absence from the lecture-room. I dressed his wound, sent him home, and hoped that in a few days all would be well, and that this, though a sharp, might prove a salutary lesson. There was little difficulty with the original hurt, but the severe fall which he must have had at that time, had so shattered his frame, little fitted to sustain an injury, that he went on from one bad symptom to another, until, in spite of my having the aid of some of the most accomplished practitioners in town, his malady tedious, painful, and loathsome, slowly exhausted him.

I was conversing alone with him one day on his precarious condition, when, with an expression of the most poignant anguish, he thus addressed me:—"Doctor, I know from what you say that I am dying, and I shall go to hell." Yes, gentlemen, it was this dreadful monosyllable that rung in my ears. It required little penetration to see that it was the first time that he had ever breathed his thoughts to another mortal. I shall not easily forget the thrill of horror that these words sent through me. I almost felt as if I saw the dread abyss yawning for its victim, and as if, so far as human means could be instrumental for his aid, there was none to guide him where to look for succour, but this feeble and unworthy hand. Thank God, it was not so; we were enabled to procure for him that spiritual assistance which he required, now that he so much more needed the divine than the physician. Into the results of the help thus given him during the few remaining days of his life, we dare not attempt to penetrate; but it did appear to be of happy omen, that during this brief prolongation of his existence, he thought seriously and kindly of you. On one of those days when I had finished my professional duty towards him, he addressed me, as nearly as I can remember, in these words:—"Doctor, you, as a lecturer, know a number of medical students, and have many young men going about you. I beg you to tell them to take warning by me: warn them to avoid that shop [I need not name it]; it was the ruin of me, by getting me into habits of idleness and intemperance; and it was especially the system of long credit that helped to work my destruction."

That was the message to you with which I was entrusted. He bequeathed the warning to you as a legacy; he named me his executor; I have now paid it over to you, and I demand a discharge from the trust. It rests with you now rightly or wrongly to use it.

My duty, gentlemen, is ended. I have detained you much too long; and I have now to thank you for the attention with which you have listened to me.

We have spent enough of time on our introductory address; but I trust that though deficient in many respects, it may not be altogether profitless to you. Let us now withdraw to our several class-rooms, to enter on the proper business of the new session. You, as pupils, must help us by your attention; we, as teachers, must help you by our exertions. We must pull together, heartily and cheerily, or the boat will not make way. Let us each, in our respective spheres, do his duty faithfully and steadily; and let us make the session 1851-52 happy and successful.

SURGEON'S HALL, EDINBURGH, 4th November 1851.

Part Fourth.

PERISCOPE.

MEDICINE.

HYDATIDS OF THE LIVER EVACUATED EXTERNALLY. BY M. LAFFORGUE.

A young female, after running rapidly down a hill, was seized with acute pain in the right hypochondrium. The ordinary signs of diseased liver soon supervened, and, notwithstanding the most varied modes of treatment, continued for five years. Her relations, believing that marriage would improve her condition, permitted her, in opposition to the advice of her physicians, to take a husband. The symptoms continued, though the disease did not appear to make progress. Six months after marriage she became pregnant. The symptoms now underwent exacerbation, but were soon mitigated, and the pregnancy advanced to the full term. The labour was natural, and the patient even nursed her child successfully. Finally, after the lapse of a year, her condition became more alarming, and M. Lafforgue was sent for. She was then in the following state:—Extreme emaciation, general blanching, excessive feebleness, impossibility of retaining food, which was rejected by vomiting almost as soon as swallowed, considerable tumefaction of right hypochondrium, descending to the crest of the ilium and transgressing the linea alba, obstructed respiration, orthopnœa. The respiration soon became more and more embarrassed, threatened with immediate asphxyia, the patient uttered stifled cries, demanded air, and seemed at point of death. M. Lafforgue, detecting deep fluctuation in the tumour which compressed the lungs, made with the bistoury, between the eighth and ninth ribs, an opening, which permitted the escape of a flood of thick serum. But the incision soon becoming obliterated by the presence of a thick granular matter, it became necessary to enlarge it, and immediately a hydatid, as large as a pear, was evacuated. The discharge was then re-established, bringing along with it hydatid sacs, of various sizes, some full, others completely empty, and with obliterated cavities; those which were so large as to stick in the wound were drawn out with forceps. In the space of a few minutes a vase, capable of holding six pounds of fluid, was filled with hydatids, and with a yellowish serosity of so fetid an odour as to annoy all the assistants.

The patient, after this enormous evacuation, fell into a state of extreme feebleness, the discharge was consequently arrested for the moment. Next day, however, uneasiness and dyspnœa were renewed, the bandage was removed, and a considerable additional quantity of hydatids and of fluid was evacuated. For six days the discharge continued spontaneously, and such was its abundance that it was estimated at twelve to fourteen pounds of fluid, containing several hundred hydatids. Gradually the symptoms disappeared, hydatids ceased to issue from the wound, and the fistula had nearly cicatrised, when the supervention of pains and uneasiness seemed to threaten a relapse. It proved, however, to be a second pregnancy, which led (by the pressure of the uterus) to the complete cicatrisation of the wound.—*Compte Rendu des travaux de la Société de Médecine de Toulouse; et Journal de Médecine et de Chirurgie Pratiques*, Aug. 1851, p. 344.

ON HYDROPHOBIA. BY PROFESSOR HAYNE, VIENNA.

Animals attacked by hydrophobia bite and snap at every object, without occasion, and insidiously. The bark is peculiar, passing into a piercing howl.

Dread of water is rare; they often rake straw into their water-vessels. They bite so fiercely any object held to them, that they often force out their own teeth. They growl at air-bubbles blown in water. They eat little, but swallow all sorts of indigestible substances, as straw, hair, paper, rags. They seem to have an aversion to nourishment, drink, air, heat, and light, but less to the latter than men similarly affected, and seek out dark and cool corners. The disease lasts four days, the bark becomes hoarser and feebler, the look fainter, the eye as if covered with dust, hazy, the under jaw partly paralysed, partly clenched (trismus). The physiognomy ferocious, the brow wrinkled. Vomiting frequent; bowels confined; at last great emaciation. *Section.*—Blood dark and greasy; occasionally dark blood-red sugillations in the gastric mucous membrane; sometimes the epithelium in part destroyed (hemorrhagic erosion); sometimes not a trace of hyperæmia. Liver full of dark-red blood. Spleen generally healthy, rarely dark. Intestines at times partially hyperæmic; occasionally a superficial redness of the mucous membrane of the fauces and gullet. Other organs healthy. Suppressed sexual appetite and anger may cause hydrophobia, or so envenom the bite given as to produce it secondarily. In Turkey and Kamtschatka it is almost unknown, and was so in Egypt, though formerly known there, till recent police regulations of Mehemet Ali have brought it to light. Hydrophobia is common amongst wolves, because the huntsmen destroy the females, and the males furiously fight for those left!!

For prevention, the author recommends muzzling; the speedy destruction of the bitten part by a red-hot iron, and the application of a ligature above it round the limb. Further treatment is best managed in an hospital, and must be humane,—close confinement being avoided, as productive of dread.

Dr Kuhnen recommends as remedies,—gentiana amarella, and campanula glomerata, &c., but particularly the root of spiræa ulmaria. A peasant, bitten by a mad dog, and already hydrophobic, got in three portions a piece of spiræa root the length of a finger; he became quiet, went home, vomited, slept, and awoke well.

Dr Chabaunon recommends the destruction of the bitten part with sulphuric acid, which he prefers from its powerful action; it may be employed so long as symptoms of hydrophobia are still unmanifested, as the poison is very slowly absorbed. Of fifteen so treated none died; while a sixteenth uncauterized did.—*Schmidt's Jahrbücher*, August 1851.—B.

EPILEPSY.

Drs Everts and Van Leeuwen contribute to the Dutch "Weekblad voor Geneeskundigen" a variety of statistical details, with reference to the periods of recurrence of the paroxysms of epilepsy, as observed in 30 patients, during a sequence of 183 days. The tables which they have constructed show the sum of all the attacks:—1st, Those by night and by day; 2d, Those in the fore part of the day, or in the after part, and in the fore or after part of the night; and, 3d, Those of each at eight different periods of every day. For the limit between each day, and between day and night, they have selected nine P.M. as a convenient period. From the results we learn, that of 13 epileptic females 1 only had a greater number of attacks by day than by night; while 12, on the contrary, had more attacks by night than by day. Of these 12, there were 9 who had a greater number of paroxysms during the fore part of the night than in the after part; and 2 of the 9, a considerably greater number between the hours of nine and twelve, than between those of twelve and three; while the 7 others had more nearly an equal proportion during both of these periods. The sum of the paroxysms observed was very considerable; and the result establishes, that the fore part of the night (that is, the portion between the hours of nine P.M. and three A.M.), must be regarded as the period during which epileptic females are most liable to the accessions of the disorder. Reckoning all the paroxysms together, which took place from six to nine P.M., and all those from nine to twelve P.M., we find the surprising difference of 136 to 480. With regard to the 17

male patients, the reverse of this may be said to have been partly established; though the periods of increased or diminished proneness to the accessions were by no means generally so distinctly marked as in the females. The greatest proportion of attacks occurred towards the hour of nine A.M. In epileptic females, therefore, the period of approaching repose appears to be that most exposed to the exacerbations of the disease, and in males that of awakening.—*Nederlandsch Weekblad voor Geneeskundigen*, 26th January 1851.

CONNECTION OF AMAUROSIS WITH GRANULAR DEGENERATION OF THE KIDNEYS, AND PROTRUSION OF THE EYEBALLS.

Dr Hays has recently called the attention of the Philadelphia College of Physicians to this association, which he has seen exemplified in several cases. Of these, Dr Hays has recorded four. In the *first* there existed, during life, decided prominence of the eyeballs, and after death, the kidneys were found to be affected with granular degeneration. In the *second*, amaurosis, anæmia, and marked prominence of the eyeballs followed a threatened abortion, for which the patient was largely bled, subsequently succeeded by convulsions, and abortion at the fifth month. These symptoms were followed by considerable dropsical swelling, and a highly albuminous condition of the urine. A granular state of the kidneys was believed to exist, but no post-mortem examination was made. In the *third* case, the patient had gradually become amaurotic, with the eyes slightly but very perceptibly protruded; dropsy and scanty coagulable urine were consequent symptoms, and after death the kidneys were found to be granular, and the liver affected with cirrhosis. The subject of the *fourth* case was a married lady, who, when pregnant with her first and only child, had become dropsical, and suffered from impaired vision. When seen by Dr Hays, the eyeballs were so prominent as to make her ashamed of being seen abroad. She was excessively debilitated, and very anæmic. She had puffiness of the face, and the urine was albuminous. Good diet and tonics were employed; and though she had suffered relapses, hopes were entertained of her ultimate recovery. Dr Hays farther noticed, in these cases, a peculiar brownish-yellow tint of the skin, particularly of that of the face.—*American Journal of Medical Sciences.*

[The perusal of these cases reminds us of the interesting observations of Dr Begbie on protrusion, or enlargement of the eyeballs, as a symptom of anæmia; and taking into consideration the fact of the existence of this peculiar condition in all Dr Hays' recorded cases, we think it very probable that the protruded eye is to be looked upon as a symptom of a more general affection than that of the kidney,—namely, of the altered condition of the blood presumed to exist in all such cases. Amaurosis, as a concomitant, and not unfrequent, symptom of Bright's disease, has long been known and appreciated in this country.]

SANITARY CONDITION OF ICELAND.

In no other portion of the world is there a population so singularly situated as that of Iceland. The country, stretching from the 63d degree of latitude to the polar circle, presents a monstrous labyrinth of rock, and ice, and fire, enveloped in storms. Its mass is almost wholly of volcanic origin, the mountains being chiefly constituted of tuff, with the various descriptions of trap and trachyte, and having their sides covered with glaciers. A fresh green clothes many of the valleys; but there is nowhere either tree or grove, unless a few stunted birches be entitled to the appellation. The indications of still active volcanoes, the fuming of hot sulphurous vapours, and the gushing of numerous boiling springs, the whole under a climate with a mean annual temperature of about 39° of Fahrenheit, or, in the more northern parts, of 33° only, are the leading additional features which complete the peculiar scene.

Treatises on medical topography, including whole countries, and especially in regions so remote, are rare in scientific literature, and, with respect to Iceland, must have been hitherto quite unprecedented. By a royal resolution, however, of 12th March 1847, Dr P. A. Schleisner was commissioned to make

an inspection of Iceland, and of the Vestmannöe, partly to institute an inquiry into the causes of an extensive prevalence of trismus among the infantile population of the last-named islands, and partly with the view of investigating the general hygienic condition of the country. From an abstract of the report of Dr Schleisner,[1] published in the "Norwegian Magazine for Medical Science," we proceed to extract a few of the more interesting particulars, adding here and there such brief comment as may suggest itself.

Dr Schleisner has arranged his materials in five divisions, embracing,—1st, The prevailing forms of disease and causes of mortality. 2d, A description of the epidemics. 3d, The proportions of births and deaths, and other numerical relations of the population. 4th, The physical and sanitary conditions. 5th, The medical organisation.

Though his stay in the country was not a protracted one, he had himself opportunities of observing and treating upwards of 300 cases of sickness; in addition to which he examined a series of annual medical reports, presenting lists of cases treated by the resident practitioners, both in public and private practice. In this way he was enabled to bring together nearly 3000 cases of illness, for examination and comparison. The tabular view which he has given of these diseases presents several remarkable points which instantly arrest the attention. Thus, in casting a glance along its columns, we find that liver complaints constitute not less than 12·6 per cent. of the whole amount of sickness. Of 327 patients personally examined by the author, 57 were thus affected, this smaller number thus showing even a still larger proportion of cases. The disease, it appears, is more prevalent in the inland districts than upon the coasts; and in one locality it was so abundant, that he found from two to three cases in every household. Males and females are alike liable to its attacks, the proneness to which is observed to increase with advancing age, till it reaches its maximum with men at between thirty and forty years, and at between forty and fifty with women. Even at between one and ten years so many as 26 were found affected. The universal cause of the disease was the generation of hydatids, chiefly acephalo-cysts, according to the views of the author; though it is probable that farther investigations will establish the presence of the echinococcus. The usual treatment is puncture, a measure which is had recourse to even more frequently by the quacksalvers than by the regular practitioners, and seldom with any immediate dangerous consequences, but equally seldom with ultimate success in effecting a cure.

Catarrhal and rheumatic affections, as might have been anticipated, appear also conspicuously in the table; the former constituting 6·7, and the the latter 9·49 per cent. of the cases recorded. Pulmonary consumption and scrofula are of rare occurrence; and the author here naturally directs attention to the possible influence of the large proportion of oleaginous matters which enters into the ordinary diet of the Icelanders. Delirium tremens, so often encountered in the northern parts of the Continent, is here seldom met with. The *spedalsk*, or elephantiasis, has decreased considerably since the beginning of the century.

A disease of great fatality in Iceland, but not included among those in the table, probably because rarely the subject of regular medical treatment, is the trismus neonatorum (Isl. *Ginklófi*), which chiefly prevails in the Vestmann Islands, and in the nearest portion of the main island, in which localities, from an average of the last twenty years, 64 per cent. of the children thus perish annually, from the fifth to the twelfth day after birth. This melancholy result is attributed in great measure, and with much probability, to the generally filthy and unwholesome condition of the residences of the population, and to the prevalent custom of carrying the infants to church for baptism when only

[1] Island undersögt fra et Lægevidenskabeligt Synspunkt af P. A. Schleisner, Dr medicinæ. Kjöbenhavn, 1849.

two days old, and, singular to tell, of rarely nursing them much beyond that time.

Besides the ordinary catarrh to which the inhabitants are liable, Iceland has had its periodical visitations of influenza, the latest epidemics having been those of 1816, 1825, 1834, and 1843, every one of which produced for the time a marked increase in the rates of mortality. Out of an average population of about 50,000, upwards of 9000 are estimated to have perished from this cause during the last century and a half. Typhus in the same period destroyed nearly 5000, an amount of mortality which in this instance appears to denote a more than proportionately great prevalence of the disease, as its type seemed to have been mild, and the deaths in several of the epidemics were only 1 in 30 of the cases treated. Scurvy was at one time of frequent occurrence, probably as a consequence of the famines which not rarely devastated the country, but of late years it has been much less prevalent. We might have anticipated that the nature of the food of the Icelander, taken in connection with the cold, tempestuous and moist climate, would have created a remarkable proneness to this disorder. But we ought, perhaps, to consider that the effects of the dried fish, and salted or smoked meats, eaten with only a very slight and rare admixture of vegetable substance, which constitute the ordinary diet, are sufficiently counteracted by the large quantities of acid butter and the rancid fat and oils which are used as condiments, and by the sour whey, diluted with water, which is the common beverage. Small-pox and measles have been extensive causes of mortality, while the influence of scarlet fever and hooping-cough has not been inconsiderable. Upon the whole, during the century and a half over which Dr S. has been enabled to extend his computations, he estimates that approaching to 50,000 individuals, of the always scanty population, have been victims to diseases of an epidemic nature. Of these, 21,684 have perished since the commencement of the present century.

In estimating the comparative mortality of the population, Dr S. states, that while of 1000 infants of both sexes in Iceland, 569 will reach an age of only fourteen years, the same proportion in Denmark will attain the age of thirty-eight years in males, and of almost forty-one years in females. This is mainly accounted for by the violence and frequency of the epidemics; though, even independent of these, there are abundant other causes in Iceland which tend powerfully towards lowering the amount of expectation of life. In a population thus situated, the statistician needs scarcely occupy himself with ascertaining the rate of increment. The wonder is that it should maintain itself stationary.

The houses of the Icelanders are nearly in the same condition now, as in the times of the origin of their peculiar literature and history. They are either built of turf or of unhewn stones, or of both in alternate layers. The roof also is covered with turf, so that in summer the low edifice resembles a green hillock. The interior is dark and damp, for there are no proper windows, and therefore neither sufficient light nor ventilation. Fuel is miserably scarce, and that employed of the very worst description: dried dung of sheep and cattle, fish bones, carcasses of sea-fowl, &c. The stench from the smoke thus created, along with that from filth otherwise accumulated, penetrates everywhere, and becomes intolerably offensive. The usual clothing is of coarse woollen, and personal cleanliness is habitually neglected. The chief departments of economic industry are the fisheries, and the rearing of sheep and cattle; while a minor, yet still important branch, is the capture of sea-fowl. Yet the people thus situated are attached to their country; and a certain amount of elementary education is generally diffused among them. This may possibly excite surprise; but there can be none, that with such a diet as we have previously hinted at, and under hygienic conditions otherwise so faulty, they should be subjected to an excessive mortality. Von Troil, who visited them about 1770, might well tell us, that he hardly remembered to have ever seen an Icelander laugh. But, if there

be little hilarity, it is but justice to say, that their many privations are endured with uniform patience.

The medical organisation of the country appears to be wholly inadequate. While there are 187 priests and 294 churches, there are only 8 publicly appointed medical practitioners, and not a single hospital. The condition of the practitioner is also very unsatisfactory. While the district allotted to him is necessarily of extravagant dimensions, his salary is only about L.33 of our money, and his emoluments from private practice, or from other resources, add little to the amount. There are only three qualified midwives ; but abundance of women who practise obstetrics, for the most part with an utter deficiency of all requisite knowledge. Hence, it is unfortunately by no means rare, that women in childbed should die undelivered.—*Norsk Magazin for Lægevidenskaben*, B. iv., H. 10, 1850.

MIDWIFERY AND DISEASES PECULIAR TO WOMEN.

THE INFLUENCE OF PREGNANCY, CONFINEMENT, AND NURSING, ON THE DEVELOPMENT AND PROGRESS OF PHTHISIS. BY M. DUBREUILH, JUN., OF BORDEAUX.

M. Grisolle, in name of a commission, consisting of MM. Louis, Bricheteau, and Grisolle, read to the Academy of Medicine of Paris, on 7th October, a report on a memoir by M. Dubreuilh on the above subject. In a paper presented to the Academy, under nearly the same title, two years ago, M. Grisolle, upon the strength of some observations which had shown uniform results, concluded that pregnancy had not that suspending effect on phthisis, which has hitherto generally been attributed to it; that, so far from prolonging the life of the patient by arresting the progress of the tubercular lesion, it shortens life by hastening the advance of the malady. M. Grisolle, in putting these observations into shape, expressed an expectation that other observers, to whom he appealed, would only confirm his remarks. This is the object of the present memoir by M. Dubreuilh, jun., which was laid before the Academy on 24th December 1850, and on which M. Grisolle was appointed to report.

In the cases collected by M. Dubreuilh, which amount to thirteen, the phthisis appeared, or at least was recognised, during the first three months of gestation. Of this number, four women appeared to be in the enjoyment of excellent health at the time of conception, whilst the other eight (*sic in orig.*) had already presented for a greater or less time some symptoms more or less suspicious. Contrary to what would be expected in conformity with the ideas generally prevalent, in all these cases the symptoms, instead of improving, were seen to declare themselves more unequivocally, and the disease made progress. In none of the cases of M. Dubreuilh, as in none of the cases formerly collected by M. Grisolle, was the suspending action of the pregnancy observable. On the contrary, it seems to play the part either of a determining cause or of an aggravating circumstance.

According to the reporter, the cases where the first phenomena of phthisis develop themselves at the beginning of pregnancy, and during a state of health heretofore good, are more common than those where the pregnancy is consecutive to the first symptoms of the organic disease. He has observed that women decidedly phthisical become, contrary to what has been said, rather seldom pregnant. M. Dubreuilh has also observed this circumstance, and, according to M. Delafond, the same is true of the lower animals. The author has, moreover, remarked, that phthisis co-existing with pregnancy does not undergo in the leading symptoms any remarkable modification. This had also been pointed out by M. Grisolle, who had even noticed in his former researches that the state of pregnancy did not modify nor render more frequent certain phenomena of the disease, such as dyspnœa and hæmoptysis, as might have been expected.

M. Dubreuilh has expressed an opinion relative to the progress of phthisis

during pregnancy, in which the reporter does not agree with him,—viz., that, during the later weeks of gestation, there is a sort of interruption of the morbid action. M. Grisolle maintains, that if this does occur in some cases, they are only exceptional. It is rare, according to him, that phthisis, complicated with pregnancy, presents in its progress those intermissions or temporary suspensions which are so common in ordinary tubercular disease. He has invariably seen it make an ascending progress, and become complicated with all the accidents which can occur in the course of phthisis.

The author has also investigated the effects of delivery and of the puerperal state on the progress of phthisis, and he seems to be led to believe that these conditions hasten, oftener than M. Grisolle has stated, the progress of the disease, whatever in other respects be the period of the malady. He thinks that these new conditions can develop phthisis completely, provided the predisposition exists,—or accelerate the fatal termination, when the malady has already declared itself. According to M. Grisolle, that occurs sometimes, but not so often as M. Dubreuilh supposes. M. Grisolle adheres to the opinion, that delivery is rather to be wished for than feared; for if some women, already quite exhausted, sink soon after, it is very rare that this happens if the pulmonary disease has not passed the first or second stage. It is, then, more common to see the symptoms improve; there may even be such a suspension of the disease as to lead to the belief that there has been a cure.

M. Dubreuilh, wishing to clear up all parts of the question, has investigated the influence which phthisis exerts upon pregnancy, and he agrees with M. Grisolle in remarking, that most phthisical women go to the full term; that the most of them have easy labours, with little pain; and that attempts at nursing always produced deplorable results both to mother and child.

After this analysis of the details, the reporter thus sums up the value of M. Dubreuilh's whole memoir:—

The questions which M. Grisolle has propounded are important and practically interesting in the highest degree. He has once more shown that marriage, that popular remedy for all the ills of young women and for debilitated constitutions, ought always to awake the anxiety of the physician. Observations now abound to show, that pregnancy, far from being a fortunate circumstance, is, for women who have either an innate or acquired predisposition, often the determining cause of tubercular disease of the lungs. The co-existence of pregnancy with phthisis, instead of being a fortunate occurrence, as is supposed, increases and hastens the danger. We, therefore, are not entitled, with an author of last century, to say, that of two women, equally consumptive, she who becomes pregnant will surely live to the end of the period of gestation, whilst the other will die before that time. To be near the truth, the proposition should be reversed. M. Dubreuilh therefore deserves credit for having helped to destroy an erroneous and dangerous opinion.—*Gazette Médicale*, 11th October 1851.

[MM. Grisolle and Dubreuilh appear, from an article in the same number of the Gazette Médicale, to have come to their conclusions in this way:—They find the mean duration of phthisis in pregnant women to be nine months, in non-pregnant women fourteen or fifteen months; and therefore they conclude that pregnancy does not retard but accelerates phthisis. These statistics may help to shake our confidence in the old opinion, but do not establish the new doctrine. The true way of settling the question is, by observing the symptoms and progress of well-marked phthisis before, during, and after pregnancy, in individual cases. A few well-observed facts of this kind will be more precise and significant than the most laborious statistics of the average duration of phthisis in pregnant and non-pregnant women.—*M. Delchambre, in Gazette Médicale, ubi supra.*]

INFLUENCE OF THE OVARY IN MENSTRUATION. BY DR HANNOVER.

With regard to the menstrual function (says Dr H.), the uterus may be considered to be a blood-secreting organ, and corpora lutea may be formed, even if

the uterus be removed, as Bischoff has proved by experiments on rabbits. That the uterus does not contain the real cause of menstruation is seen from a case related by Moss, in which, notwithstanding the extirpation of the uterus, the menses continued to flow from the vagina. The determining point of the whole menstrual function is in the ovaries; when these organs are removed, both rut and menstruation cease. The rut does not return in animals whose ovaries have been removed, the same being the case with regard to menstruation in women. Thus Pott reports a case of a woman, twenty-three years of age, who suffered from an inguinal hernia, which included both ovaria: she was otherwise healthy, had large breasts, and menstruated regularly. Both the ovaria were removed; the patient recovered, but she grew thin, her breasts shrank, and menstruation did not again occur. Another girl, in whom the uterus was wanting, but the ovaries were probably sound, experienced the periodical menstrual molimen without discharge of blood. Robert also states, that the catamenia disappear in women living in Central Asia, whose ovaries have been excised. I observed the case of a widow, in Frederick's Hospital, of a healthy constitution, who died at the age of twenty-two years, and who had never menstruated; whether she had ever experienced the menstrual molimen, I unfortunately had not ascertained. The uterus was of the size and form of that of a child fourteen years of age; its parietes were scarcely two lines thick, its cavity contained a sanguinolent mucus; the tubes, as well as the ovaries, were small; the latter were rather loaded wth blood, and contained no Graafian vesicles. The urinary bladder, kidneys, and external organs of generation, were in a natural state.

Although this last case is not entirely conclusive, nevertheless the part the ovary performs in the function of menstruation is evident from the preceding observations. It is even probable that the menstrual discharge itself is determined by the ovary; so that it is closely connected with the hemorrhage that attends the detachment of the ovum from the ovary, and with the filling of the emptied Graafian vesicles by blood from rupture of the vessels. The following case proves that this hemorrhage may be considerable:—

M. H., forty-one years of age, was brought to the hospital on the 23d of June 1845, having been ill nine days. She died on the 6th of August from a cerebral tumour, and softening of the brain. She had menstruated before she was brought to the hospital; she menstruated on the 8th of July, and again also shortly before she died. On examination after death, the uterus was found to be enlarged; its cervix contained a cyst, the size of a nut, filled with albuminous fluid. In one ovary a coagulum of blood was found. Both tubes were distended by a considerable quantity of liquid blood.—*London Medical Gazette*, October 10, 1851.

THE OFFSPRING OF DEAF AND DUMB PARENTS.

Of about 200 deaf mutes who attended a festival at the asylum in Hertford, last September, 103 had been or are now married. Both parties were deaf and dumb in forty of these marriages; in twenty-three one could hear and speak. From the thirty-fifth annual report we learn, "that thirty-one of these couples had not become parents, but the remaining seventy-two were the parents of 102 children, of whom ninety-eight could hear and speak, and *four* only were deaf and dumb. One of the four was the only child of his parents, both of whom were congenitally deaf. Besides the parents, the paternal grandfather, a sister of the father, and two sons of this sister, were deaf and dumb; and on the mother's side, an aunt of the child was deaf and dumb. In the other family, that of the three children, the father lost his hearing by disease at two years of age, and had no known relative deaf and dumb. The mother was born deaf, and had a deaf and dumb brother. These were the only cases of the kind present at the festival. The question, whether the children of a deaf-mute parent are more likely than others to be deaf and dumb, has not been solved in any other country so satisfactorily as in our own. In Europe comparatively few of the deaf and dumb have married, and those we believe have generally selected hearing partners.

Here, however, in the large majority of cases, both husband and wife are mutes. The per centage of deaf children, the offspring of such marriages, is undoubtedly somewhat greater than among an equal number of the children of other parents, taken like the deaf and dumb from the various conditions of life. But if the facts above stated afford a fair criterion for judging on this question, we are safe in the belief that this result might be obviated in a great degree, if *intermarriage* could be prevented between those deaf-mutes whose parents or other near relatives seem to have been constitutionally prone to the infirmity of deafness. This could only be done by moral suasion; but if influential people, and especially the parents of deaf-mutes, were deeply impressed with the idea that marriage should not take place between two persons, each belonging to a family in which the great evil of deafness was constitutional, lest the offspring of that union should be deaf and dumb, much might be done towards preventing the extension of the evil. But there are other facts on this subject which would seem, at first view at least, to confound the speculations we have based on the cases above mentioned. For instance, a husband and wife, once pupils here, were both congenitally deaf; but neither, as we believe, has had any deaf and dumb relative, except their three children, and these were all deaf from birth. The circumstances of another of our former pupils and his family connections are as follows:—His brothers and sisters hear, and they married hearing persons. One of the brothers has two deaf and dumb children, one sister has one, and another two deaf and dumb children, and one of the latter has a deaf and dumb child. There is, besides, one other deaf-mute in the circle of relatives,—possibly more than one, though no one of the ancestors is known to have been a deaf person. The man first referred to married a deaf and dumb woman, *and their five children all hear and speak."—Boston Medical Journal,* July 1851; and *London Medical Gazette,* September 26, 1851.

Part Fifth.

MEDICAL NEWS.

PROCEEDINGS OF THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH IN REGARD TO HOMŒOPATHY.

A VERY full meeting of the Society was held on Wednesday the 19th December, there being no less than *sixty-four* members present. After some public business, of which we shall publish an abstract in January, the President, *Dr Begbie,* stated, that Mr Syme had a motion to submit to the Society, and that the Council had determined to leave it entirely to the wishes of the members present to decide whether the discussion of this motion should take place in the course of private or of public business. *Dr Christison* then moved, and *Dr Maclagan* seconded the proposal, that strangers be requested to withdraw. Afer some remarks by *Dr Andrew, Dr Macaulay,* and *Dr Alexander Wood,* the *President* announced that Dr Christison's motion was carried unanimously, and visitors were accordingly requested to retire.

Mr Syme in moving "*That the public profession of Homœopathy shall be held to disqualify for being admitted or remaining a Member of the Medico-Chirurgical Society of Edinburgh,*" said,—That in addressing the Society upon this occasion, he considered it equally unnecessary and inexpedient to offer any exposition

of what were called the principles of homœopathy, or enter into any inquiry as to the soundness of their foundation. He proceeded upon the supposition, that all the members of the Society had formed an opinion in regard to this subject. If they had formed the same opinion which he had done, it would of course be useless to say anything in favour of it; and if they had arrived at a different conclusion, it was not to be expected that any statements of his could then produce a change in their sentiments. In short, the object of his motion was not to influence their judgment, but to afford them an opportunity of expressing it.

Before proceeding any further, he begged to relieve himself from the imputation of inconsistency, which Dr Henderson had laid to his charge. In a letter addressed to the patrons of the University, Dr Henderson, after commenting upon certain proceedings of the Medical Faculty, which he had unwarrantably published without their authority, went on to say:—"And what makes this rapid change all the more remarkable is, that it is singularly inconsistent with their conduct during the previous seven years that their colleague has been known over the three kingdoms as a believer in homœopathy. Thus, the Professor of Clinical Surgery, who took the lead in this proceeding of the Faculty, was the mover, in the College of Surgeons a short time previously, of a resolution which bore, that meeting in consultation with those who practise homœopathy is to countenance the system; yet he himself has repeatedly given such countenance to it in this city. Lest he may have forgotten the occasions, I may remind him of two that fell within my own knowledge. The one occurred a few days before he set out to take possession of his London appointment, the other a few days after his return from that enterprise."

This charge had been eagerly adopted by the London medical periodical press, which from week to week represented Mr Syme as guilty of the grossest inconsistency, in publicly opposing what he had privately countenanced. Regarding inconsistency of conduct as equivalent to a practical falsehood, he should have considered himself, if guilty of it, as quite unworthy to bring this matter before the Society, and was therefore glad to have an opportunity of declaring that the charge in question had no foundation whatever, as the following facts, he hoped, would show.

Little importance had been attached to homœopathy in Edinburgh, until, about seven years ago, reports began to circulate that it was introduced into the clinical practice of the Royal Infirmary. Mr Syme being then Dean of the Medical Faculty, had felt it his duty to inquire officially if it was true that Dr Henderson had treated a fatal case of acute pneumonia by infinitesimal doses of the tincture of phosphorus, and having received a reply in the affirmative from him, intimated that he must bring the matter before the Medical Faculty. He did so, and the result in a very short time was, that Dr Henderson ceased to be a physician of the Royal Infirmary, and a Professor of Clinical Medicine in the University of Edinburgh. Mr Syme thought that after this very distinct demonstration of his sentiments towards homœopathy, it should have required some very decided evidence of an opposite kind to warrant the allegation of Dr Henderson. In order to avoid any room for cavil on the subject, he had written to Dr Henderson for specification of the cases to which he had referred in his letter. The first of these he would mention was that of a young man from Dundee, whom he had been requested, by Dr Nimmo of that place, to take under his charge. Finding that he was attended by Dr Henderson, he had requested a meeting with him, for the purpose, not of consultation, but of arranging for the medical treatment of the patient by the respected secretary of the Society, Dr John Taylor, with whom Mr Syme might co-operate, as he could not do so with Dr Henderson. The other case was that of a child, respecting the state of whose spine he was asked to give his opinion a few days before leaving Edinburgh for London. He had then met several members of the profession for the first time, and being under no pledge or implied obligation not to meet Dr Henderson, did not think he would have been justified in declining to do so, under the peculiar circumstances of that occasion. As it might be supposed that he had perhaps been less scru-

pulous with regard to the other homœopathic practitioners in Edinburgh, viz., Drs Russell, Black, and Wielobycki, he would add, that he had had no communication whatever with the first mentioned for at least seven years. That the second had proposed to attend along with him a lady who had come from England to undergo an operation, but met with a decided refusal of any such co-operation; and that the last mentioned had in vain importuned him to render the surgical assistance which was required by a patient under his care. Such being the whole amount of countenance which Mr Syme had afforded to the homœopathists of Edinburgh, he hoped the Society would think him justified in stating, that Dr Henderson's charge was utterly devoid of foundation.

Mr Syme then said that he would endeavour very shortly to explain what he conceived to be the duty which became a member of the profession, in regard to homœopathy. As an individual member, he had long resolved not to adopt homœopathy, regarding it as mischievous folly; and not to oppose it, as being one of those popular delusions against which reason is of no avail. As the member of a licensing board, he did not conceive himself entitled to reject any candidate, except on the ground of having failed in complying with the prescribed course of study, or acquiring the expected amount of information. If a candidate were base enough to obtain a diploma, by expressing sentiments as to the uses and doses of medicines different from those really entertained by him, he would be clearly guilty of fraud, for the odium of which he should be answerable to the public. But it was plain that examining boards could not prevent any such fraudulent proceedings, any more than they could anticipate the perversion of those who had received their diplomas, and that any attempt to do so by acting upon private information, while of very questionable legality, would tend to increase the evil by encouraging implicit confidence in all who obtained diplomas.

The powers of licensing boards being thus limited, it was the more incumbent upon members of the profession to use whatever other means were in their power to maintain their principles pure and unsullied. For this purpose a society, such as the Medico-Chirurgical, had a clear line of duty. It was a voluntary association for cherishing sound principles of practice, for elevating professional character, and for promoting good fellowship. But if any member should depart from the principles which he had professed on entering the Society, should ridicule and revile all those who adhered to these principles, and place himself in opposition to all the feelings respected by the profession, there could be no doubt as to the propriety of desiring him to withdraw from their body; or if any one in the same state of mind should apply for admission, it was surely their duty to exclude him. In fact, the homœopathists had anticipated their wishes, since they never appeared at their meetings, and shunned their brethren in private, however intimate their relations with them might have been previously. He, therefore, trusted that the motion he had now to make would be unanimously adopted by the Society.

Dr Simpson seconded Mr Syme's motion. And as Mr Syme had taken the present opportunity of answering some of the charges which had been brought against him as to countenancing Homœopathy, Dr S. hoped the Society would accord him also the same privilege, of replying to similar charges which had been brought against himself, from the same quarter, and on the same subject. Dr S. had not read the pamphlet from which Mr Syme had quoted, nor any of the other pamphlets published during the recent discussion; but he had read the first letter which Dr Henderson addressed to one of the Edinburgh newspapers. It appeared at a time when Dr S. happened to be absent from Edinburgh, and he first heard of it when in England. In that letter Dr Henderson stated, that Dr S. had met with him in consultation in the treatment of some cases. On the morning after he arrived in Edinburgh, he waited upon Dr Henderson at his own house, to ask him what these cases were. He had, within the last few years, on two occasions, watched, for some long anxious hours with Dr Henderson, at the bedside of Dr H.'s own wife, but she was assuredly treated most allopathically, not homœopathically; and, in doing so, no one would accuse Dr S. of abetting or countenancing homœopathy. But, in confirmation of his statement, Dr Henderson

mentioned two patients whom Dr S. had attended,—one for uterine disease; the other for disease of the labium. Dr Henderson previously attended these cases, and had certainly asked Dr S. to take charge of them, and Dr S. had done so; but as certainly he did not attend them with, or for, Dr Henderson. Very likely he might have told Dr Henderson, when he happened to see him, what view he took of the cases, but he understood he had full and uncontrolled professional charge of them, and their treatment; and, in treating allopathically what had resisted homœopathy, he was not in any way, he conceived, countenancing homœopathy itself. He applied potassa fusa (not a homœopathic proceeding) to the cervix uteri, to remove the state of indurated hypertrophy and induration. In the other case, after trying various measures (iodine, nitrate of silver, &c.), to reduce a hypertrophied labium, Mr Syme went with him, and removed a large portion of it with the knife. Dr Henderson adduced another case as one he had alluded to. It was an instance of a small pediculated uterine polypus, the projecting body of which was offering to ulcerate. Dr Henderson had asked him to see this patient, and was present when Dr S. removed the polypus with the scissors. Dr Denman had years ago laid down, as a rule, the necessity, when practicable, of having the evidence of some other practitioner as to the real existence of a polypus; and Dr S. had felt this necessity strongly in the present instance, as the lady had been long the patient of a celebrated London accoucheur, who had been intimate with Denman, but who had overlooked the existence of the polypus, and would possibly, if informed of the measure adopted, deny its propriety. Dr Henderson was the only medical man that the lady would, he believed, have allowed to be present; and she required no kind of constitutional treatment. With the exception of his own wife, this was the only patient at whose bedside he had met Dr Henderson; and the case was one which did not involve, in any degree, any allopathic or homœopathic principle of treatment. But, even though in this Dr S. did not countenance, or intend to countenance the practice of homœopathy, he now believed he acted wrongly. With the views which he had latterly come to hold on the nature and character of the so-called practice of homœopathy, he conceived he had erred in even going thus far. So much had he felt this, that when, two years ago, he gave an account of the above, and some other cases of polypus, to the Society, and afterwards published that account in the Monthly Journal for January 1850, he stated always (in confirmation of the accuracy of his report, and in accordance with Denman's rule), the names of the other practitioners who had seen the cases with him, when others had been present; but, in the case alluded to, he omitted the name of Dr Henderson, as he felt it was perhaps more than questionable whether he should have met him, at even a slight surgical proceeding, such as the above. The Society, he believed, would agree with him as to the fact, that the profession here, as elsewhere, were not fully awakened to the duty in this respect which they owed to themselves, and to the public, till comparatively lately. Even if Dr Henderson's allegations were all true to a far greater extent, they only amounted to this, that the profession have hitherto been over-indulgent to him and his heresy,—believing, as most of his old medical friends did, that the delusion could only be at most but a temporary one on his part. But because they had been considerate and tolerant to him in times past —and few men taking up such a heretical position as he had done, had ever met with greater consideration and tolerance—this was no reason why they should continue to extend to him their hopeless kindness for the future. And for one, Dr S. rejoiced that the Colleges had taken up the subject, and set the matter on its right footing, by making the question of meeting homœopathists not a question longer left to the responsibility and importunities of individuals, but a question which the profession had fixed and settled in their corporate capacity. The resolutions of the Colleges would, he believed, be doubly useful by not only determining for the future the proper line of duty of the profession towards homœopathists, but by showing also to the homœopathists their exact position in relation to the profession. Almost all the fellows of our

two Colleges of Physicians and Surgeons belong to the Medico-Chirurgical Society; and in their capacity as fellows of their respective Colleges, have already unanimously approved of the resolutions of the Colleges regarding the propriety of excluding homœopathists from our professional communion. Most, if not all, of the members of the Medico-Chirurgical Society who do not happen to be fellows of either of the Colleges, hold, I believe, the same views. In requesting, therefore, the practitioners of homœopathy to withdraw themselves from us, the Medico-Chirurgical Society is merely, so far, carrying into practical effect the resolutions of the Colleges on this same subject. And most of the members are probably aware that while very strong legal doubts exist as to public chartered corporations, like the two Colleges, having the power of exercising this right, fortunately, any private unchartered Society, like the Medico-Chirurgical, need fear no legal obstacle in enforcing their judgment. In passing therefore—and it is to be hoped, unanimously—such a motion as that proposed by Mr Syme, we, the members of the Medico-Chirurgical Society, show our anxiety and desire to be rid of the professional presence of homœopathists in all our institutions, by showing them our determination to be rid of them in those places from which we have an undoubted right of enforcing their exclusion. Every proper feeling of respect for ourselves, and every proper feeling of regard for the advancement of the great science which we cultivate, seems now to call upon us for this disagreeable step. For years past they have been allowed, and almost without gainsay or resistance or our part, to hold up to the lay public all the best acknowledged principles of our medical philosophy and practice,—principles that have stood the test of thousands of years,—as delusions and errors. And they cannot surely deem us either rash or severe in our proceedings, if slowly, but at last, we turn round and ask them, as we do to-night, to remove themselves from amongst men whose opinions they systematically affect to deride and decry. Nay, what good can they themselves derive from pertinaciously adhering to a Society like ours, composed of professional men banded together with the view of cultivating medical science in a mode and on principles which they at one time professed to honour, but which they now profess to repudiate? As Mr Syme has properly observed, they have in fact already all but separated from our Society, inasmuch as they have alienated themselves from our Society and College meetings and discussions, as if ashamed of the very step which they had taken. When we ask them to retire from our Society, as Mr Syme's motion proposes, we ask them merely to sever entirely a tie which they have already virtually broken. Dr Simpson further argued for the propriety of this motion on another ground,—viz., that the presence of homœopathists at our Society, even if they did come to our meetings, was not at all calculated to promote that principal and leading object of the Medico-Chirurgical Society,—the cultivation and advancement of medical science and knowledge. For he held that no man, or set of men, could in any degree aid in effecting this object, who entertained principles such as those which form the basis of the homœopathic belief. We were as much justified in asking those who had taken up a belief in the follies of Hahnemann and his Organon to withdraw from our Society, as a Christian congregation would be justified in expelling those of its members who came to believe in the follies of Joe Smith and the Mormon Bible. These two heresies—the homœopathic and the Mormonite—seemed, in fact, to have many points in common, though the one belonged to medicine, and could only be properly judged of by physicians; and the other belonged to theology, and could only properly be judged of by clerical men. They were both equally absurd. Some homœopathists profess Hahnemann to have been actually inspired, as the Mormonites hold Joe Smith to have been. Both heresies also were extending, it was true, but that was no evidence of the value of either. It has been alleged that we have no confession of faith in medicine,—no standard by which we can possibly judge, as clerical bodies do, of the amount of error and deviation of those members of the profession who, from time to time, may choose to set at nought the common principles of that profession. But we have a

confession of faith, and a standard by which we can judge such men, namely, THE STANDARD OF COMMON SENSE If we judge by this simple standard the opinions of Hahnemann and his followers, we will find them sadly, very sadly, wanting. In fact, all the dogmas and doctrines peculiar to him, are a tissue of the strangest contradictions and wildest absurdities. If a grown-up man were gravely and seriously to assert to the world that two and two make five, the world would be inclined to look upon him as doubtfully rational, inasmuch as he defied the principles of common sense. And when other grown-up men tell the world that they can cure this or that disease with a billionth or decillionth of a grain of this or that common and probably even inert drug, they express an opinion perhaps even more intensely and directly absurd than the doctrine of two and two making five. All men can at once, because without any great intellectual effort, see through the foolishness and impossibility of the idea of two and two making five, but they do not equally see through the absurdity and impossibility of the more complicated, but equally ridiculous idea, of the billionth or decillionth of a grain of sulphur or of chamomile, or belladonna, or the like, having any possible effect whatever upon the economy, for, resting contented with the mere name, they never once think or dream of what in reality a billionth or a decillionth amounts to. Medical jurists and lawyers know of no poison, however strong and powerful, the billionth or decillionth of which would in the least degree affect a man or harm a fly. They tell us that the mere division of their drugs invests them with "tremendous" power; but they could not injure or affect the frailest insect with one of their "tremendous" billionths or decillionths. For what, in fact, is a billionth of a grain? Why, if a single grain of sulphur were divided, as the homœopathists use it and other drugs, into billionths, and if our common parent, Adam, when called into existence some 6,000 years ago, had then begun swallowing a billionth every second, and if he had been permitted to live up to the present time doing nothing but swallowing night and day 60 billionths every minute, he would as yet have completed only a small part of his task. It would require him to work and swallow at the same rate for 24,000 years yet to come, in order to finish one single grain of a drug, which has little or no effect on his present descendants in doses of many grains. Yet the homœopathists believe that a few of the sulphur billionths which Adam would have swallowed during these 30,000 years would cure, forsooth, an attack of jaundice. If we appeal to our standard—*the standard of common sense*—should not men holding principles of such unmitigated ridiculousness be requested to withdraw from this and other medical societies? But a billionth was, after all, only the sixth dilution of a grain. Homœopathists have dilutions far more refined, and doses far more infinitesimal than that. Some amongst them allege that drugs should rather be used in their thirtieth dilution, or in decillionths, and even some aver, that the smell of such a dose is enough. When they speak of the decillionth of a grain, they seem little to reckon what a decillionth amounts to. For it is a sum, the mere figures of which can scarcely give us any conception of its infinitesimal amount, [viz.,—
1,000,000,000

would extend several times from the sun to the outermost planet in our system, or even as far as the nearest fixed star. A grain of belladonna or other of their drugs, distributed in such decillionth-dose globules, would make continuous lines many million times longer than all the railroads and other roads that ever were, or ever will be, formed on the face of the earth. And yet, say the homœopaths, take a pin's point and lift out a few of these belladonna globules spread out on these interminable lines of road, and you will cure with them a case of scarlet fever. Surely men holding such fantastical doctrines, are not men mentally fit to be members of such a Society as this. They are heretical to the extent of utterly outraging the confession of faith that we use in medicine, — *the standard of common sense*; and they cannot rightly deem us tyrannical if, in consequence, we request, in such a Society as this, to be relieved of their companionship. One remark of Mr Syme's regarding the number of homœopathic practitioners in Edinburgh, reminded Dr S. of a curious feature in homœopathic practice among us. He was not sure of how many practitioners of homœopathy we had in Edinburgh; but they were all versant with the fact, that there were three homœopathic drug shops in town. That fact was in itself a significant and illustrative fact. He did not know the number of drugs that homœopathists used, but he did not suppose they exceeded 250 or 300 separate articles. He was not aware of what a grain of each might cost; but he imagined, not above a penny or two at most, on the average. And one single grain in even their fourth or fifth dilutions, would, of course, be sufficient during any one druggist's life-time, not only for a whole town such as Edinburgh, but for a whole universe; while a grain of a drug divided into quintillionths or decillionths might in truth serve an entire race during an entire geological epoch. A homœopathic apothecary's stock in trade could not consequently well exceed a few shillings, or a few pounds at most, and need not surely require renewal during the longest life time. And yet these same homæopathic dilutions seem convertible, through an adequate amount of credulity on the part of the public, into annual incomes sufficient for the maintenance of three thriving drug establishments! To effect this, the druggists must sell their pharmaceutical exiguities at something like a billionth or decillionth of profit. He did not blame the druggists for this, or for charging, as they did, very smartly for the globules which they sold. Their high price was indeed in itself a more potent therapeutical agency than any problematical medicinal matter which they contained; for what a man pays high value for, must, he naturally believes, be of high value. But surely we may well smile at the deluded buyers zealously emptying their purses in the purchase of such dreamy nonentities. Nor did it seem necessary that the globules of sugar of milk should go even through the form of being medicated with their supposed quadillionths, quintillionths, &c., of the drug whose name they bore. At least it had been averred that some in England who pretended to manufacture these medicated globules on the large scale, had found that the sugar of milk, of which these globules were composed, answered just as well with or without a dip in the pretended dilutions. And as far as regarded the patient and the disease, it mattered nothing what globule was given— whether one of charcoal or arsenic. They were both so diluted as, of course, to be entirely inefficacious; and all the drugs and globules in a homœopathic medicine-box being rendered equally null and innocent by their previous reduction, any one of them could readily and successfully replace any other. Early in the history of homœopathy in Edinburgh, Dr Simpson had witnessed a circumstance proving, if any proof were required in such an instance, that all homœopathic globules act alike,—that any one may replace any other in their list; and that it is the belief with which the drug is taken, and not the infinitesimal drug itself, which produces the effect ascribed to it. Some eight or ten years ago, an old schoolmate of Dr Simpson having begun business as a homœopathic druggist in Liverpool, kindly sent Dr S. a present of a small box of homœopathic medicines; and a very beautiful painted box it was. During the time it was in Dr S.'s possession, he put it only to one use, viz., he gave it as an occasional plaything to his eldest son, who was then a child. The boy,

revelling in his permitted amount of mischief, used in his sport to uncork the small bottles, empty their globules into a heap, and then refill the bottles from the general mass. Of course, this had speedily the effect of altering and disarranging the contents of the entire lilliputian drug-shop; the globules pertaining to the different bottles were more or less thoroughly mixed together; and sometimes when the child was tired of his occupation, others at last refilled the bottles from the general heap. A professional brother happening to call at Dr S.'s house one day when Dr S. was absent from home, saw the box, and put it in his pocket. Many weeks afterwards, the new proprietor of the box met Dr S., and told him that he had been trying to practise homœopathically, at which Dr S. expressed his regret; and he added, that he had seen some wonderful effects and cures from using the drugs contained in Dr S.'s own former homœopathic box! Wrongly perhaps, as Dr S. now thinks, he did not at the time, tell this physician that the globules of the bottles which he had been using were elaborately commixed; but the whole struck him as so good a joke at the moment, that he thought he would reserve it to bring it out upon his friend on some future and more ripe occasion, for the purpose of laughing him out of his homœopathic delusion. But, unfortunately, matters hastened rapidly on, the physician became more and more a homœopathist, and then it became too serious a matter to joke about, when he actually published a list of supposed homœopathic cures. The whole thing assumed so grave an aspect, that he never mentioned it, until the physician, who had appropriated the box, had become far too hardened in his homœopathic practices to allow of any hope of his re-conversion. From the time that the physician in question mentioned the results of using the box, till within the last few months, he and Dr S. never spoke on the subject of either the box or homœopathy. But the conversation turning on this last occasion upon the box, he remarked to Dr S., that Dr S. had no right to say, as he had heard reported, that it was the use of Dr S.'s commixed box of medicines that had converted him to homœopathy; because, as he added, he had another box of homœopathic drugs at the time he began, and Dr S. had no power of knowing that it was not this other box that he had employed. Dr S., of course, confessed that he had no means of knowing that it was Dr S.'s former box which he had employed, excepting this, that previously he had distinctly and deliberately told Dr S. that it was Dr S.'s old box that he had used, and the results of which had effected the change in him at the time of the first conversation. And Dr S. felt sure of one other circumstance, namely, that if that physician, or any other homœopathist, would use homœopathic boxes equally commixed as that which Dr S. once possessed, they would get results just as good as they would obtain from the supposed non-mixed globules. There is a homœopathic hospital now in London. If the physicians of it will take twenty or thirty boxes of homœopathic drugs, and allow any one to commix the contents of a few of them, and to furnish others with nothing but sugar and milk globules, they would find it, he believed, utterly and totally impossible, after trials of weeks or months, to point out the medicated from the non-medicated boxes, and those whose contents were thoroughly mixed, from those whose contents were all in due form and order. And he felt assured that the Society and the medical profession would join him in his sincere conviction and belief, that such an experiment was not in the least degree objectionable, in as far as the good and safety of the patients of the homœopathic hospital were concerned. For whether the drugged or undrugged globules were exhibited, one and the same result would follow,—viz., that the diseases of the patients would, under either mode of management, be allowed to go on uncontrolled and uninfluenced to their own natural termination. Fortunately for such an inane and negative practice as homœopathy, most diseases, even acute inflammation of the lungs, &c., did, in a large proportion of cases, tend ultimately towards a successful and happy termination, through the spontaneous and unaided efforts of nature alone; and the swallowing of a few infinitesimal globules of any kind would not alter in any degree this result. But Dr S. feared that if he spoke longer he might fall into the error spoken of by Mr Syme,—viz.,

the error of discussing the merits, or rather demerits, of homœopathy; while the Society were met, not for this purpose, but for the purpose of expressing their judgment in regard merely to the propriety of practitioners of homœopathy withdrawing from the Society. There was another class of practitioners who ought, he thought, certainly to be included in the category of those whose names should be removed from the list of members. He alluded to such practitioners as were not simple-minded, but sincere homœopathists. He alluded to those who pretended to be homœopathists, but acted as allopathists,—who doctored people according as people themselves wished, either with drachms of drugs, or billions of a grain of the same; who wished to be considered homœopathists, but, as had been often detected, drugged their globules and tinctures with active doses of the most powerful medicines; who spread out the snare of homœopathy as a golden man-trap to catch credulous and trusting patients, and afterwards either openly or surreptitiously applied to them all the usual means employed in the armentarium of rational medicine. Some men pretended they could honestly and honourably mix up the two practices. Most physicians naturally doubted whe ther any man could in honour and honesty combine such incompatible incongruities. Neither any true homœopath nor any true allopath would give this spurious set credit for their integrity of purpose and principle. If any such were members of the Society, let them certainly and undoubtedly be requested to retire. Their conduct was the conduct of the poor Swiss clergyman, who, living in a district partly of Romanists and partly of Protestants, thought it no discredit to eke out his living by preaching a strong Romanist sermon to the Romanists in the forenoon, and a strong Protestant sermon to the Protestants in the afternoon. He repeated, that he hoped and understood that Mr Syme's motion included this hybrid and equivocal class of practitioners.

Mr Syme begged to ask who the practitioner was that had been converted to homœopathy, by watching the effects of the globules contained in Dr Simpson's mixed homœopathic drug-box; and he hoped, for the sake of the profession in Edinburgh, Dr S. would have no objections to mentioning the practitioner's name.

Dr Simpson said that the practitioner alluded to was Professor Henderson.

The motion was carried unanimously.

Letters from Dr John Rutherford Russell and Dr James Russell, resigning their seats as members of the Society, were then read by the Secretary. It was unanimously agreed to accept these resignations.

Dr Christison then said, that he greatly regretted that a sense of duty had obliged him to take the unpopular step of moving the exclusion of strangers from the previous discussion. He now begged to move, that the secretaries be empowered to draw up an account of the evening's proceedings, and to publish it through the usual channel.

This proposal was unanimously agreed to.

MEDICAL RELIEF OF THE POOR IN SCOTLAND.

It appears, from the report of the commissioners appointed to inquire into the administration and practical operation of the poor laws in Scotland, that previous to the year 1844 there was scarcely any provision made for *medical* relief to the poor out of the *poor funds in this country*. In a large majority of parishes, these paupers were gratuitously attended and supplied with medicines by resident medical gentlemen, occasionally aided by charitable neighbours.

By a statute which came into operation in August 1845, parochial boards were "required, out of the funds raised for the relief of the poor, to provide for medicines, medical attendance, nutritious diet, cordials, and clothing for such poor, in such manner, and to such an extent, as may seem equitable and expedient."

In 1846, the Board of Supervision directed the attention of parochial authorities to this subject, pointing out the small amount charged for medical relief in the whole of Scotland during the previous year. The consequence was, that next

year showed an expenditure of L.12,879, 9s. 6½d., being more than thrice the sum expended in 1846, for the medical relief of paupers in a country numbering 2,620,216 inhabitants.

Still there was reason to fear a great inadequacy of this species of relief, and a government grant of L.10,000 was made for the purpose of stimulating and aiding, without superseding, parochial exertions in this direction.

The plan hitherto followed in the distribution of this sum among the different parochial boards, was suggested by a report of the Board of Supervision, in Feb. 1848. Its principles are briefly these—No sum from the grant is allocated to any parish, unless its previous year's expenditure on medical relief of its paupers bears a certain ratio to the density and actual amount of its population. Each parish which expends a proper sum, is reimbursed to the extent of one half thereof out of the grant; and the surplus, derived from the non-co-operation of certain parochial boards in this scheme, is thereafter divided in due proportion among such boards as have by their own exertions established a right to such participation.

As might be expected, the above plan has its opponents, and the parochial board of Old Machar, in particular, has been raising a little agitation, and memorialising the Lords Commissioners of the Treasury against it.

The subject has been deemed of sufficient importance to engage the attention of the Edinburgh Royal Colleges of Physicians and Surgeons; and as a departure from the excellent plan recommended by the Board of Supervision, would in their opinion tend to frustrate the objects contemplated by government in making the grant, viz., the adequate medical care of the poor, the fair remuneration of the parochial surgeon, and the stimulation of parochial exertions, the Colleges have addressed separate memorials to the Treasury, in which they strongly recommend a persistence in the present mode of allocating the grant.

PUBLICATIONS RECEIVED.

Recherches sur les Principes Immédiats qui composent le Sang de l'Homme et des Principaux Mammifères. Par MM. F. Verdiel et W. Marcet. (Extrait du Journal de Pharmacie et de Chimie, t. xx., Août 1851.) Paris.

On the Nature and Treatment of Softening of the Brain. By Richard Rowland, M.D. London: Highley and Son. 1851.

On the Transmission from Parent to Offspring of Some Forms of Disease. By James Whitehead, M.D. London: Churchill. 1851.

The American Journal of Dental Science. Edited by C. A. Harris, M.D., and Alfred A. Blandy, M.D. Three Numbers of New Series 1851. Philadelphia: Lindsay and Blakeston.

Report of the Proceedings of the Pathological Society of London. Fifth Session, 1850-51. London. 1851.

Die Quarantäne-Reform und die Pestfrage Beobachtungen und Antrage von Karl Ludwig Sigmund, M.D., u. s. w. Wien. 1850.

Supplement to Principles of Surgery. By James Syme, F.R.S.E., &c. Edinburgh: Sutherland and Knox. 1851.

Lectures on the Physical Diagnosis of the Diseases of the Lungs and Heart. By Herbert Davies, M.D., &c. London: Churchill. 1851.

Manual of Physiology. By W. B. Carpenter, M.D., &c. London: Churchill. 1851. Second Edition.

Lectures on the Principles and Practice of Surgery. By Bransby B. Cooper, F.R.S. London: Churchill. 1851.

Charleston Medical Journal and Review. September 1851.

The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias. By Henry Beasley. Fifth Edition. London: Churchill. 1851.

Transactions of the Medical Society of the State of Pennsylvania at its Annual Session, held in the City of Philadelphia, May 1851. Vol. I. Philadelphia: 1851.

The Stethoscope and Virginia Medical Gazette. Vol. I. No. X. October, 1851. [We are obliged to the Editor for several other Numbers which have been formerly received. We must, however, decline the *exchange* as unequal.]

On the Morbid Conditions of the Bile and Gall-Bladder. By Edward Crisp, M.D., Physician to the Metropolitan Dispensary. London: Teape and Son. 1851. Reprinted from the London Medical Examiner.

Bradshaw's Companion to the Continent, with Observations on the Influence of Climate and Travelling. By Edwin Lee, Esq. London. 1851.

INDEX.

Acton on diseases of the urinary and generative organs, reviewed, 254.
Address, inaugural, by Professor Christison, 292; at opening of extra-academical school, by Dr Douglas Maclagan, 567.
Aethyle, Dr Strecker on nitrate of oxide of, 283.
Ague, treatment of, with persesquinitrate of iron, 336.
Albuminuria and puerperal eclampsia related, 386.
Alliers on percussion and auscultation, reviewed, 147.
Amaurosis and granular kidneys, 575.
Aneurism of aorta, Dr W. T. Gairdner's cases of, 137, 184; Mr Syme on treatment of popliteal, 444.
Animal heat, MM. Dumeril, Demarquay, and Lecointe on, 177.
Ankle-joint, Professor Syme on amputation at the, 55.
Aorta, aneurisms of, 137, 184.
Apoplexy, saturnine, and detection of lead in the brain, 65.
Arneth, Dr, on puerperal fever, 70; on the obstetric practice of the Vienna Lying-in Hospital, 84.
Arsenical poisoning, Dr Traill's case of, 1; M. Barse on, 483.
Artemisia vulgaris in chorea, 480.
Artificial nose, Dr Roberts on, 182; Dr Mackenzie on, 348.
Ascites cured by iodurretted injections, 373.
Australian females, on procreative power of, 353.
Ayer, Dr, on congenital variola in twins, 277.

Ballingall, Dr G. R., on strangulated inguinal hernia, 21.
Barse, M., on arsenical poisoning 483.
Baryta, poisoning with muriate of, 67.
Bell, Dr Charles, and Mr George Hamilton, on the use of muriated tincture of iron in erysipelas, 69; Mr B. on scarlet fever in Watson's Hospital, 105.
Bennett, Professor, on leucocythemia, or white cell-blood, 97, 317; on colchicum in the delirium and coma of scarlatina, 158; on bleeding in pneumonia, 159; on difficulties in the diagnosis of pneumonia, 161; on treatment of pulmonary phthisis, 163; clinical lectures by, 158, 264, 462, 546; on carbonaceous lungs, 264; on the treatment of laryngitis by topical applications, 462; report of cases of bronchitis, 548, 549, 550,—of pleuritis, 552,—of pneumonia, 556, 557.
Bernard, M., on the production of sugar in the liver of man and animals, 270.
Bickersteth's, Mr E. R., reports of clinical surgical wards, 469, 559.
Bilifulvine, Virchow on, 474.
Birth, after mother's death, 64; of five children at once, 281.
Blood, on the chemical condition of the, in various diseases, 269; Zimmermann's mode of analysing, 366; successfully transfused, 447.
Blood-corpuscles, Dr Sanderson on the metamorphosis of coloured, 216, 521.
Blood-letting in pneumonia, Prof. Bennett on, 159.
Bookless, Mr J., case of uterine hydatids, &c., 24.
Books received, 95, 200, 304, 400, 496, 590.
Bouvier and Chatin, MM., on the detection of lead in the brain and liver after saturnine poisoning, 65.
Brain, lead detected in the, 65; Donders' experiments on the, 166.
Bramwell, Dr, on cerebellar tumour, 442.
Breed, Dr, on phosphoric acid in urine, 368.
Bronchitis, Dr W. T. Gairdner on the morbid anatomy of, 2, 238.
Buren, Dr Van, on an ovarian cyst, 390.
Bussemaker's Oribasius reviewed, 360.

Cabot, Dr, on keloides occurring in a child, 378.
Cancer of the lung, Dr H. Douglas on, 180; of the liver of a fowl, 182; of stomach, 183.
Cannabis indica, Dr A. Christison on, 26, 117.
Capillary tubes for vaccination, Dr Husband on, 229.
Carpenter's principles of physiology, reviewed, 449.
Carrière's climat de l'Italie, reviewed, 537.
Cartilages, on the healing of wounds in articular, by Dr Redfern, 201.

Cazenave, M., on the compatibility of certain skin-diseases with healthy lactation, 63.
Cerebellar tumour, Dr Levin's case of, 61; Dr Bramwell's case of, 442.
Chailly-Honoré, case in which chloroform was objected to by the patient, 389.
Chatin and Bouvier, MM., detection of lead in the brain and liver, by, 65.
Chloroform, curious objection to the use of, 389.
Cholera, treatment of, by tannin, 62.
Chorea cured by mugwort, 480.
Christian, Dr, on extra-uterine pregnancy, 387.
Christison, Dr A., on Indian hemp, 26, 117.
Christison's, Professor, inaugural address, 292; on the present state of medical evidence, 401.
Churchill, Dr Fleetwood, on ovarian irritation, 390.
Clinical medicine, Professor Bennett's lectures on, 158, 264, 462, 546.
Clinical microscope, Dr W. T. Gairdner on a, 187.
Clinical surgery, Professor Syme's lecture on, 53; Mr Bickersteth's reports on, 469, 559.
Cœcum, Dr Storer on ulceration of the, 378.
Colchicum, in scarlatina, Professor Bennett on the use of, 158; autumnale, Dr J. M. Maclagan on, 501.
Colon, obstruction of, relieved by operation, by Mr Luke, 179.
Constantinople, treatment of the insane in, 399.
Convulsions, puerperal, and albuminuria, 386.
Cooper's lectures on surgery, reviewed, 541.
Cormack, Dr, on scarlatinal vaginitis, 280.
Corpus luteum, Dr A. Harvey on, 305.
Corrosive sublimate, case of poisoning with, 533.
Costes, M., on iodurctted injections for the cure of ascites and hydrarthosis, 373.
Cozzi, Professor, on the chemical condition of the blood in various diseases, 269.
Crawford, Mr, ligature of left subclavian, by, 275.
Crawford, Dr, on supernumerary toes and fingers, 356.
Cyst, ovarian, case of, by Mr Van Buren, 390.

Daremberg's Oribasius reviewed, 360.
Deaf and dumb, on the offspring of the, 580.
Debaugne, M., solution of iodine in water, by, 282.
Debreuilh, M., on pregnancy, nursing, and phthisis, 579.
Decapitation, experiments on bodies after, 172.
Demarquay, M., on animal heat, 177.
Depaul, M., on puerperal convulsions, 386.
Desgranges, M., on treatment of prolapsus uteri, 388.
Dickson's, Mr, case of fœtal monstrosity, 82.
Donders' experiments on the brain, 166.
Douglas, Dr H., on encephaloid disease of the lung, 180.
Duclos', M., case of anchylosis of elbow-joint, 273.
Dumeril, M., on the effects of various therapeutical agents on animal heat, 177.
Duval's experiments on bodies of decapitated criminals, 172.

Easton, Dr J. A., contributions to legal medicine by, 430.
Eclampsia, puerperal, and albuminuria, 386.
Eczema treated with empyreumatic oil, 381.
Elbow-joint, Professor Syme on excision of the, 54; incomplete anchylosis of the, 273.
Emphysema pulmorum, Dr W. T. Gairdner on, 68.
Empyreumatic coal oil used in eczema, 381.
Encephaloid disease of the lung, Dr H. Douglas on, 180.
Epilepsy, Drs Everts and Van Leeuwen on, 574.
Erysipelas, Dr Charles Bell and Mr George Hamilton Bell on the treatment of, by means of the tincture of the muriate of iron, 69.
Evans's, Dr, obstetrical extractor, 64.
Eversant, M., on hernia of the ovary mistaken for an encysted tumour, and removal with fatal result, 276.
Everts, Dr, on epilepsy, 574.
Evidence, medical, Dr Christison's lecture on, 401.
Exchange list, 96.
Excision of elbow-joint, Prof. Syme on, 54.
Extra-uterine pregnancy, 387.
Extractor, Dr John Evans's obstetrical, 64.
Extracts, M. Martin's mode of preserving, 483.
Eyeball, Dr Hays on prominent, 575.

Fever, Dr Arneth on puerperal, 70; Dr Moir on puerperal, 70; Dr Simpson on ditto, 72; Dr Peddie on ditto, 80; Mr B. Bell on scarlet, in Watson's Hospital in 1851, 105; Dr Retzius on the power of quinine as a preventive of puerperal, 279; oxalate of potash in puerperal, 379.
Fingers, Dr Crawford on supernumerary, 356.
Fistula, vesico-vaginal, cured by operation, 387.

Gairdner, Dr W. T., on the pathology of bronchitis, 2, 238; on emphysema pulmorum, 68; on aortic aneurism, 137, 184; on cancer of the liver of a fowl, 182; on urinary deposit of urate of soda, 182; on a clinical microscope, 187.
Galloway's first step in chemistry, reviewed, 264.

Glanders, Dr F. W. Mackenzie's case of, 375; M. Graslepois' case of, 375.
Godfrey, Mrs, on spinal curvature, reviewed, 151.
Graduates, medical, at Edinburgh, in 1851, 298.
Græffe's treatment of cholera by tannin, 62.
Graslepois, M., case of glanders by, 375.

Hæmatoidine, Virchow on, 474.
Hannover, Dr, on menstruation, 579.
Hartt, Dr, on rupture of the uterus occurring twice in the same subject, 276.
Harvey, Dr A., on true corpora lutea, 305.
Hayne on hydrophobia, 573.
Hays, Dr, on amaurosis and protrusion of the eyeball, 575.
Hayward, Dr, on vesico-vaginal fistula, 387.
Head, Dr J. Traill's case of injury of the, 446.
Heat, MM. Dumeril, Demarquay, and Lecointe, on, 177.
Hemorrhage accompanying the expulsion of uterine hydatids interspersed with moles, by Mr J. Bookless, 24.
Hemorrhœa petechialis, Dr Imlach's case of, 135, 182.
Hemp, Dr A. Christison on Indian, 26, 117.
Hernia, Dr G. R. Ballingall's case of strangulated, 21; Mr Obre's case of obturator, 178; of ovary, 276.
Hippocrates, translation of, by Littré, reviewed, 360.
Homœopathic candidates, 197.
Homœopathy, 197, 299, 300, 494, 581.
Huc's travels in Tartary, Thibet, and China, reviewed, 45.
Huguier, M., on retro-uterine sanguineous tumours, 278.
Husband, Dr, on the use of capillary tubes in vaccination, 229.
Hydatids of the uterus, Mr Bookless's case, 24; of the liver evacuated externally, 573.
Hydrarthosis cured by iodureted injections, 373.
Hydrophobia, Professor Hayne on, 573.
Hysteria, Dr Douglas Maclagan's case of, 121; and paralysis, 370.

Iceland, Dr Schleisner on its sanitary condition, 575.
Imlach, Dr, on hemorrhœa petechialis, 135, 182.
Inaugural address, delivered 1st August 1851, by Professor Christison, 292.
Indian hemp, Dr A. Christison on, 26, 117.
Infirmary, clerkships in the Royal, 303.
Inversion of the uterus, 277, 497; of vagina, 278.
Iodine rendered soluble in water, by Debaugne, 282; injected in ascites and hydrarthosis, 373.
Iron, poisoning by sulphate of, 65; tincture of the muriate of, in erysipelas, 69; persesquinitrate of, in ague, 336.

Jenner, Dr, on the treatment of sarcina-ventriculi by the neutral sulphites, 382.

Keloides in a child, Dr Cabot's case of, 378.
Kerr, Mr W., on treatment of ague with persesquinitrate of iron, 336.
Kidney, granular, connected with amaurosis, 575.
Kinesipathy, reviewed, 257, 458.
Kiwisch, Dr, case of ovarian pregnancy by, 478.

Labus, Dr, on pellagra, reviewed, 452.
Lactation, compatibility of healthy, with certain cutaneous diseases, 63.
Lafont-Gouzi, M., on treatment of eczema with empyreumatic coal oil, 381.
Lafforgue on hydatids of the liver, 573.
Lambert, Dr, on inversion of the vagina during labour, 278.
Lane's Materia Medica reviewed, 534.
Lardner's handbook of natural philosophy and astronomy, &c., reviewed, 157.
Laronde, M., on stercoraceous accumulation mistaken for pregnancy, 64.
Laryngitis, Professor Bennett on the application of nitrate of silver for the cure of, 462.
Law and quackery, 398.
Lead detected in the brain after saturnine apoplexy, 65.
Lebert's practical treatise on cancerous diseases, reviewed, 50.
Lecointe, M., on animal heat, 177.
Lectures on clinical surgery, 53; on clinical medicine, 158, 264, 462, 546; at Surgeons' Hall, 567.
Lee on Climate and Travelling reviewed, 537.
Legal medicine, Dr Christison on the present state of, 401; Dr J. A. Easton's contributions to, 430.
Leucocythemia, or white cell-blood, Prof. Bennett on, 97, 317.
Levin's, Dr, case of cerebellar tumour, 61.
Limousin-Lamothe, M., on poisoning by sulphate of iron, 65.
Lip and nose, Dr Mackenzie on restoration of, 348.
Lithiasis in Russia, 274.
Lithotomy, Professor Syme on, 55, 469; in Russia, 274.
Littré's Hippocrates, reviewed, 360.
Liver, lead detected in the, after saturnine poisoning, 65; cancer of a fowl's, 182; production of sugar in the, 270; hydatids of the, 573.
Lizars, Mr J., and the "Monthly Journal," 301.
London Medical Gazette, 198.
Luke, Mr, case of obstructed colon relieved by an operation by, 179.
Lungs, Dr W. T. Gairdner on lesions of the, connected with bronchitis, 2, 238; on emphysema of the, 68; Dr Douglas on encephaloid disease of, 180; Prof. Bennett on carbonaceous, 264.

Mackenzie, Dr, on asphyxia caused by impaction of a foreign body in the pharynx, 68; on restoration of upper lip and nose, 348.
Mackenzie, Dr F. W., on a case of glanders, 375.
Maclagan, Dr Douglas, on a case of hysteria, with albumen and xanthic oxide in the urine, 121; opening address at Surgeons' Hall, 567; Dr J. M., on Colchicum, 501.
Martin, M. Stanislas, on a mode of preserving vegetable extracts, 483.
Matrasses, Mohr's mode of coating, 281.
Medical graduates in the University of Edinburgh, on 1st August 1851, 298; Faculty of the University of Edinburgh, resolutions relative to the chair of general pathology by the, 299; directory for Scotland, 304, 496; evidence, Professor Christison on, 401; relief of the poor in Scotland, 589.
Medicine, clinical lectures on, by Dr Bennett, 158, 264, 462, 546; legal, Dr Easton's contributions to, 430.
Medico-Chirurgical Society of Edinburgh, the transactions of the, 68, 180.
Menstruation, Dr Hannover on, 579.
Microscope, Dr W. T. Gairdner on a clinical, 187.
Miller, Professor, on aortic aneurism, 185; Dr H. Miller's case of inversio uteri, 497.
Mohr, M., on a mode of coating retorts, &c., 281.
Moir, Dr, on puerperal fever, 70; on the induction of premature labour, 88.
Moles, Mr Bookless's case of, 24.
Monstrosity, Mr Dickson's case of fœtal, 82.
Montgomery, Dr, on a peculiar form of thrombus, 474.
Mormonier's, Dr, successful case of transfusion, 477.
Mugwort in chorea, 480.
Muriate of baryta, poisoning with, 67.

Neumeister, Dr, on the cure of chorea with mugwort and assafœtida, 480.
Nevins' London Pharmacopœia, reviewed, 534.
Nose, Dr Roberts on artificial, 182; Dr Mackenzie on restoration of the, 348.
Nursing and phthisis, 579.

Obre, Mr, case of thyroidal hernia remedied by operation, 178.
Obstetrical extractor, Dr John Evans's, 64; Society's transactions, 81, 492.
Oribasius, Daremberg and Bussemaker's, reviewed, 360.
Ovarian pregnancy, 478.
Ovary, hernia of the, 276; dropsy of the, cured by operation, 279; cyst of the, 390; irritation of the, 390; influence on menstruation of the, 579.
Oxalate of potash in puerperal metritis, 379.
Paralysis and hysteria, 370.
Peaslee, Dr, on cure of double ovarian dropsy by operation, 279.
Peddie, Dr, on puerperal fever, 80; on chloride of zinc in ulceration of the os and cervix uteri, 81.
Pellagra, Labus on, reviewed, 452.
Pericarditis, Dr J. Taylor on some causes of, 56.
Persesquinitrate of iron, a cure for ague, 336.
Petit's experiments on decapitated criminals, 172.
Pharynx, Dr Mackenzie on impaction of a foreign body in the, 68.
Phillips' London Pharmacopœia reviewed, 534.
Phosphoric acid in urine, Dr Breed on, 368.
Phthisis pulmonalis, Professor Bennett on the treatment of, 163; its connection with nursing and pregnancy, 578.
Physiological Society, transactions of the, 90, 187, 289, 391, 485.
Pneumonia, on bloodletting in, by Professor Bennett, 159; on difficulties in the diagnosis of, by do., 161.
Poisoning by Scheele's green, Professor Traill's case of, 1; by lead, 65; by sulphate of iron, 65; by muriate of baryta, 67; with strychnia, 234; arsenical, 483; with corrosive sublimate, 532.
Pregnancy, simulated by stercoraceous accumulation, 64; extra-uterine, 387; ovarian, 478; and phthisis, 579.
Premature labour, on the induction of, by Dr Arneth, Dr Moir, and Dr Simpson, 87, *et seq.*
Prolapsus uteri, M. Desgranges' treatment of, 388.
Provincial Medical and Surgical Association, resolutions of the, 300.
Publications received, 95, 200, 304, 400, 496, 590.
Publisher, change of our London, 302.
Puerperal fever, Dr Arneth on, 70; Dr Moir on, 70; Dr Simpson on, 72; Dr Peddie on, 80; Dr Retzius on quinine as a preventive of, 279; on oxalate of potash in, 379; convulsions and albuminuria, 386.
Purpura hemorrhagica, Dr Imlach's case of, 135, 182.

Quinine recommended by Dr Retzius as a preventive of puerperal fever, 279.

Redfern, Dr, on the healing of wounds in articular cartilages, 201.
Reports of cases in surgical clinical wards, 469.
Retorts, Mohr's mode of coating, 281.
Retro-uterine tumours, M. Huguier on sanguineous, 278.
Retzius, Dr, on quinine as a preventive of puerperal fever, 279.
Reviews—Huc's Travels in Tartary, Thibet, and China, 45; Lebert's Practical Treatise on Cancerous Diseases, 50; Skoda

on Percussion and Auscultation, 147; Albers on Auscultation, 147; Walshe on Diseases of the Chest, 147; Godfrey on Spinal Curvature, 151; Squire's London Pharmacopœia, 153; Wilson's Portraits of Diseases of the Skin, 157; Lardner's Handbook of Natural Philosophy and Astronomy, 157; Acton on Diseases of the Urinary and Generative Organs, 254; Kinesipathy, 257; Galloway's First Step in Chemistry, 264; Smith on Enlargement of the Spleen, 357; Littré, Daremberg, and Bussemaker's French editions of Hippocrates and Oribasius, 360; Carpenter's Principles of Physiology, 449; Labus on Pellagra, 452; Roth's Exposition of Kinesipathy, 458; Translations of the new London Pharmacopœia, by Richard Phillips and J. Birkbeck Nevins, M.D., 534; Lane's Materia Medica, &c., 534; Carrière's Climat de l'Italie, 537: Lee on Climate, &c., 537; Sigmund on Quarantine Reform, 539; Bransby Cooper's Lectures on Surgery, 541; Syme's Principles of Surgery—Supplement, 546.

Roberts, Dr, on artificial nose, 182.

Rochard's experiments on decapitated criminals, 172.

Roth's Exposition of Kinesipathy, reviewed, 458.

Rupture of the uterus, 276.

Russia, frequency of lithiasis and lithotomy in, 274.

Sanders', Dr W. R., abstract of Bernard's observations on the production of sugar in the liver, 270.

Sanderson, Dr, on the metamorphosis of coloured blood-corpuscles, 216, 521.

Sandras, Dr, on hysterical paralysis, 370.

Sanguineous tumours, M. Huguier on retro-uterine, 278.

Sarcina-ventriculi, Dr Jenner on the cure of, 382.

Scarlatina in George Watson's Hospital, Mr B. Bell on, 105; Professor Bennett on use of colchicum in, 158.

Scarlatinal vaginitis, Dr Cormack on, 280.

Scheele's green, Professor Traill on a case of poisoning with, 1.

Schleisner, Dr, on the sanitary condition of Iceland, 575.

Schneider's, Dr, case of birth after the mother's death, 64.

Serlo, Dr, on birth of five living children at once, 281.

Sigmund, Professor, on quarantine, reviewed, 539.

Simpson, Professor, on puerperal fever, 72; on the use of chloride of zinc in uterine ulcerations, 82; on the induction of premature labour, 88.

Skin diseases compatible with healthy lactation, 63.

Skoda on auscultation and percussion, reviewed, 147.

Small-pox occurring after a third vaccination, 310.

Smith, Dr A., on a case of poisoning by strychnia, 234; Dr E., on inversion of the uterus, 277; Dr G., on enlargement of the spleen, reviewed, 357.

Spleen, on enlargement of the, 357.

Squire's London Pharmacopœia, reviewed, 153.

Stercoraceous accumulation simulating pregnancy, 64.

Stewart, Dr A. P., on confluent small-pox after a third vaccination, 310.

Stomach, Dr Wilson on malignant disease of, 183.

Storer, Dr, on ulceration of the cœcum, 378.

Strecker, Dr, on nitrate of oxide of aethyle, 283.

Stricture of the urethra, treatment of, by external incision, 19, 54, 146, 235.

Strychnia, Dr Smith's case of poisoning with, 234.

Subclavian, ligature of left, by Mr Crawford, 275.

Sugar in the liver of man and animals, Professor Bernard's observations on the discovery of, 270.

Sulphate of iron, poisoning with, 65.

Sulphites in the treatment of sarcina-ventriculi, 382.

Surgeons' Hall, opening address delivered at, 567.

Surgery, lectures on clinical, by Professor Syme, 53; Reports by Mr Bickersteth, 469.

Syme, Professor, on treatment of stricture by external incision, 19, 54, 146, 235; on excision of the elbow-joint, 54; on amputation at the ankle-joint, 55; on lithotomy, 55; clinical observations by, 53; on the treatment of popliteal aneurism, 444; supplement to Principles of Surgery, 546.

Tannin in cholera, recommended by Græffe, 62.

Taylor, Dr J., on some causes of pericarditis, 56.

Teas of commerce, on, 284.

Thomson, Dr T. R. H., on the reported incompetency of the aboriginal females of New Holland to procreate with native males after having borne children to a European or white, 353; Dr F., case of poisoning with corrosive sublimate, 532.

Thrombus, Dr Montgomery on a peculiar form of, 474.

Toes, Dr R. Crawford on supernumerary, 356.

Toynbee, Mr, on the membrana tympani, 473.

Traill, Professor, on a case of poisoning with Scheele's green, 1; Dr J., on injury of the head, 446.

Transactions of the Edinburgh Medico-Chirurgical Society, 68, 180, 581; of the Edinburgh Obstetrical Society, 81; of the Edinburgh Physiological Society, 90, 187, 289, 391, 485.

Transfusion of blood successful in a case of post-partum hemorrhage, 477.

Tumour of cerebellum, Dr Levin's case of, 61; Dr Bramwell's case of, 442.

Urethra, stricture of the, treated by external incision, 19, 54, 146, 235.
Urine, xanthic oxide observed in, by Dr Douglas Maclagan, 121; urate of soda in, observed by Dr W. T. Gairdner, 182; Dr Breed on phosphoric acid in, 368.
Uterine hydatids and moles, Mr Bookless's case of, 24; ulcerations treated by chloride of zinc, 81.
Uterus, rupture of the, 276; inversion of the, 277, 497; prolapsus of the, 388.

Vaccination, Dr Husband on the use of capillary tubes in, 229; remarks on, by Dr A. P. Stewart, 310.
Vagina, inversion of the, 278.
Vaginitis, Dr Cormack on scarlatinal, 280.
Van Leeuwen on epilepsy, 574.
Varieties, 93, 193, 494.
Variola, congenital, occurring in twins, 277; after a third vaccination, 310.
Vesico-vaginal fistula, Dr Hayward on, 387.
Vienna Lying-in Hospital, Dr Arneth on the practice of the, 84.
Virchow on hæmatoidine and bilifulvine, 474.

Walshe, Dr, on Chest Diseases, reviewed, 147.
Warrington, Mr, on the teas of commerce, 284.
Watson's Hospital, Mr B. Bell on scarlet fever in, 105.
Welti on oxalate of potash in puerperal fever, 379.
Wilson, Dr, case of malignant disease of stomach, 183.
Wilson's Portraits of Diseases of the Skin, reviewed, 157.
Wolf, Dr, on poisoning with muriate of baryta, 67.

Xanthic oxide in urine, Dr Douglas Maclagan's case of, 121.

Zimmermann, G., on a mode of analysing the blood, 366.
Zinc, chloride of, in uterine ulcerations, 81.

END OF VOL. XIII.

WELLCOME LIBRARY INSTITUTE

MURRAY AND GIBB, PRINTERS, EDINBURGH.